Geological Survey of Canada
Open File 1945

The Heavens Above and the Earth Beneath

A History of the Dominion Observatories

Part 2
1946-1970

John H. Hodgson

1994

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Cover

"The Heavens Above and the Earth Beneath" which is appropriate to the multiple mandate of the Dominion Observatories, is taken from the Second Commandment, Exodus 20, 4.

CORRECTIONS AND ADDITIONS TO PART 1

Page ii – The Title "The Heavens Above and the Earth Beneath" is taken from the Second Commandment, not the First. The reference to Exodus 20, 4 is correct.

Page 1 – Fifth paragraph. Mr. DeLaunais' name is Richard, not Charles.

Page 8 – LH Column, Line 1. The underlined section should read:

He resisted suggestions that he should move to Ottawa. "My family is better off here and enjoys comforts that in a city are denied with the same income. My income in Ottawa will be necessarily less, for here many an odd dollar I earn beside my Government pay"12.

Page 59 – Right column, fourth paragraph. The dam across the Chaudiere Falls was built in 1908, not 1900.

Page 146 – Top photograph. K.O. Wright has identified the man with McKellar as Tom Hutcheson.

Page 163 – M.M. Thomson has made some additional identifications: 2 C.R. Westland; 12 J.E.R. Ross; 13 F.J. Dunn; 16 "Chris" Christensen; 19 Bill Hughson(?).

Back Cover – When this photograph was published the unidentified figure, third from the left in the front row, was thought to be R.K. Young. Dr. Peter Millman pointed out to me that the blackboard carries a number of Chinese characters. Klotz reports that a Dr. H.E. Johns, an Englishman working at "Union College, China", spent several weeks at the Observatory in 1918. The unidentified figure is probably Johns, and the occasion a seminar in which he spoke of his work in China.

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THE HEAVENS ABOVE AND THE EARTH BENEATH
A HISTORY OF THE DOMINION OBSERVATORIES

Part 2
1946-1970

PREFACE

This volume continues the history of the Dominion Observatories from the end of the Second World War to 1970, at which time responsibility for astronomy was taken over by the National Research Council and that for geophysics by the Earth Physics Branch of the Department of Energy, Mines and Resources.

The format has been changed somewhat from that of Part 1. There a chapter was devoted to each Administration, beginning with a general history of the period and followed by descriptions of the work in the various scientific disciplines. This simple arrangement has not proven satisfactory for Part 2, which covers only two Administrations, a long one by Beals, a short one by Hodgson. The change of Directors had little effect on the scientific work, and to divide it into two sections would be pointless.The history of the two Administrations is treated in two initial chapters, and these are followed by a number of chapters dealing with the scientific work on a discipline basis. For the sake of continuity, chapter numbers are continued from Part 1.

At the beginning of Hodgson's directorship the Government approved the construction of a 150-inch telescope, to be named after Queen Elizabeth II. This apparently happy decision led, in five short years, to tremendous dissension within the astronomy community, to the consequent cancellation of the telescope, and to the breaking up of the Observatories Branch. A final chapter is devoted to these traumatic events.

Many former colleagues, some retired, some still active, have read sections of this volume relating to their particular fields. Their names are too numerous to list, but if this volume has given a true picture of the scientific work of the Observatories it is because of their collaboration. Any errors or obscurities that remain are, of course, my responsibility.

I continue to be much indebted to Charles Levesque and Louise Simpson of the former Earth Physics Branch Library, who have been most helpful in tracing references. Richard DeLaunais has supplied photographs of the Ottawa work, Dr. Alan Batten those from the Dominion Astrophysical Observatory.

Mr. Lawrence Farrington and Ms. Thelma Nicholson have searched the Privy Council files and supplied important material relating to the cancellation of the Queen Elizabeth II telescope. I am most grateful to them.

I am most appreciative of the continued support of Dr. R.P. Riddihough, Chief Scientist of the Geological Survey of Canada, in bringing this history to publication.

Finally I express my gratitude to the staff of the National Archives. As a neophyte historical researcher I must sometimes have been a trial to them, but their response has been uniformly courteous, helpful and efficient.

John H. Hodgson

Dr. C.S. Beals, FRS, Director and Dominion Astronomer, 1946-1964.

V – THE BEALS ADMINISTRATION 1946-1964

As the time came for Stewart's retirement there was a good deal of speculation within the Observatory about who his successor would be, and, indeed, about a possible reorganization of the Branch. Since geophysics had become a major part of the Observatory program, many thought the new Director should be a geophysicist. Some people went so far as to suggest that astronomy didn't fit well into a Department of "Mines and Resources", and should be transferred to the National Research Council, leaving behind a purely geophysical organization. Some senior officials in the Department, and particularly in the Geological Survey, would have taken this a step further and absorbed the geophysics into the Survey. I don't know who made the decision to continue the Branch in its existing form and to reach out to Victoria to select an astronomer with impeccable credentials as its new head[1].

Neither do I know why Beals accepted the job. He had a secure position at a major observatory, and a world-wide reputation which was shortly to win him an FRS. He had essentially no experience in administration and knew nothing about geophysics. He and Mrs. Beals had an established social position and many dear friends in Victoria, and knew almost no one in Ottawa. He must have known that he would face some resentment in being parachuted into an organization that knew nothing about him.

Whoever made the decision to offer him the job, and for whatever reason he accepted it, we can with hindsight say that it was a wise decision for the Department and a happy one for him. He arrived in Ottawa in mid-November, 1946[2]. For the first several months he signed his letters "Acting Dominion Astronomer"; Stewart was on retirement leave until February 15, 1947[3] while DeLury, who succeeded briefly in an acting capacity, was on retirement leave until May 23, 1947.

He didn't get into his new office promptly; neither Stewart nor DeLury was in any hurry to vacate the premises and, in notes prepared for an after-dinner speech many years later[4], he recalls that he was given a desk vacated by the Assistant Director, "which had papers piled into a conical shape in the centre". He doesn't say so, but he might have noted that he looked out on a window-sill which bore the debris of thirty years of bird feeding. When he finally got into the Director's office he found the remains of three rugs on the floor, one over the other, with each rug showing through the worn spots of the one above, like a sequence of eroded geological strata. We have already noted in Part I, that he discovered, buried in the accumulation of books and papers, scientific manuscripts that had been awaiting publication approval for some twenty years. There is also a cryptic remark in the aforementioned notes: "chickens in the attic". I had heard this story before, that the caretaker kept chickens in a room under the dome, but I had always thought it a canard.

It was not just the Director's offices that cried out for attention; the entire building showed the results of many years of neglect. The library stacks had spread untidily into the halls. All the rooms needed redecoration. One particularly vivid memory is of the hall ceilings. They were completely hidden with festoons of dust-covered wires. J.P. Henderson was technically excellent, but he was not well organized. When time signals were required anywhere in the building, as for example in the seismograph vaults, he would run a twisted pair from the time room, along the hall ceilings to the required point. There was really no other way to do it since there were no built-in conduits in the walls, but Henderson never remembered which wire was which, and when a similar signal was needed again, he would run another wire. I believe that the telephone wires were similarly supported and duplicated.

Hodgson had been named Assistant Dominion Astronomer, again in my opinion, a wise selection. I have mentioned elsewhere my father's abhorrence of disorder and his meticulous attention to detail. I think he lacked the breadth of vision to carry out the massive reorganization that was required, but he was exactly the sort of lieutenant Beals needed. He was put in charge of the clean-up. A truck was drawn up to the back door and everyone was encouraged to get rid of accumulated junk. Hodgson stood in judgement about what should go; it seemed that the only word he knew was "Out"! The three carpets, the festoons of wires and, one may suppose, the chickens, all went. I have never heard of anything that anyone regretted. The entire building was redecorated, telephone and time lines were installed, properly identified, in modern conduits.

The situation with Observatory House was not much better. Stewart didn't leave the house until March 1947, and the Beals had to live in a hotel, not only through their first winter, but until the end of the year, by which time the house had been renovated and redecorated to his specifications. The renovations were extensive, involving the removal of walls to provide more gracious living space. The Department of Public Works wasn't too happy about it and in mid-July expressed the opinion that it would be better to build a new house than to proceed with the renovations[6]. Nevertheless they pressed on, and in early January 1948, Beals was able to write to the Chief Architect: "From being a dilapidated and depressing place to live, it is now bright and cheerful and the kind of house in which one can take a good deal of pride."

This mattered a great deal to Beals. We saw in Part I the importance Plaskett placed on having a residence suitable for the reception of visitors to his observatory. This is an astronomical tradition; an Observatory House is a complement to most major Observatories. Our Observatory House became a gracious home in which visitors were entertained and in which, beginning in 1949, the entire staff was welcomed at a Christmas party, complete with Santa Claus.

Beals had little administrative experience, little knowledge of administrative procedures, and no familiarity whatever with the people involved. He was not much helped by the Observatory's administrative staff. Orville Sills, who had been appointed Administrative Officer by Klotz, was still in that position, and was not helpful. He felt that he should have been named Director since only he could understand the complexities of administration. In a sense he was right. Over the years he had developed a system of such labyrinthine complexity that no one else could understand it; it was a black hole into which things could disappear never to be found again. Sills was superannuated in early June 1947, to be replaced by H.H. Cooper, a competent and loyal lieutenant who was of great assistance to Beals in bringing order out of the existing chaos.

When Beals arrived in Ottawa, the Observatories were part of the Department of Mines and Resources, a Department that had inherited a great hodge-podge of responsibilities from the Department of the Interior. The scientific functions of the Department were grouped in a "Surveys and Engineering Branch", directed by J.M. Wardle; the name of the Branch was changed in about 1948 to "Mines, Forests and Scientific Services", under a new Director, W.B. Timm. Throughout this period the Deputy Minister was Dr. H.L. Keenleyside.

To judge from the correspondence that survives, Beals rapidly established good relations with each of the Directors to whom he reported. How did he fare in the larger councils of the Department? No records remain, but it is probable that things were not easy. There would have been some residual resentment from those who had hoped for the transfer of astronomy to the National Research Council and the amalgamation of the remaining Branch into the Geological Survey, and a general resistance to curiosity oriented research. Evidence of this is found in a memorandum[7] in which Beals proposed that the Observatory should be reorganized into a "Dominion Institute of Astronomy and Geophysics, in close co-operation with the Geological Survey for the benefit of the mining industry of Canada". The memorandum was accompanied by a review of existing programs, appropriately oriented. Nothing came of the proposal, but it suggests that Beals felt defensive.

In early 1950 there was a major Government reorganization; responsibilities for Indian Affairs, Immigration, Forestry, Water Power Resources and the National Museum of Canada were transferred to other Departments, and the new Department of Mines and Technical Surveys became an "integrated organization whose primary function [was] to provide technological assistance in the development of Canada's mineral resources through studies, investigations and research in the fields of geology, mineral dressing, and metallurgy, and of topographic, geodetic, and other surveys[8]." There were five scientific Branches, of which "Dominion Observatories" was one, reporting directly to the Deputy Minister. Marc Boyer was the first Deputy Minister of the new Department. A further change was added in 1951; an office of "Director General of Scientific Services" was interposed between the Branches and the Deputy. Initially this position was filled by G.S. Hume; in 1956 he was succeeded by W.E. van Steenburgh. Over the next ten years there were numerous changes in the senior departmental administration, with the office of the Director General moving from a line to a staff responsibility and back again, but, throughout, van Steenburgh played a major role in the affairs of the Observatories. Finally, in 1962, on the death of Marc Boyer, he became Deputy Minister, and continued in that position until 1966.

In the notes already mentioned[4] prepared for an after-dinner speech, Beals speaks of the problems of his early days in Ottawa:

> "Consisted of meetings, committees, boards, reports, financial discussions, personality crises, feminine temperament, dinners, receptions and numerous other apparently useless and time-consuming activities. While they no doubt have their uses I have found a majority of government committees a crashing bore and have followed Sam Goldwyn's example of saying "include me out" whenever possible. Became expert at spotting and if possible forestalling the character who gums things up by starting a spirited argument on subject totally foreign to the matter under discussion."

Beals knew little about geophysics, a discipline that involved somewhat more than half his staff. This did not bother him. He was a well-trained physicist, quick to grasp the fundamentals, and completely candid about his lack of knowledge of details. In those days the Civil Service worked until 1 pm on Saturdays; each Saturday morning Beals would make the rounds of the various offices and laboratories, asking questions, making suggestions, offering encouragement. His education was furthered by regular attendance at meetings of the National Research Council Associate Committee on Geodesy and Geophysics, never hesitating to ask questions, never worrying whether the questions appeared elementary to others.

Beals had no illusions about the problems he faced in the reorganization of the Observatory. Indeed he arrived in Ottawa with an unjustifiably low opinion of its scientific standards. He said many years later that in considering the move he had been shocked to find that the only Publications being issued were of the Bibliography of Seismology. He took this to mean that no scientific work was being done. This was unfair; it ignored the fact that much of the output of the geophysics divisions took the form of bulletins and it took no account of publications in outside journals. Nevertheless this attitude did define the new Director's burning ambition – to bring the Observatory into a position of scientific respectability.

The years of Beals' directorship lacked the confrontations of the King and Klotz years, and the insurmountable problems that faced Stewart. They coincided almost exactly with the benign post-war era in which science was expanding everywhere, in universities and in government, and before the navel-gazing years of business-school management. Without being deeply committed to formal planning and management, he moved the Observatories along a steady, upward, path. While the advance was made possible by the climate in which he found himself, Beals was very definitely the architect of the organization that developed in the post-war years.

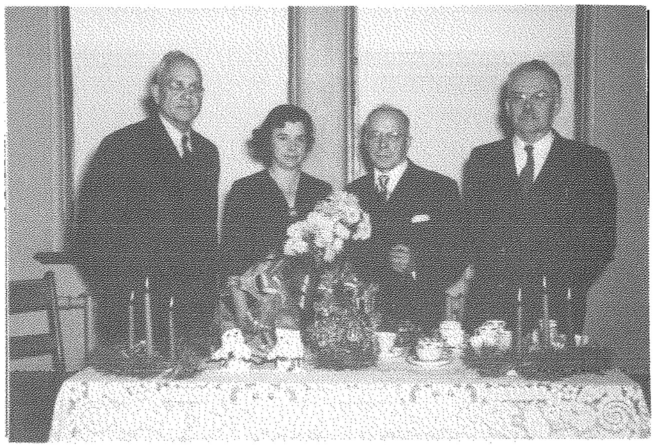
ADMINISTRATION

In this section, and indeed in most of this chapter, the account will dwell largely on developments in Ottawa. The Dominion Astrophysical Observatory had not deteriorated during the war years and, given support in staffing and budget, could move forward without direction from Ottawa. Its administrative developments will be treated in Chapter VII.

Staff Development

Beals' first task was to build up a competent staff and to restore a morale depleted by years of neglect. Stewart had already begun this process. M.J.S. Innes and W.G. Milne had been recruited to the Gravity and Seismology Divisions respectively, P.H. Serson, J.F. Clark and A.A. Onhauser had strengthened the Magnetic Division, and the Positional Astronomy Division was at full strength. Because E.A. Hodgson was Assistant Dominion Astronomer his position in Seismology was vacant, even though he was still Division Chief. Beals invited me to apply for the position and I joined the staff in May 1949. E.A. Hodgson retired in December 1952 and the position of Assistant Dominion Astronomer was allowed to lapse.

Except for Astrophysics, each division was now a viable entity, with a chief, a potential successor, and enough staff to do serious work. The reorganization of the Astrophysical Division was soon accomplished[9]. "During the past year two of the former Divisions, namely, Solar Physics and Photoelectric Photometry, together with a new branch of work on meteoric astronomy, have been combined into a single Division of Stellar Physics. ... Dr. P.M. Millman, formerly of the David Dunlap Observatory, and Squadron Leader in the RCAF, joined the staff of the Observatory on June 15, 1946." Millman was to be responsible for the work in meteoric astronomy. J.L. Locke was recruited in the spring of 1949 to take charge of the solar program[10]. Initially, Beals acted as Chief of the Division but Millman was confirmed in that position shortly afterwards.



Retirement party for Dr. E.A. Hodgson, December, 1952. L to R: Dr. G.S. Hume, Mrs. Hodgson, Dr. Hodgson, Dr. Beals.

Beals built on this foundation. He arranged for the retirement of some older staff members and their replacement with younger and better trained, or better motivated, people. He succeeded in having a number of new positions established; he normally limited his requests for new positions to three per year, believing that the Observatories could not assimilate staff effectively at a faster rate. He monitored the competitions for these positions carefully. In his travels, in Canada and overseas, he was on the look-out for suitable recruits.

In discussing these early days many of my former colleagues have pointed out a problem of which I was never sufficiently aware, perhaps because I was part of it. They refer to the rigid compartmentalisation imposed by the Divisional system. Years of underfunding had led to an adversarial attitude between Divisions which inhibited inter-disciplinary cooperation. Junior scientists, particularly those coming from university departments, found this atmosphere stultifying. Beals was fair in assigning resources; I'm not sure whether he recognized the deeper implications of the system.

The following table, derived from Departmental Estimates[11], shows the growth of the staff throughout both the Beals and the Hodgson regimes. The data are not completely satisfactory; the format varies throughout the period and there are sudden, spurious, jumps in the numbers, indicated by asterisks, which seem to be due to differences in the way the seasonal staff is reported. There are other significant jumps, as we shall presently see.

These numbers are faceless, but it is quite impossible to list all the additions and changes in the staff in the space available; the individuals will be known by their scientific works, documented in succeeding chapters.

It was not enough to increase staff numbers, it was also necessary to motivate the existing staff, to bring them out of the scientific shallows in which they had been stagnating and into the main stream. One of Beals' first acts was to reinstitute the seminars, which had not been held for many years. Another was to bring distinguished scientists to the Observatory to advise and stimulate the staff.

Shortly after his arrival Beals made a proposal[12] in this regard. "I would like very much to see some mechanism developed whereby we could employ a limited number of foreign scientists during the summer months. Even in a country like the United States, which is very much more self-contained scientifically than Canada, the stimulation provided by outside scientists is considered absolutely essential. ... The needs of Canadian scientific institutions for such outside contacts is even greater." This program was accepted by the Department and, over the years, a number of distinguished scientists – Byerly, Coulomb, Bartels, Bullen, Lehman, Honda, Runcorn, Rikitake and several others – visited the Observatory for periods of a few months and provided intellectual stimulation to the young staff.

A second source of stimulation was provided by postdoctoral fellows; these fellowships had been available in the National Research Council since 1948; in 1955 the program was extended to other science-oriented departments, including Mines and Technical Surveys. Young scientists from overseas, in their immediate post-doctoral years, were eligible for these fellowships which were tenable for one year, but could be extended to a second. The Branch was normally assigned three fellowships at any one time. These bright young people brought fresh ideas to the Observatories as well as providing highly-motivated extra hands. Some of them, among others A.H. Batten, W.R. Jacoby, R.P. Riddihough, R.A. Stacey and J. Popelar, joined the permanent staff.

The Observatory was the first Branch in the Department to begin what might be called sabbatical leave. In 1958 I applied for leave with full pay to spend a year at the University of Paris. Beals and van Steenburgh supported me enthusiastically, the Department worked with Treasury Board to draw up a set of guidelines, and I spent the academic year 1958-1959 in Europe. This lead was followed in other Branches.

In a somewhat parallel process a number of staff members were transferred to other organizations to prepare them for new responsibilities within the Branch: J.A. Galt spent a year at Jodrell Bank Radio Astronomy Observatory before the establishment of the Dominion Radio Astrophysical Observatory, and V. Gaizauskas spent a year at the Kitt Peak National Observatory while preparing plans for the reorganization of the solar physics program.

Shortly after his arrival Beals established the series "Contributions from the Dominion Observatory", which paralleled an already existing "Contributions from the Dominion Astrophysical Observatory". These series consisted of reprints from journals; each reprint was assigned a number in the series, and each was issued with a standard cover. The Contributions gave continuity and publicity to the output of the Observatories and, since they obviate the need for elaborate journal searches, have proved invaluable to me in preparing these notes on the scientific output of both organizations.

STAFF NUMBERS - DOMINION OBSERVATORIES						
FISCAL YEAR	OTTAWA			VICTORIA		
	Permanent		Temporary	Permanent		Temporary
1947-1948	32		14	6		4
1948-1949	32		14	5		4
1949-1950	32		26*	6		9*
1950-1951	32		31	6		7
1951-1952	33		51*	8		12*
1952-1953	31		57	9		13
	Full Time	Part Time	Seasonal	Full Time	Part Time	Seasonal
1953-1954	69	4	21	18	-	4
1954-1955	70	4	26	20	-	5
1955-1956	75	4	30	18	-	5
1956-1957	79	4	30	19	1	5
1957-1958	82	4	30	19	1	5
1958-1959	82	4	26	19	1	6
1959-1960	92	4	31	19	1	6
1960-1961	102	4	35	22	1	7
1961-1962	109	2	35	24	1	7
	Staff		Man Years	Staff		Man Years
1962-1963	174*		148	33*		28
1963-1964	174		148	33		28
	Continuing		Casual	Continuing		Casual
1964-1965	138		11	25		2
1965-1966	146		11	30		2
1966-1967	152		11	35		2
1967-1968	176		13	38		2
1968-1969	189		13	39		2
COMBINED STAFF						
			Continuing	Casual		
1969-1970			214	20		
1970-1971	Earth Physics Branch Staff - 164 my					

Financial Resources

There was a steady advance in the scientific productivity of the Branch. Existing programs were expanded, new ones were begun. What were the resources that made this possible?

The following table shows the annual budgets for the two observatories and the part of that specifically allocated to the purchase of scientific equipment. It is derived from the Departmental Estimates already alluded to[11].

Problems of Space

Where were the expanding programs and increasing staff to be housed? The problem of space was urgent almost from the time Beals arrived. Extra desks were placed in the existing offices, and the "round rooms" at all levels, including the windowless one immediately beneath the dome, were converted to offices. When work began on the airborne magnetometer in 1948 the design group took over part of the basement of Observatory House, which never again reverted to Beals' personal use; when Beals moved into a newly-constructed home in Manotick in 1963, Observatory House was converted entirely into laboratories and offices.

In 1957 a Defence Research Board laboratory building became available and was taken over as an instrument design shop by the Magnetic Division. This site was on grounds of the Experimental Farm, south of the Arboretum, between the Prescott Highway and the Canal. An additional, non-magnetic, building was later erected on the same site to house the astatic magnetometer. The site was abandoned in 1968 when the Ottawa Magnetic Laboratory, at Blackburn, was completed. This development will be discussed in the following chapter.

Almost immediately on his arrival Beals began to campaign for additional space. A memorandum[13] dated September 30, 1948, proposes "an extension to the Observatory Building", consisting of three floors and a basement, 120 by 48 feet. Nothing further was heard of this plan, but by 1952 the proposal for a separate building was accepted. This building, consisting of a basement and one additional floor, was to lie in an east-west direction in the area between Observatory House and the Machine Shop; it was designed with strength adequate to permit the addition of a second storey. Plans were completed by December, 1952[14], and the building was officially opened on March 30, 1955. It provided a spacious assembly hall and comfortable laboratory

ANNUAL BUDGETS - DOMINION OBSERVATORIES

	OTTAWA		VICTORIA		
FISCAL YEAR	TOTAL BUDGET	SCIENTIFIC EQUIPMENT	TOTAL BUDGET	SCIENTIFIC EQUIPMENT	
1946-1947	$155,590		$46,993		
1947-1948	205,591		55,213		
1948-1949	317,018		71,486		
1949-1950	312,630		72,859		
1950-1951	413,788	$104,090	65,385		
1951-1952	450,588	79,990	84,340		
1952-1953	514,194	87,850	104,566		
1953-1954	518,057	81,500	108,981		
1954-1955	579,421	67,000	120,353		
1955-1956	621,860	60,000	116,663		
1956-1957	697,653	92,138	148,997		
1957-1958	774,019	85,000	161,145		
1958-1959	1,472,646	331,000	296,390	$29,300	
1959-1960	1,540,537	224,800	192,912	67,975	
1960-1961	1,608,288	288,500	259,986	163,525	
1961-1962	2,143,575	250,850	446,000	152,600	
1962-1963	2,571,800	355,000	253,400	45,700	
1963-1964	2,217,000	305,000	278,500	51,000	
				DAO	QEII
1964-1965	$2,643,000	$479,000	$403,000	$73,000	
1965-1966	2,893,400	628,000	1,289,400	92,000	$503,000
1966-1967	3,223,000	730,000	1,760,000	46,000	826,000
1967-1968	3,833,000	824,700	2,050,000	161,000	450,000
1968-1969	4,383,500	864,000	845,800	50,000	54,000
1969-1970	TOTAL BRANCH BUDGET of which EQUIPMENT			$5,558,600 $1,225,600	
1970-1971	EARTH PHYSICS BRANCH BUDGET of which EQUIPMENT				$3,983,000 $486,000

and office space for the Gravity and Magnetic Divisions, as well as a laboratory and a small solar tower with coelostat for the Solar Physics group.

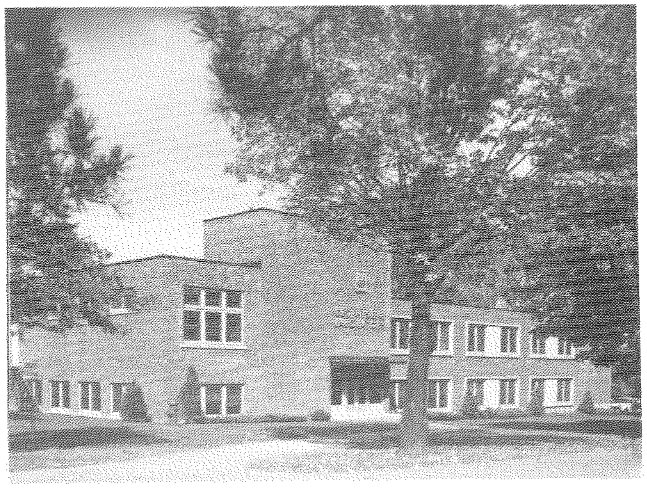
The Seismology Division was able to expand in the main building into offices on the second floor; this in turn allowed the basement rooms they had been occupying to be turned into a laboratory for the development and testing of seismic equipment.

The relief afforded by this new building was short lived and by early 1958 Beals was pressing for the addition of the proposed second floor. Plans were available by October 1958, at which time someone discovered that, in fact, the foundation was not strong enough to support the second floor. The plans were modified to provide a wing running at right-angles to the existing one, resulting in an L-shaped building with basement and main floor. The building was completed by 1960.

The next major break occurred in 1961 when the Geodetic Survey staff was transferred to the main Departmental complex on Booth Street, vacating the Geodetic building for Observatory use. It had suffered many years of neglect, and its refurbishing required some months, but the building was ready for occupancy by mid-1962. The Library moved to the top two floors, Seismology to the basement and ground floor. At the same time a tunnel was constructed connecting the new "Seismology" building to the main Observatory, and new vaults were constructed off this tunnel.

To complete the story on space allocation, when Astronomy was transferred to the National Research Council in 1970, the Library was transferred back to the main building, where it took over most of the space vacated by the Positional Astronomy Division. Seismology expanded to fill the old Geodetic building.

This will be a suitable place to mention two initiatives which Beals took in building improvements. The first, involving the installation of an elevator in the main Observatory building, was made in 1957. Originally the building had a spacious central hall, rising to the second floor, with a curved staircase along its outside wall. The elevator, which travelled from the basement to the dome, filled up the free space in the hall. It was a mixed blessing; we lost our gracious entrance hall, and the structure on the roof, housing the hoist machinery, was something of an eyesore.


The Geophysical Laboratory.


The refurbished Geodetic Building with its new "Seismology" sign.

The second initiative was more attractive. Juan Geuer, an artist who joined the staff as a draughtsman in 1956, proposed that he should paint the ceiling of the first floor round room with a mural, based on the signs of the zodiac. Beals agreed enthusiastically, and Geuer spent several months on the work. Everyone was so pleased with it that it was arranged for Geuer to spend several months at the Dominion Astrophysical Observatory painting a mural in the entrance hall to the dome.

THE SCIENTIFIC ENVIRONMENT

The years of the Beals' administration represented, for the Observatories, as for Canadian science in general, a period of unprecedented expansion. Existing programs were enlarged and many new ones were begun. These new developments will be described in detail in later chapters. Some of these programs were initiated by Beals, most grew from the interests of the Divisional staffs or were imposed by international pressures; the important point is that there was a surge in the scientific output of the Dominion Observatory and that support for the already strong program of the Dominion Astrophysical Observatory was maintained. In this section we shall consider some of the factors that influenced the development.

The first of these factors was the attitude of the government. Canadian industry, aided by Canadian science, had played a major part in winning the war, the economy was expanding, and the government recognized, in science, the machinery that would drive that expansion[15]. Annual increases, modest in hindsight, were made in the budgets of the scientific departments, and the National Research Council set up a program of grants to support graduate research in universities[16]. Then, on October 4, 1957, the Soviet Union launched Sputnik I. With the realization that the west's scientific predominance was far from assured, the level of funding for science increased dramatically. For the Observatories it almost doubled, as shown in the table on page 8. From that time forward, worthwhile projects had a reasonable chance of support. The machinery for obtaining such support was uncomplicated. For most projects the decision was made by Treasury Board; larger ones were referred to the Privy Council Committee for Scientific and Industrial Research. Senior Ministers sat on this committee, and they could call on knowledgeable scientists in government, universities or industry for guidance.

Generous educational grants were made to discharged military personnel. These served two purposes, the direct one of providing for their education, the indirect one of easing the return of such large numbers to the work force. These young people made excellent students; they had been matured by their war experiences, they were eager to make up for the time they had lost. Several of these returning students played an important role in developing geophysics in Canada.

Relationship with Universities

It is necessary to distinguish between astronomy and geophysics in considering our relationship to the universities. Astronomy had been a field of excellence for Canadians for several generations. The two principal groups, at the Dominion Astrophysical Observatory and at the University of Toronto, cooperated scientifically but operated independently of each other, coordinating through the National Committee for the International Astronomical Union (IAU). Many of our astronomers had studied, as undergraduates or graduates, at the University of Toronto, and it continued to be a source of trained personnel. With the advent of the Queen Elizabeth telescope we attempted to establish a close relationship with the universities through the establishment of an Institute of Astronomy on the campus of the University of British Columbia; with the cancelation of the telescope the attempt came to nothing. That failure will be discussed in a later chapter.

Shortly before the end of the war the Department of Physics of the University of Toronto began to develop a section of geophysics. It was initially devoted almost exclusively to mining geophysics; in 1947, under the leadership of J.T. Wilson, its interests were channeled to "pure" geophysics. A number of war veterans were enrolled in its courses, undergraduate and graduate; many of these worked for the Observatory during the summer months and used the observations they had taken as a basis for thesis work. Several Observatory staff members, on educational leave, took graduate degrees at Toronto at the same time. The Toronto group had, of course, research interests beyond those of the Observatory. Members of this early group of graduate students went out from Toronto to found geophysics departments in a number of other universities or became Division chiefs in, and ultimately Directors of, the Dominion Observatory. This common origin led to a cohesion of planning and a cooperation of effort that will be apparent in the scientific chapters to follow.

The relationship between the Observatory and the developing university departments was a symbiotic one. As the departments developed, more and better trained scientists became available to occupy positions in the government and in industry, and a venue was provided for our existing staff to upgrade their training. The use of educational leave became a major tool of staff development, and was soon extended to cover attendance at universities in the United States and Europe. At the same time we were able to support the university groups through the loan of equipment, by including them in major projects such as large-scale crustal studies and by supplying them with data from our networks of magnetic and seismograph stations. The two groups stimulated each other: interest in terrestrial heat-flow expanded from the universities to the Observatory; studies in magneto-telluric currents and geomagnetic depth sounding moved in the other direction.

Associate Committee on Geodesy and Geophysics (ACGG)

In considering factors that influenced the development of the Observatory we must certainly include this committee[17]. It was set up in October 1945 to advise the National Research Council (NRC) on the needs of geophysics – applied geophysics. Electronic developments accelerated by World War II research suggested important possibilities in magnetic and electric prospecting, and the committee was structured to develop these. It was chaired by Col. J.T. Wilson, Director of Operational Research, NDHQ, fresh from his exploits as leader of Operation Muskox.

Seventeen members were present, and they established six working subcommittees: magnetic, gravity, electromagnetic, seismic and acoustic, tectonophysics and miscellaneous geophysical methods, electrical phenomena of the Earth. Over the next two years the subcommittees reviewed their fields of responsibilities thoroughly and brought in specific recommendations.

Within a year the question of the relationship of the NRC Committee to the International Union of Geodesy and Geophysics (IUGG) was being considered. As we saw in Part I, Canada adhered to the IUGG through a committee, now largely moribund, in the Department of Mines and Resources. A joint meeting of the two committees was held in November 1946, at which it was agreed that they should amalgamate to form the Associate Committee on Geodesy and Geophysics (ACGG) of the NRC. This committee would serve the dual purpose of advising the Council on developments in geodesy and geophysics, pure and applied, and of serving as "The National Committee for Canada of the International Union of Geodesy and Geophysics." Wilson remained as chairman of the reformed committee, with the officers of the defunct EMR committee assuming honorary committee assuming honorary positions. Stewart became a member of the enlarged committee and so remained, even after his retirement, until Beals could be appointed.

Initially it was felt necessary to adapt the subcommittees to fit the Association structure of the Union, but this arrangement was soon abandoned. Subcommittees were set up to satisfy the national need; for international purposes one or more subcommittees could report to a particular Association. Over the years the numbers of subcommittees expanded from six to thirteen: Aeronomy, Exploration Geophysics, Geodesy, Geodynamics, Geomagnetism, Glaciers, Gravity, Hydrology, Isotopes, Meteorology, Oceanography, Seismology, Volcanology. The sub-committees on Geomagnetism, Gravity, Seismology and Geodynamics were of particular interest to the Observatory, and during the period here considered they were almost always chaired by Observatory personnel.

One early effort of the Committee, of lasting importance, was the establishment of the Canadian Geophysical Bulletin, which began in 1947 as a quarterly, becoming an annual publication in 1952. It provides an invaluable conspectus of Canadian Geophysics and publications. I know of no equivalent publication elsewhere, and I have been much indebted to it in the preparation of this history. The Editors of the Bulletin over the years were as follows:

1947-1951 – J.T. Wilson
1952 – E.A. Hodgson
1953-1968 – G.D. Garland
1969-1974 – C.M. Carmichael.

In 1952 the ACGG initiated a study on the possibility of establishing a Canadian Journal of Geophysics. A survey of the numbers of papers that might be submitted to such a journal yielded numbers too small to convince the NRC of the viability of a new journal, and the matter was dropped. It was raised again in 1962, a new survey was made of potential papers, the total was sufficient to convince the Editorial Committee of NRC, and the Canadian Journal of Earth Sciences began publication in 1964.

Wilson retired from the chairmanship of the ACGG in 1950 to be succeeded by Beals. Thereafter the succession of Chairmen was as follows:

C.S. Beals – 1951-1957
B.W. Currie – 1958-1964
R.J. Uffen – 1964-1967
J.H. Hodgson – 1968-1971
J.A. Jacobs – 1972-1974.

It is impossible to exaggerate the importance which the ACGG had for Canadian geophysics. It provided a forum for the effective exchange of information about national and international developments in geophysics. It directed Canadian participation in a number of international efforts, some of which we shall now consider.

The International Geophysical Year (IGY)

The IGY was initially conceived as a successor to the International Polar Years of 1882-1883 and 1932-1933, but its scope rapidly expanded beyond the Polar regions to include the equatorial zone and several belts joining pole to pole. It was managed by a committee established by the International Council of Scientific Unions – le Comité Spécial de l'Année Géophysique Internationale (CSAGI). Planning began in mid-1951; detailed programs were designed, instrument standards were established and detailed instructions were prepared and published to ensure worldwide uniformity in the observations. Three World Data Centres were established to receive and exchange the vast amount of data anticipated. To accommodate the scope of the work the "year" was extended to eighteen months, from July 1, 1957 to December 31, 1958.

Canadian participation was guided by the ACGG. The project is first mentioned in the minutes of February 20, 1953, at which time a small committee was set up under Beals to consider the Canadian position. Following an extensive survey, it issued a preliminary report on IGY researches to be undertaken in Canada, and established an enlarged working group, with specialists in all the areas of interest, to take responsibility for the program. This group refined and costed the proposals. In 1955 a "Coordinating Committee", made up of four senior government directors, was appointed by the President of NRC to administer the program. These four directors – C.S. Beals, F.T. Davies, D.W.R. McKinley and D.C. Rose – were in a position not only to direct the work but to ensure its financial and logistic support. This was an important precedent, which worked extremely well and which established a procedure for subsequent international programs.

The IGY coincided with a period of maximum sunspot activity, with resultant disturbances in the earth's magnetic field. The detailed mapping of solar flares was a basic IGY program. A number of Observatories, including our own, cooperated, providing detailed information on the growth and decay of the flares. To study the magnetic effects our Geomagnetic Division extended its network of observatories throughout the country, including the Arctic, and began, for the first time, sophisticated analysis of the records in addition to the routine publication of data. Recording fluxgate magnetometers were supplied to some dozen auroral or ionospheric stations operated by universities or other departments, which laid the basis for continuing cooperation in post-IGY years.

In demonstrating that cooperation between scientists throughout the world could yield results greater than the sum of the individual efforts the IGY changed the direction of geophysical research. Not the least of its benefits was the increased funding it solicited from the participating governments. That the IGY produced a bulge in both our staffing and budget is evident in the tables given earlier. There is some lag in the financial increase, due to the length of time required to build and instrument station in remote locations, but the effect is certainly present.

Upper Mantle Project (UMP)

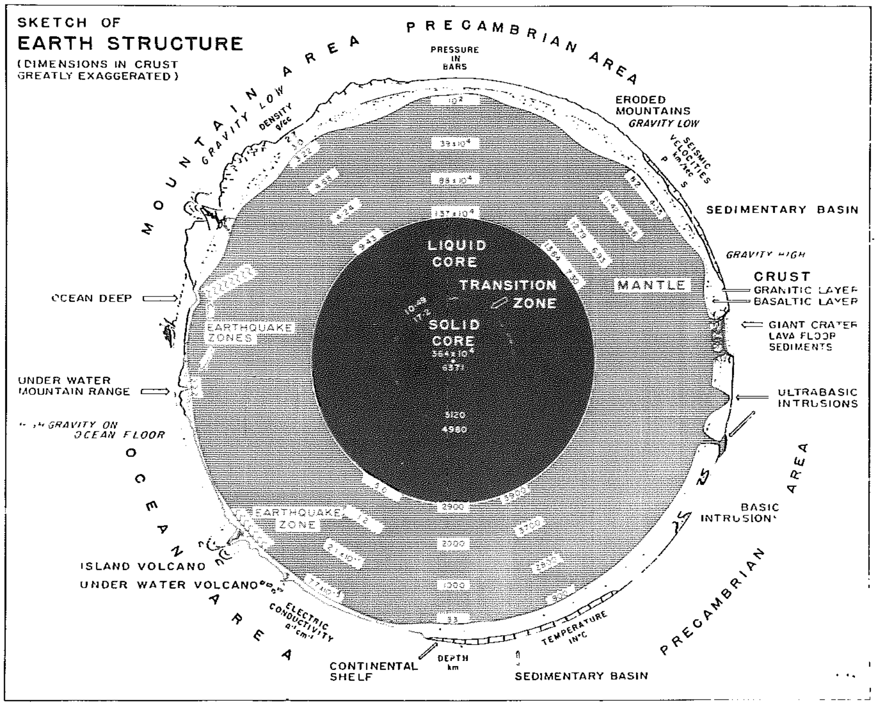
The Helsinki meetings of the IUGG, in July 1960, were dominated by papers reporting on work done during the IGY. There was a general feeling of euphoria at a job well done. In this climate Professor V.V. Beloussov, Chief Soviet delegate, proposed that this cooperative spirit should be continued, but directed towards the earth's interior, specifically to the "Upper Mantle and its influence on the development of the earth's crust"[18]. His proposal was accepted by the IUGG executive which established a working group to develop the idea and called on member countries to consider what they could do within the general area of the project[19]. The International Union of Geological Sciences (IUGS), through its Canadian Chairman J.M. Harrison, also endorsed the project. It was agreed that it would cover the three-year period January 1962 to December 1964.

Despite these official signs of support there was little action taken by the Member countries. Canada was the exception. Several people, particularly R.J. Uffen and G.D. Garland, urged that a Canadian program should be planned, and the matter was taken up by the Associate Committee. Following the experience gained in the IGY, a sub-committee was set up, known as the Scientific Committee for the Upper Mantle Project (SCUM). This Committee, which originally consisted of four people – G.D. Garland, J.H. Hodgson, L.W. Morley and R.J. Uffen, was to plan the Canadian program. The Committee was gradually expanded and modified, but it continued as an effective mechanism throughout the planning stages. At the same time, again profiting by the experience gained during the IGY, a "Coordinating Committee" was established to "grease the wheels". This Committee was chaired by W.E. van Steenburgh, with D.C. Rose to represent the NRC and R.J. Uffen to represent the Universities. Later I became a member as a sort of secretary to van Steenburgh.

The in-house government responsibilities were entirely within EMR, principally in the Observatory and the Geological Survey (GSC). By May 1961 a Departmental program had been decided on and its staffing and budgeting implications worked out. Except for the establishment of a new section on Heat Flow the program for the Observatory called for a speed-up of work already planned, rather than for new departures. In most respects this was also true of the GSC, but they also planned to drill two deep holes in ultrabasic bodies to actually sample mantle rocks. The Departmental program called for the establishment of 45 new positions; of these 33 were in the Observatory.

This Departmental program was described in a 26-page pamphlet[20] which received very wide circulation, not only in Canada but throughout the world, and served as a model for many other national programs. It suffered from the fact that no mention was made of University projects. To meet this need, a progress report was prepared for distribution at the Berkeley Meetings of the IUGG in August 1964[21].

Because many of the Canadian publications arising from the Project would be published in British or American journals it was feared that the Canadian contributions might be overlooked. It was agreed that all papers published as part of



In order to better understand programs proposed in the Upper Mantle Project, Beals, with the artistic assistance of Juan Geuer, produced this cross section of the Earth. It was widely copied.

the program should carry the footnote "Canadian Contribution to the International Upper Mantle Project No. xx". A register to issue contribution numbers was set up within the Seismological Division, and lists of contributions were issued at regular intervals.

The two Canadian reports spurred national committees in other countries to action, but it was slow in developing. An Upper Mantle Symposium was held at the Berkeley Assembly, but the papers consisted mostly of pious hopes or a rehash of past work. Little had been accomplished in an organizational way. The Committee set up in Helsinki was still in existence, it had proposed that its membership should include some representatives from the IUGS[22] and this was formally approved by the General Assembly[23], but Canada was still the only country with any established program. By May 1964, this fact was recognized by the Committee, which "as a general policy, ... decided that the Upper Mantle Project shall initially consist of a three year period beginning on 1 January 1965. The introductory preamble [to the announcement] is to be worded in such a way that those countries, such as Canada, that have already made considerable contributions to the Upper Mantle Project will not look as though they had been cut off.[24]" At the same time the official association of the IUGS in the Project was recognized.

An important result of this expansion and rescheduling of the Project was the establishment of a Canadian Secretariat in the Geological Survey under the direction of C.H. Smith. This Secretariat, which worked closely with the SCUM and Coordinating Committees, provided excellent leadership to the Canadian program as well as coordination with the international organization. It, and the International Program, continued in operation until 1970. Further progress reports were published in 1964 and 1967[25].

The establishment of the Secretariat removed the Observatory from its central position in the project and, in a sense marked the end of our involvement. We played an active part in symposia[26] and in other reports of our work, but increasingly this work had become part of our normal programs. As shown in the staffing and financial tables given earlier, the UMP had given us the staff and funds to make a step increase in our programs and we were getting on with the job.

The Polar Continental Shelf Project (PCSP)

This Project, which had a great influence on the work of the Dominion Observatory, was not developed in the Associate Committee. It had its birth in the politics of the day[27]. In the election campaign of 1958, Diefenbaker announced that he had a "Vision – A Canada of The North". The voters were caught up in the rhetoric, and the Conservatives swept into power with the largest majority seen in Canada up to that time. Diefenbaker's vision was much more than rhetoric. Before the election he had appointed Alvin Hamilton as Minister of Northern Affairs and National Resources. Hamilton "knew the North, had for years been preaching its potential and was now at last in a position to act"[28]. Among the means available to him was a senior committee, the Advisory Committee on Northern Development.

The idea for the Polar Shelf Project originated in this committee, but because most of the scientific work envisaged lay within the competence of the Department of Mines and Technical Surveys, the design of the Project was done by a committee chaired by van Steenburgh. The Project was conceived as an organization that would carry out scientific programs of its own, while at the same time providing logistic support for a variety of government-sponsored scientific work in the high Arctic. The committee proposed[29] that a reconnaissance project, based on Isachson, should be undertaken in 1959, with the first full year of operation planned for 1960. It called for the construction of a scientific laboratory and additional living quarters at Isachsen, for the establishment of a Decca navigation system, for sea and air supply to Resolute and for both fixed-wing and helicopter support of the various working parties. A staff of six people, including a Senior Coordinating Officer, was proposed. The Advisory Committee on Northern Development approved the plans and it was accepted by the Government.

The Project was managed by a Steering Committee chaired by van Steenburgh, made up of all Branch Directors and the Departmental Chiefs of Administration and Personnel[30]; it was granted a separate budget and establishment. When a Branch needed additional staff to carry out its Project responsibilities, these were established and costed within the Project establishment, although, as the interested party, the Branch might supervise the recruitment. When the Project work in a particular discipline became indistinguishable from the normal Branch responsibilities in that discipline, the responsibilities, and the associated staff, could be shifted to the Branch. A large number of people were recruited to the Observatory staff in this way.

As planned, the work began in the area of Isachsen, and progressed westward; the base was moved to Mould Bay in 1964, and to Tuktoyaktuk in 1968. Later two permanent bases were established, at Tuk and Resolute. From the beginning it was not confined to the Polar Continental Shelf, but was involved also with the Queen Elizabeth Islands and the Arctic Basin.

The history and accomplishments of the Polar Continental Shelf Project have been thoroughly and elegantly described, and pictured, by F. Foster and C. Marino in a book marking its twenty-fifth anniversary[31].

The Observatory work carried out through the Project will be described in the Divisional sections; the Project made possible a great deal of important work in the Arctic that would otherwise have been beyond Branch resources.

The Diefenbaker government ran into serious financial difficulties during its mandate and in 1962 it imposed an emergency austerity program which involved, among other things, a freeze on Civil Service hiring. No new positions were granted, and any position opened by resignation or retirement could not be filled. This proved a serious setback to the advances made both in the UMP and the PCSP.

Learned Societies

In the pre-war years covered in Part I there were few professional societies in Canada, and their numbers were limited even in the United States. The Royal Society of Canada, with its several sections, filled the need, not only for Fellows but for the scientific community in general. With the end of the war the number of professional societies burgeoned and attendance at the meetings of the Society declined. Nevertheless, as the national academy of arts and sciences, it remained an important institution in the intellectual life of the country.

Several staff members were elected to Fellowship: McKellar in 1942, J.H. Hodgson in 1958, Innes in 1962, Whitham in 1969. Pearce was President of the Society in 1949-1950, Beals of Section III (Physical and Mathematical Sciences) in the same year. The Tory Medal, awarded for "outstanding research in a branch of astronomy, chemistry, mathematics or physics" was won by Beals in 1957 and by Petrie in 1961.

For the astronomers the most important professional society was the American Astronomical Society. It met in Ottawa in 1949 and in Victoria in 1956. Beals was elected President for the years 1962-1964, the first Canadian to hold the office. For the Victoria staff the Astronomical Society of the Pacific was also important. The Ottawa staff involved in the meteorite crater program were active in the Meteoritical Society. Astronomers, both in Ottawa and Victoria, were strong supporters of the Royal Astronomical Society of Canada.

For geophysicists the Associate Committee on Geodesy and Geophysics, and its subcommittees, served many of the functions of a professional society. As we have seen, the main committee was effective in the support of major projects; the sub-committees, to an increasing degree, managed cooperative ventures and arranged symposia in their fields of interest. As will be described in the next chapter, this process eventually led to the establishment of The Canadian Geophysical Union.

The International Union of Geodesy and Geophysics (IUGG)

As we have seen, the Associate Committee on Geodesy and Geophysics became the National Committee for the International Union of Geodesy and Geophysics in 1946. The chairman of the committee was the principal delegate to the meetings, and sub-committee chairmen acted as principal delegates to the equivalent Associations. In the early post-war years, as throughout the previous history of the Union, the sub-committee chairmen exercised their responsibility through correspondence.

Beals was the only Observatory representative at the Union meetings in Oslo in 1948 and in Brussels in 1951. Unlike Ogilvy in the prewar years, he was a competent and active representative, attending Association meetings and reading papers and reports from Observatory staff. It was not, however, an arrangement satisfactory to his expanding staff, or consistent with his plans to develop the Observatory as an internationally recognized centre of excellence.

By the time of the next meeting, at Rome in 1954, Beals had become chairman of the ACGG. More than a year before the meetings he wrote to E.W.R. Steacie, President of the NRC[32]:

> "The organization of the Associate Committee of Geodesy and Geophysics depends to a large extent on a number of sub-committees whose annual reports and the discussion thereon form a major part of the agenda of the Committee meetings. The chairmanship of these subcommittees has sometimes been considered rather a labour by those who held these posts, although for the most part the work has been done willingly enough. ... However [some] have expressed themselves rather strongly that if they performed the work over a number of years ... they should have the opportunity of attending meetings of the International Union in order that they might perform these duties more efficiently. I must confess that there seems to me to be a great deal of reason in this view and I should be glad to know whether you think this would be a sound basis for a Canadian delegation to the Union."

The letter went on to suggest that the various government departments involved should be expected to support their own members so selected, and that the Council be asked to support six additional persons as delegates, using as a basis of selection their functions on the Associate Committee. Steacie supported the recommendation, and a small committee was set up, chaired by J.T. Wilson, to select the six people. I was one of the six; the Department agreed to support Beals, Madill and Innes. This was a matter of more than immediate importance, for it established a precedent; at subsequent meetings the number of delegates from the Observatory was always in reasonable relation to staff numbers.

As chief delegate, one of Beals' responsibilities was to invite the Union to hold its next meeting in Canada. This should have been a pleasant task, but it was a difficult one because the President of the Union, Sydney Chapman of Great Britain, was determined that the invitation should not be put before the Council of the Union; he favoured an invitation from Argentina. Beals persisted, the invitation was submitted, and declined in favour of Argentina. After a year it became evident that Argentina was not going to be able to hold the meeting, and the Canadian invitation was belatedly accepted.

The Meetings were held in Toronto, where J.T. Wilson and his staff did a magnificent job in staging a highly successful meeting despite the late start.

International Astronomical Union (IAU)

We saw in Part I that the Canadian Committee for the International Astronomical Union was reorganized after the death of Klotz to give adequate representation to both university and government astronomers, and to the two government groups. Throughout Stewart's administration its meetings were usually limited to the Directors of the three principal observatories, and its agenda to housekeeping matters related to Canada's membership in the IAU. Shortly after his arrival in Ottawa, Beals reorganized the committee, enlarging its membership, and turning its meetings into scientific sessions at which papers were presented. In 1971 this grew into the Canadian Astronomical Society.

The success of Beals' struggle to increase Observatory attendance at the meetings of the IUGG carried over to the IAU. Staff attendance and participation flourished.

SCIENTIFIC DIRECTION

There can be no doubt that Beals achieved his aim – to bring the Observatories into a position of scientific respectability. Indeed, as the chapters to follow will show, he far exceeded this aim. The Dominion Astrophysical Observatory continued to be, and the Dominion Observatory became, world-class institutions in their fields of expertise.

Beals was very much helped in his drive to excellence by van Steenburgh. "Van", as he liked to be called, was an entomologist by training who joined the Department of Agriculture in 1927, served in the artillery during the war, and became Director of Armaments Research before returning to the Department of Agriculture. He transferred to Mines and Technical Surveys in 1956, as Director General of Scientific Services. He continued in this post until 1962, at which time he became Deputy Minister. He retired in 1966.

Beals[33] describes him well:

> "A scientist himself, he had a nose for a scientific project that would succeed, and his remarkable sense of urgency caused him to cast a baleful eye on anything, from government regulations to foot dragging individuals, that stood in the way of any enterprise that he promoted. His understanding of the need of scientists for personal encouragement and freedom from bureaucratic pettifogging more than compensated for his tendency toward crustiness, so that working with him was always an enlivening and occasionally a hazardous experience."

Amen! He was a marvellous leader. The list of the things that he supported, and in many cases made possible, include the Polar Continental Shelf Project and the Canadian program for the Upper Mantle Project; the Canadian standard seismological network; the Yellowknife seismic array; the Dominion Radio Astrophysical Observatory; the 48-inch telescope at Victoria; and the Queen Elizabeth Telescope. He had retired by the time that telescope was cancelled. Beals speaks of him "shouldering obstacles aside with a few snorts of derision". J.M. Harrison, who worked closely with him for many years, expressed it another way. When problems became insurmountable he would call on Van "to drive his Mack truck through them." That always did the trick!

Beals was clear about his function as director. He was the chief scientist; he never let administration interfere excessively with his scientific interests or with his direction of the Observatory's scientific program. He continued his studies of emission line stars, particularly the P-Cygni stars, and of interstellar material, with the assistance of various members of the staff. These astrophysical studies had, of course, to be based on observations obtained at Victoria; there was no possibility of reviving the 15-inch telescope as a useful instrument.

About the time that he was exhausting the possibilities of available plates, Beals' attention was drawn to a large circular feature near the village of Brent, in Algonquin Park. This eventually proved to be a meteorite crater and it introduced Beals to a subject that occupied his scientific interest for the rest of his life. It was also a subject of the greatest importance in the development of the Observatory. It required the attention of both astronomy and geophysics, and brought almost every Division and a large proportion of the staff into the study at one time or another. This was very important in breaking down the Divisional barriers mentioned earlier.

By the mid-1950s Beals felt sufficient confidence in his organization and in his staff to delegate many of his routine responsibilities. I for example took over much of the routine work of staff recruitment, Serson oversaw the operation of the machine shop, Innes supervised the draughting office. After a few general instructions we were allowed our head. This left Beals much more time for his crater research. In the summer of 1962 he made an extensive tour of Europe to visit the sites of known or suspected meteor craters in France, Germany and the Soviet Union. He gave a number of lectures and met many fellow scientists previously known to him only through correspondence.

Beals was firm in his pursuit of excellence. Always open to suggestion, he would seek all points of view, but he had to be convinced of the scientific merit of any new project, or indeed of any continuing one, and would force a change of direction if he felt that it was called for. Every paper or report issued by the Observatory had to have his approval, both for its scientific content and its literary merit. He read every one and insisted on understanding it, if not in detail at least insofar as its aims, principles and conclusions were concerned. This sometimes led to embarrassing situations, when one was required to include the most elementary discussions in order to bring the Director's knowledge up to the level of the paper. Once, in frustration, I suggested to him that all papers in spectroscopy should include an elementary description of spectra; he was not amused!

The availability of the telescope for public viewing on Saturday nights had been an Ottawa tradition since the inception of the Observatory. Beals encouraged and expanded this program, freeing Miriam Burland from other responsibilities to manage it. Since the telescope was no longer being used for scientific work, she arranged for the visit of groups during the week. On Saturday evenings Divisions took turns in putting on additional displays, which helped control the flow to the telescope. Eventually the numbers of visitors grew so large that lectures had to be given in the assembly hall as a further means of control. Miss Burland prepared pamphlets, illustrated by Juan Gueur, on the various astronomic objects viewed in the program.

In the normal relations of his office Beals always acted with gentleness and tact. His shock at the dearth of Observatory Publications has been mentioned. His close friend Herzberg[34] tells us that when, on arrival in Ottawa, he found that many years of magnetic observations and the complete sequence of observations from the meridian circle had never been published, he, "with considerable tact and insistence, persuaded the staff members responsible for these measurements to publish them." It certainly was done with tact; I had always thought that the idea had originated with the authors.

Beals was very approachable, interested in the work and well-being of his staff. He continued his frequent visits to their offices and laboratories, talking to the scientists about their work, always interested in new developments. His door was always open and one didn't need an appointment to see him.

The foregoing, rather laudatory, words suggest that everything was perfect; it was not. In retrospect, Beals' judgement on the merit of scientific papers was not always sound. He was alarmed by anything that was speculative, even when it was based on sound observation. His insistence on absolute control may have been justified in the case of our own Publications, but when he refused to let papers go forward to refereed journals he was surely exceeding his authority; when he tried to impose his literary standards on journals with established editorial rules he was exceeding common sense!

Nor has time proved his judgement on scientific programs universally sound. His insistence on continuing the solar work in the very poor Observatory location was surely a mistake. The important palaeomagnetic program was started for the wrong reason, to test cores from the crater drilling program for magnetic meteoritic material. The astatic magnetometer was not the right instrument for his purpose, yet he tied it up for several years at a time when it might have moved the Observatory to the forefront of palaeomagnetic research.

The period of Beals' tenure was marked by an explosion in instrumentation. Wartime developments in materials and techniques had peacetime applications in all branches of geophysics and, to a lesser extent in astronomy. Transistors, first developed in the late 1940s, lowered power requirements and led to more portable field equipment. Because of the expansion of geophysical studies, both pure and applied, the supply of this new equipment became commercially viable. Observatory scientists contributed instruments to this pool, and they were licensed to commercial firms for manufacture.

As we saw in Part I, Beals made important contributions to astrophysics through his development of the microphotometer and the intensitometer for the study of spectra. He was sympathetic to instrument development programs and, in the case of astronomical instruments, contributed to them. He was not knowledgeable in electronics; many of the developments in geophysical instruments were beyond his understanding but not beyond his support.

He was not sensitive to the needs of the technical services – the machine shop, the draughting office, the photographer. This was largely because, as Director, he never had to wait for anything. The rest of us would sometimes wait for months for draughting or photographic work necessary to prepare manuscripts for publication. This was not deliberate on his part; the technicians involved always handled his work before anything else.

Much of the photographic work involved the copying of draughted figures, such as maps, sometimes large ones. The only camera available for this was an old one intended for portrait work which had a plate holder that could be tilted in any direction. It was not sufficient to focus the camera on the material to be copied; the plate holder had to be exactly at right angles to the lens axis and this could only be done by eye. Beals kept a powerful magnifying glass on his desk, and when any work came back from the photographer he would go over it, inch by inch; if he could find anything even slightly out of focus, back the work went! Finally Locke and I constituted ourselves a committee of two to look after the photographer. We submitted a budget on his behalf and provided him with modern equipment.

In 1951 Beals was elected a Fellow of the Royal Society of London. The citation mentions nothing about his work as Director:

> "Distinguished for his contributions to observational astrophysics, including particularly the study and interpretation of the broadened emission lines in stars with extended atmospheres and interstellar lines."

It is interesting that his old friend Gerhard Herzberg was elected to Fellowship at the same time. Beals contribution as Director was recognized in 1958, with the award of the Gold Medal of the Professional Institute of the Public Service of Canada

> "for his contribution in raising the Dominion Observatory to the standing of one of the world's leading institutions in the fields of astronomy and geophysics."

In Beals personal files there are a number of letters from Petrie discussing scientific and personal matters. Only half of the exchange is present; there are no copies of Beals responses. A group of these letters, beginning in February 1964, and continuing to February 1965, some eight months after his retirement, deal with Beals' behind the scenes efforts to promote the 150-inch telescope. These will be discussed in the final chapter. There is however a most interesting letter dealing with the future of government astronomy[35]. This is in response to a letter from Beals, which, unfortunately, is missing.

> "The second part of your letter contains the interesting discussion of the future of the astronomical part of the Dominion Observatory. It is quite plain that your present site is quite unsuitable for observational work and that it will continue to deteriorate. It seems to me that a better site must be found so that the fine and powerful equipment at Ottawa [may] be able to produce the high-quality work which it should do rather than languish in a spot where it cannot make a worthwhile contribution to astronomical progress. Another strong reason for a move is the depressing effect a poor site must exert upon the astronomers who see their

training and abilities to some degree wasted. The stimulation and incentive supplied by a first class site are of paramount importance and are in fact essential if we are to continue to maintain our place in world astronomy. When I contemplate the southern face of Kobau at approximately 5000' elevation (a spot one might pick for a solar telescope) and compare it with observational conditions on Carling avenue, the comparison is almost more than one can bear. The answer to your first question then is an emphatic affirmative – the Observatory must move.

I feel that when the move is made it should be to Western Canada. All experience, here, and in the U.S., suggests that generally the observatories should be west of the continental divide. There are some special observations which can be carried on at indifferent sites but generally speaking the instruments should go west. Whether all the astronomers should be moved or whether one should plan an observatory in the west and an astronomical institute in the east is a matter for thought and discussion.

I believe it would be better to amalgamate all government astronomy into one institution even if this means a separation from geophysics. The advantages of bringing together in one institution positional astronomy, astrophysics, solar astronomers and radio astronomers are enormous. The mutual stimulation received from contacts between sections would exert a most beneficial influence – we, in Canada, have suffered from the great distances separating our two observatories.

If the 150" telescope goes forward it will be essential I think to effect some kind of a union. It is not hard to foresee a serious decay at the DAO were it not linked with the new telescope and indeed an integral part of it. No doubt a DO remaining in Ottawa would suffer also from the migration of our best and most energetic people to the large telescope – remember the blow dealt the DO

Dr. Beals at the time of his retirement.

Dr. and Mrs. Beals cut their retirement cake.

when Plaskett, Young, *et al* forsook it for the 72" telescope. I would even go so far as to suggest that the DAO might be moved, lock, stock and barrel to unite with the 150" and the astronomical section of the DO likewise. The plain truth is that we do not yet have enough first-rate astronomers to operate many separate establishments.

The above are my general reactions to your suggestions considering only the scientific aspect and not worrying about administrative problems which can be solved with the necessary good will. I have not discussed your letter with anyone so the opinions are purely personal. In general I feel that we should plan for an expansive future even if it might appear that we are slightly visionary. Canada is a rapidly growing country with a great future and our astronomical plans should be correspondingly ambitious.

Your suggestion of a committee to discuss the future and to advise the Department is a good one and I hope that you will take it up. I feel however that you should preside over this committee, at first at least, in order to give it the benefit of your views and to define properly its problems, and generally to see that it gets off on the right foot. A committee of this sort should do a good deal of work between the meetings, in the form of individual effort, and it should certainly consult outside people for advice and guidance."

The above letter is quoted here, rather than in the final chapter, because it shows that Beals was fully aware of the inadequacy of the Ottawa site and because, had retirement not removed him from a position of responsibility, he was preparing to do something about it. The site was particularly bad for the solar physicists; I'm not sure that they were aware that Beals fully understood their difficulties.

Beals retired at the end of June 1964. This did not mark the end of his scientific life. He continued his work on craters, aided by several grants from the Observatory and from NASA. With the editorial assistance of D.A. Shenstone he produced a two-volume monograph "Science History and Hudson Bay" which was published by the Department[36]. He was a member of the National Advisory Council on Astronomy, the Committee which is proposed, above, in the correspondence with Petrie.

He was extremely careful not to intrude on Observatory affairs after his retirement; he wouldn't even take books from the library or ask for minor assistance from the technical staff without asking for my approval. He was, however, always most helpful with advice and support, particularly in connection with the development of the Mt. Kobau observatory and the associated Institute of Astronomy. When the project was cancelled he was quite devastated. Herzberg[34] quotes from a letter Beals wrote to K.O. Wright "... ever since the decision on the telescope project I have been effectively in a state of shock, almost unable to talk about it or to communicate with others."

Several of the honours Beals received have already been described. To these should be added Honorary Doctorates from Acadia (1948), New Brunswick (1956), Queen's (1960) and Pittsburgh (1963). He was awarded the Leonard Medal of the Meteoritical Society in 1966 "for his outstanding work in the discovery, the physical investigation, and the study of the origin of ancient Canadian meteorite craters." In 1969, only two years after its establishment, he was appointed an Officer of the Order of Canada.

He died on July 2, 1979, at the age of 80.

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2. I have not found an exact date; however a personal letter from McKellar, dated May 1, 1947, says that, within a few days, it is now six months since Beals left Victoria.
3. "Staff changes, January 1 to September 1, 1946"; National Archives of Canada, RG48, hereafter referred to as Observatory Papers. This reference Volume 13, File 1514-4.
4. Notes, dated February, 1964, for a "Dinner Talk at Toronto, Ontario", National Archives of Canada RG30, B32, hereafter referred to as Beals Papers. This reference to Box 4, File IJ.
5. Letter, C.S. Beals to J.M. Wardle, July 10, 1947. Observatory papers, Volume 10, File 1014.
6. Letter, C.S. Beals to Chief Architect, Department of Public Works, January 9, 1948; Observatory Papers, Volume 10, File 1014.
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    J.E. Blanchard, Editor. "Canadian Upper Mantle Report, 1967"; Geological Survey of Canada Paper 67-41, Queen's Printer Ottawa, 1967.
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27. I am here following:

    Peter C. Newman. "Renegade in Power – The Diefenbaker Years"; McClelland and Stewart, Toronto, 1963.

28. Ibid. 217.

29. "Minutes of a Meeting to Discuss Scientific Studies in the Arctic Basin"; Dominion Observatory Administration File 1135-P6, February 7, 1958.

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32. Letter, C.S. Beals to E.W.R. Steacie, September 30, 1953; National Archives of Canada, Observatory Papers, 14, File 1520-2, Part C.

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Dr. J.H. Hodgson, Director of the Dominion Observatories, 1964-1970; Director of the Earth Physics Branch, 1970-1973.

VI – THE HODGSON ADMINISTRATION 1964-1970

Beals' successor as Director was selected through a competition by the Civil Service Commission, to which a number of potential incumbents were invited to apply. Dr. R.M. Petrie, the Director of the Dominion Astrophysical Observatory, was one of those invited. He declined, thus confirming a decision that he had taken some months before[1]. Like Plaskett, two generations earlier, he was unwilling to leave his interesting research career for an administrative job in Ottawa.

His decision was a matter of regret to the Commissioners and they coupled the announcement of my appointment as Director with one transferring the title "Dominion Astronomer" to him. This, with its suggestion of possible administrative entanglements, was alarming to Petrie, and one of my first acts as Director was to assure him that there was no such danger[2].

> "It is my understanding that there is no change whatever in the administrative set-up. You continue to be responsible for the scientific direction of the Dominion Astrophysical Observatory and for the immediate administration of it. I am responsible for the administration of the Branch and, in the course of this, I must provide you with administrative support. I may even, on occasion, have to modify your scientific plans in the light of overall Branch responsibilities. As I understand it this was exactly the relationship between you and Dr. Beals.
>
> This does not mean that the title 'Dominion Astronomer' is a hollow one. When someone is required to speak for the government on astronomical matters or to represent the government on committees or meetings or in similar circumstances, it will be you that they will call on rather than me. Your responsibilities will be scientific rather than administrative, to bring the prestige of your reputation to such gatherings."

At the same time that the title "Dominion Astronomer" was transferred to Petrie the name of the Branch was changed from "Dominion Observatories" to "Observatories Branch". Van Steenburgh said that the one was a consequence of the other[3], although I never understood how this followed.

In Chapter V I described the period of Beals' administration as "a benign era". The years of my Directorship on the contrary were most difficult, with great changes in the size and mandate of the Department and in the approach of the Government to the control and administration of the public service.

We shall consider the Department expansion first. Marc Boyer died in office in 1962 and was succeeded as Deputy Minister by van Steenburgh. Van Steenburgh experimented with headquarters administration; in 1962 he appointed J.P. Drolet Assistant Deputy Minister (ADM) for Research; in 1964 Drolet became ADM for Mines and J.M. Harrison, the Director of the Geological Survey, was named ADM for Research, a position which he occupied until well past our period of interest. In the meantime a new Branch, Marine Sciences, had developed under the Direction of W.M. Cameron, with headquarters in Ottawa and a major oceanographic research Institute in Bedford, N.S.

In 1966 the Department was renamed the Department of Energy, Mines and Resources and was given an extended mandate[4]. "Most significant among the changes [were] (1) the acquisition from the former Department of Northern Affairs and National Resources of water inventory and planning, and the administration of mineral exploration in certain areas under federal jurisdiction, including Hudson Bay and the continental shelves off the Atlantic and Pacific coasts; and (2) the newly-created function of studying, recommending and coordinating federal policies related to energy development." The Department had moved from its quiet scientific cul-de-sac to a position of major, policy-making importance. The Observatories were now a small frog in a greatly enlarged puddle.

Van Steenburgh never acted as Deputy Minister of the new department; he retired in 1966, at the age of 67, and was succeeded by a senior mandarin, Dr. C.M. Isbister. We remained under Isbister's direction until the close of our period of interest.

Now for the other changes. My directorship was a period in which the disciples of the Harvard School of Business Administration increasingly took over the management of the public service. The new methods had been embraced by business, particularly in the United States, where Robert McNamara had brought them from industry to his post of Secretary of Defence. In Canada the Glassco Commission sounded the call: "Many tens of millions of dollars would be yielded annually by modern, scientific, highly developed management techniques."

It was not until the arrival of Trudeau in 1968 that the new methods had their fullest flowering. Gwyn tells the story succinctly[5].

> "It amounted to a new religion, complete with new prophets. ... Hand in hand with the idea went all the shiny new tools, artifacts for rational managers: computers, data banks, and high-speed communication systems; flow charts, flip charts, PERT charts, decision trees; new problem crunching theologies like PPBS (Program, Planning and Budgeting Systems) and MBO (Management by Objectives). Plus the awesome new jargon: 'interface'; 'feedback'; 'optimize'; 'priorize'; 'input'; 'output'.

. . .

> All departments were pulled upon "the Procrustean bed of PBS and MBO. No department had any real objectives by which to manage itself, other than vague ones like 'to do good' or 'to try to do better'. All departments, nevertheless, solemnly invented properly scientific objectives for themselves."

This certainly describes the Department of Energy, Mines and Resources. Life was a never-ending succession of meetings – at the Branch level with Division chiefs and section heads, at the Departmental level with Branch Directors, Assistant Deputy Ministers of various levels and representatives of the rapidly burgeoning Planning Section, where we attempted to define our mission in terms that would satisfy our masters without diminishing the effectiveness of our existing programs. It was a serious game. If one didn't play it effectively programs of great scientific merit might disappear.

The new methods applied also to personnel management. Job descriptions had to be prepared for each position and the incumbent's output was judged annually against these descriptions. Increasingly, merit rather than seniority became the basis for advancement, but management's idea of merit didn't always agree with the employee's. Unions assumed an important position, active in the defence of their members, and Personnel Advisers became indispensable members of Branch administration.

In 1966 another complicating factor was added – the introduction of the government's program of Bilingualism and Biculturalism. Language schools were set up, employees at all levels were encouraged to become bilingual, and quotas were established for different groups. Bilingualism became an additional factor in the job descriptions for many positions.

While these many changes imposed adversarial pressures on staff at all levels, they also called for increased cooperation. The Department recognize a need for special training to ensure this, and in 1967 launched a program known as "Management Grid". This program continued well past our period of interest and did indeed result in an improvement in staff coordination and management effectiveness. At the same time it provided one more complication.

In the first weeks of my Directorship the Government announced its support for the construction of a 150-inch telescope. The concept expanded to include the establishment of an Institute of Astronomy in which all Branch astronomy would be consolidated. While these projects were supported by the government they informed all our planning; when they collapsed under the attack of astronomers from the University of Toronto, they brought down the Branch with them. These events filled my time for six years, and would fill this chapter were they not deferred to a later position in the story.

It was against this background that the final years of the Dominion Observatories were played out.

ADMINISTRATION

Despite the confusion caused by changing format, the figures in the table on page 4 make it quite clear that the rate of staff recruitment changed remarkably at the beginning of my directorate. The same is true of our annual budget, as shown in the table on page 5. I should like to think that the increase is due to my winning ways with Treasury Board, but this is an improbable explanation. Inflation had only a marginal influence on the budget figures; it averaged about 3.5% over the period. The increase simply represent the rapidly expanding scientific program, which will be documented in later chapters. We may be more specific in the case of Victoria: it was due to the development of the Queen Elizabeth II telescope, beginning in 1965; the acquisition of their 48-inch telescope similarly influenced the figures from 1960 to 1962.

Two events in 1966 led to a major reorganization; these were the death of Petrie on April 8, and the decision of Locke to transfer to the NRC. A memorandum[6] survives that shows how far thinking had advanced for the complete reorganization of government astronomy.

> "We have been considering reorganizations made necessary by the resignation of Dr. J.L. Locke and the death of Dr. R.M. Petrie. These reorganizations must be made in the knowledge that in a relatively few years we shall have an Institute of Astronomy in western Canada and that all Canadian Government astronomers will be attached to it. No steps taken now should complicate the arrangements necessary in that Institute."

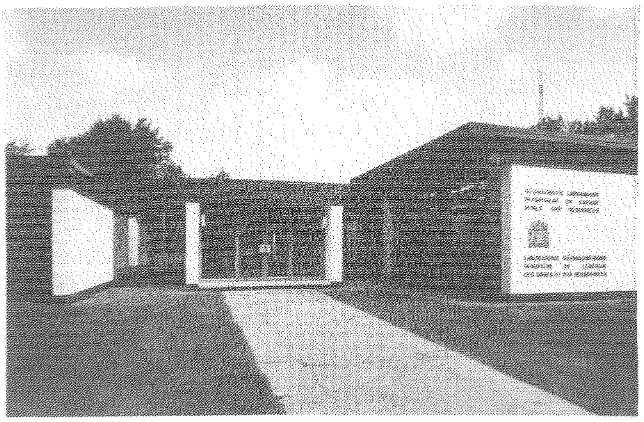
The memorandum recommended that a competition should be held for the position of Director, Dominion Astrophysical Observatory. "The title 'Dominion Astronomer' should not be associated with this competition. This title, which was conferred on Petrie by special action of the Civil Service Commission, should be retained for the Director of the Astronomical Institute when it is established."

The memorandum further recommended that the Dominion Radio Astrophysical Observatory should be given independent status, and that "the remaining members of the Stellar Physics Division be absorbed into the Positional Astronomy Division under the direction of M.M. Thomson ... the name of this Division to be changed to "Division of Astronomy". These suggestions were approved.

The Ottawa Magnetic Laboratory[7]

By 1960 Highway 401 had been built next to the Agincourt Magnetic Observatory and disturbances from neighbouring industries were apparent on the records. Furthermore, there were plans to widen Highway 401 to 12 lanes at that point; the site of the Observatory would be buried under concrete.

Surveys for possible observatory sites within 50 miles of Toronto with a potential of a few decades of undisturbed life turned up nothing, and serious surveys in the vicinity of Ottawa were begun in 1960. In 1962 the Department of Agriculture demanded that we vacate the geomagnetic laboratory on the Prescott Highway in the Experimental Farm; the land was required for a proposed Botanical Garden. In May 1963 the National Capital Commission agreed to reserve 200 acres in the Green Belt on the eastern edge of Ottawa, south of Blackburn Village for a new geomagnetic



The Ottawa Magnetic Laboratory.

laboratory. In October 1963, Treasury Board agreed to the transfer and further agreed to provide additional facilities for palaeomagnetic research, for training staff and for standardizing and testing commercial equipment.

The site was almost ideal, with an area of 1 km square over which the magnetic field was free from local anomalies within 10 gammas – a rare find in eastern Ontario. The neighbours on all sides were the Department of Forestry, conducting long-term tree-growing experiments, which should protect the site from developments producing artificial disturbances.

The plan, developed in cooperation with architects of Public Works was for a large single-storey main building and 15 small non-magnetic buildings scattered widely inside a fenced compound. The main building, containing offices, darkrooms, laboratories and workshops, is not strictly non-magnetic, but such items as steel beams were avoided so that many instruments that will not operate in a building of conventional construction can at least be checked. The 15 small buildings are strictly non-magnetic, with special concrete and hardware. They are spread roughly 100 m apart, so that experiments in one are unlikely to disturb measurements in another.

Four of the small buildings constitute the Ottawa Magnetic Observatory, with provision for standardization and staff training; four are for testing new instruments and techniques of measurement; seven are for palaeomagnetic measurements. Three of the fifteen buildings were designed for the Exploration Physics Division of the GSC and were reserved for their use.

The new premises were occupied in April 1968.

Management Grid

This program had a major effect on the management of the Branch, and is worthy of a detailed description. The thesis on which it was based was that, for effective management, a balance must be struck between total concern for personnel and total concern for production. A graph was proposed in which the former was measured along the horizontal axis on a scale from 0 to 9, the latter along a vertical axis, again on a scale from 0 to 9. Hence the name Management *Grid.* A management style in which concerns for people completely overrode those for production was designated 0,9; a hard-nosed style in which production was everything was designated 9,0. An ideal management style, in which people were used constructively to achieve high production would be 9,9. It became part of Departmental jargon to describe someone as, say, a 0,0 or a 5,5 to the great befuddlement of the non-initiate.

The first stage of the program was to subject as many staff members as possible to "Phase I". This involved an intensive five-day session, closely programmed, in which the individual was made aware of his or her personal management style and its limitations. The final day of the session was devoted to an appraisal of each individual by his fellow course members, often a traumatic experience.

When enough staff members of the organization had completed Phase I, it was ready for Phase II. In this the senior management team met in a remote location, again for five days, and analyzed their organization and each other.

To start the program, all Branch Directors attended a Phase I session in the United States, in which other members of the course were from industry or finance. Few of us showed up well, and even fewer of us recommended that the system be adopted as a Departmental program. Our advice was not accepted and a succession of sessions were held at a hotel in Cornwall to which a large number of junior managers were sent. Again many of the participants found the experience very trying. I participated as a leader in most of these courses because, like Saul on the road to Damascus, I had undergone a sudden conversion, realizing that the reason I hadn't liked the course was that I didn't like to be shaken out of my complacency about my own management style.

A number of Phase II sessions were held, the first involving the management team of the Mines and Geosciences Sector, the second the management team of our Branch. These sessions were difficult; the more senior the management the more entrenched they were in their ways. We decided that we could not proceed with the Branch Phase II sessions to the next level, which would have involved each Division Chief and his senior staff.

Instead we convened a meeting of the entire staff, many of whom had not taken any Grid training. I explained the program and our progress in it to date, and our proposal for the future. This was that each Division should set up a representative group to examine all aspects of the work situation, both those within the Division and those exterior problems that affected Divisional interests, and to make recommendations. Senior management would implement those recommendations that it could, and would discuss with the teams and with the staff as a whole, those recommendations that it could not support.

This arrangement was entered into with enthusiasm and good faith by everyone involved and many changes were brought about. Several of these involved an easing of restrictions on supply and purchasing, others were of more fundamental application to the aims of the Branch. One continuing result was a great improvement in the frankness with which junior personnel could discuss programs and suggest modifications.

One fruitful suggestion, made by K.G. Barr of the Seismology Division, aimed at promoting interdisciplinary studies within the Divisional structure. He suggested that special project groups of up to four research scientists should be relieved of other duties and allowed to pursue some selected interdisciplinary topic for several months; the group would be dissolved when its goals had been attained or had proved unattainable. Subjects, and membership of such groups, should be suggested by the staff, and might include scientists from other Branches. One such group, made up of M.J. Berry and W.R. Jacoby of Seismology, R.A. Stacey of Gravity and E.R. Niblett of Geomagnetism was set up, although with considerably less freedom than Barr had proposed, and produced a most valuable study of the Canadian cordillera[8].

The Associate Committee on Geodesy and Geophysics (ACGG)

The importance of this Committee to Canadian geophysics in general and to the Observatory in particular, has been documented in Chapter V. This importance continued; R.J. Uffen was Chairman for 1964-1967, I for 1967-1971.

One of the highlights of this period was the establishment of the Geodynamics Project; it grew out of the enlarged Upper Mantle Project and was intended to concentrate on "the dynamics and dynamic history of the earth, with emphasis on deep seated foundations of geological phenomena". It was established in 1970, as a joint IUGG, IUGS project. This action had been anticipated by the ACGG early in 1969; it established, jointly with the National Advisory Committee on Research in the Geological Sciences, an "Interdisciplinary " Committee, under the chairmanship of E. Irving. This committee supervised the waning Upper Mantle Project; when the International Geodynamics Project was established, the name of the committee was changed appropriately.

My tenure as Chairman coincided with the decision of the National Research Council to close the Committee. While the story stretches beyond our period of interest, the demise coincides nearly enough with the closing of the Observatory Branch to justify its inclusion here.

By the latter part of the 1960s the sub-committees had developed into discipline oriented, mini-societies, arranging seminars and workshops[9]. In arranging these symposia, the ACGG and its sub-committees were increasingly acting as a national geophysical society. This had two effects. Geophysicists who were not on the committees began to clamour for a better opportunity to participate; they wanted a true society in which they could be active members. The ACGG in going beyond its mandate was putting the NRC in an embarrassing position, at a time when the government's "Management by Objective" program was forcing it to examine all its operations.

Over a period of about eighteen months beginning in October 1969, I met with officers of NRC to explore their position. It was really quite clear[10]. It was time that the national interests of ACGG be channelled into an earth sciences society. NRC would continue financial support for ACGG during the changeover period and would continue to publish the Canadian Geophysical Bulletin, although not necessarily in its existing format. The international responsibilities of the Committee would be vested in a Canadian National Committee for the IUGG (CNC/IUGG) which would be fully supported by NRC. The professional society, or societies, once formed, might nominate representatives to this CNC/IUGG on a basis to be discussed.

Paralleling the negotiations with NRC I was exploring the extent of interest in the formation of the new society. The details of the negotiations have been given elsewhere[11]. The result was the formation of the Canadian Geophysical Union, a Joint Division of the Geological Association of Canada and the Canadian Association of Physicists. Members could join the Union through either parent society and would enjoy full privileges of the selected society as well as of the Union.

The ACGG held its last meeting on February 21, 1974. An informal luncheon, attended by President W.G. Schneider and Professor J.T. Wilson, was held and the Committee was declared dissolved at 12:45 P.M.[12]. The Canadian Geophysical Union held its inaugural meeting, combined with a scientific symposium, the next day.

The NRC has continued to support the CNC/IUGG but the Dominion Observatory took over, and continued, the publication of the Canadian Geophysical Bulletin.

SCIENTIFIC DIRECTION

I became Director without any strong intentions of changing things. As a Division Chief I had influenced Branch direction under Beals and I was, on the whole, satisfied with that direction. There were however three programs that I felt to be unsatisfactory. Details of these will be discussed in the later, technical, sections, and they need only be mentioned here. The first was the Mirror-Transit development of the Positional Astronomy Division; this had been eating up money for many years and seemed to get no nearer to completion. A similar criticism could be made of the Gravity Division's program for the redesign of the Mendenhall pendulums. I gave each Division a year to prove the worth of the programs; at the end of that time the Mirror Transit was dropped, the pendulum project had reached a successful conclusion.

The other area of concern was solar physics. It seemed to me to be using resources quite out of proportion to the results produced. I decided that the program should either be dropped or given a new direction. With the advice and enthusiastic cooperation of V. Gaizauskas, the section head, we followed the latter route.

I mentioned in Chapter V that Beals had not supported the technical sections – draughting, photography, library – adequately. The central administration was also weak in the clerical functions of financial and personnel control. I endeavoured with some success to correct these matters.

If Beals' tenure was marked by an explosion in instrumentation, mine was marked by an even more dramatic explosion in computers[13]. The first extensive use of computers was by the positional astronomers who, in the late 1950s, rented time on an IBM 650 at the University of Ottawa to compute star positions. Other divisions began to develop programs and the demand for computer time burgeoned; in 1962 the first Departmental computer, an IBM 1620, was installed.

From this time on Headquarters was in a constant race to meet the rapidly expanding computing needs of the Department. Much of the pressure came from the Observatories. The Gravity Division computerized its entire operations – the reduction, storage and analysis of data. Penticton converted the observations with its radio telescopes to punch cards which were processed in Ottawa. Airborne magnetometer observations were reduced and analyzed by computer. The list was ever-growing.

In 1965 the system was updated to a CDC 3100, which was selected in part to process the data from the Yellowknife seismic array. This array, which was operated jointly with the British, explored ways of monitoring nuclear tests. Since it ran continuously its data overloaded even the new computer. Clearly a dedicated computer was needed, but this was against government policy, which was for strong central units. With the support of the Department of External Affairs, a Honeywell 124 was installed in the Seismological Division in 1967. This was an important breakthrough. Other dedicated computers followed, one at Penticton for the control and direct analysis of the output of the radio telescope.

By 1970 the use of computers for data storage and general scientific computing was standard in every Division, and in every Branch. The Departmental computer was updated to a CDC 6400.

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2. Letter, J.H. Hodgson to R.M. Petrie, July 20, 1954; Dominion Observatory Administration File 3850.
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7. I am indebted to P.H. Serson for the information given here.
8. M.J. Berry, W.R. Jacoby, E.R. Niblett and R.A. Stacey. "A Review of Geophysical Studies in the Canadian Cordillera"; Canadian Journal of Earth Sciences, 8, 788-801, 1971; Contributions, Earth Physics Branch, No. 340.
9. The Sub-Committee on Exploration Geophysics arranged a "Symposium on Offshore Eastern Canada". It was held in Ottawa, on February 23-24, 1971, and 372 participants registered.

   The Sub-Committee on Geodynamics arranged a "Symposium on the Oceanic Crust", October 26, 1970; nine papers were presented and they were printed in the Publications of the Earth Physics Branch.

   The Seismic Sub-Committee arranged a "Symposium on Seismology in the '70s". It was held in Ottawa, October 27-28, 1970.

   The Sub-Committees on Aeronomy and Meteorology jointly arranged a colloquium on "Atmospheric Dynamics and Thermodynamics Below 100 km and Aeronomic Implications:. It was held in Ottawa, February 24-25, 1971. More than seventy scientists participated.

   The Geomagnetic Sub-Committee arranged a workshop on "Geomagnetic Induction Problems", held in Ottawa, March 2, 1972.
10. Letter, R.S. Rettie to J.H. Hodgson, January 27, 1972; Earth Physics Branch, Administration File 1135-A4.
11. J.H. Hodgson. "The Founding of the Canadian Geophysical Union"; Canadian Geophysical Union Newsletter, 7, No. 1, January 1989; *reprinted in* EOS, 70, No. 27, July 4, 1989.
12. Proceedings of the Sixty-Second Meeting of the Associate Committee on Geodesy and Geophysics, 7.
13. I am indebted to A.J. Wickens for the details given here.

Dr. J.A. Pearce, Director, Dominion Astrophysical Observatory, 1940-1951.

Dr. R.M. Petrie, Director, Dominion Astrophysical Observatory, 1951-1966; Dominion Astronomer 1964-1966.

Dr. K.O. Wright, Director, Dominion Astrophysical Observatory, 1966-1976.

VII – THE DOMINION ASTROPHYSICAL OBSERVATORY

INTRODUCTION

The Dominion Astrophysical Observatory (DAO) had not suffered the trauma throughout the Depression and the War that its parent organization had undergone. It was equipped with a world-class instrument and staffed by dedicated people and its support had never slipped below the critical level that would permit it to maintain its standards.

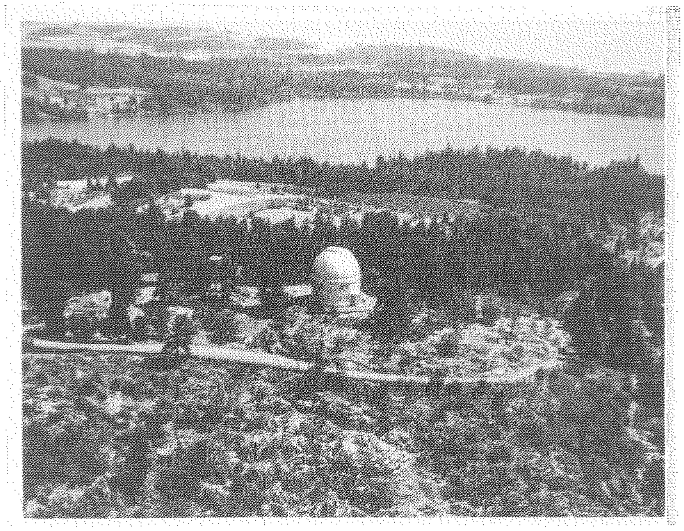
The transfer of Beals to Ottawa must have been viewed as a mixed blessing. On the one hand they now had an understanding friend in high places; on the other they had lost one of their most productive and imaginative scientists and a close friend. There was a general advancement to fill the vacated position, with Petrie becoming the First Assistant. K.O. Wright, who had been occupying a position as an astronomical assistant since his recruitment in 1936, moved in at the bottom of the ladder. Graham Odgers and Anne Underhill joined the staff in 1949. Two years later, in 1951, Pearce stepped down from the senior position in order to concentrate on his scientific work. This was coincidental with the reorganization of the Department as Mines and Technical Surveys, and the return of the title Director which had been lost in 1936. Petrie succeeded Pearce, with the title of Director; McKellar became Assistant Director. During the same year, by Order in Council, the titles of Dominion Astrophysicist and Assistant Dominion Astrophysicist were authorized for the Director and Assistant Director of the Dominion Astrophysical Observatory. Herzberg comments[1]: "One may conjecture that Beals had something to do with this change; in this way his own appointment as Dominion Astronomer would not appear as such an advance over his colleagues in Victoria."

Pearce retired in April 1958, after 34 years in the service. Shortly before his retirement he was awarded an Honorary D.Sc. by the University of British Columbia. He continued to work at the Observatory for several years after retirement. He died in 1988.

Staffing

We saw in the table on page 6 that there was a slow but steady increase in staff up until 1964. In 1965 there was a step increase, followed by a period of rapid growth; this was due to the funding of the Queen Elizabeth II telescope.

Over the years there was a good deal of staff mobility. McKellar was absent during the academic year 1952-1953, serving as a Visiting Professor at the University of Toronto. Jean McDonald was at the University of Toronto from January 1953 to June 1954, "studying the application of the Ferranti electronic computer to the problem of stellar atmospheres"; she also spent the academic year 1956-1957 on the teaching staff of the University of California at Berkeley. She resigned from the Observatory staff in 1960 when she married the Director, R.M. Petrie. Anne Underhill spent the academic year 1955-1956 on the teaching staff of Harvard College Observatory, the winter quarter, 1961, at the Institute of Advanced Studies at Princeton; she resigned from the staff in 1962 to accept a Professorship at the University of Utrecht, the Netherlands. K.O. Wright was a Guest Professor at the University of Toronto during the academic Year 1959-1960.



Aerial view of the Dominion Astrophysical Observatory in 1950. The office building will be seen centre-right of the picture, the Director's house, centre-left. Public Archives of Canada PA-149320.

The Observatory underwent a grievous loss in the death, in May 1960, of Andrew McKellar. He had suffered from leukaemia for more than 15 years but had steadily refused to let his illness interfere with his scientific work. He published more than 70 papers in a working career of 30 years; we shall be reviewing many of these in the following pages. A moving obituary notice was published by his old friend and colleague C.S. Beals[2].

Death dealt a second blow, to the Observatory and to astronomy, with the sudden passing of Petrie on April 8, 1966. We have seen something of his outstanding work in Part I, and shall see much more in the pages that are to follow. He was Director of the Observatory from 1951 until his death. To quote from the obituary notice prepared by Beals[3]: "During his regime the DAO doubled its observing equipment and added many gifted young scientists to its staff. He was able to enlist the interest of the younger staff members not only to his own field of stellar motions but also to other aspects of astronomy such as photoelectric photometry, theoretical calculations of stellar structure and the interpretation of stellar spectra in terms of the ionization and temperature of stellar atmospheres. His own and the Observatory's reputation drew many eminent astronomers to Victoria for work and study."

Petrie was succeeded as Director by K.O. Wright.

Life at the Observatory

In 1968 the Observatory celebrated its fiftieth anniversary. A meeting of the American Astronomical Society was held in Victoria to honour the occasion, and the Journal of the Royal Astronomical Society of Canada published seven papers by astronomers who were, or had been, associated with the Observatory[4]. Two of these papers, by Pearce[5] and Beals[6], give reminiscences of life at the Observatory over their long association with it. In reading these papers one is struck by the close-knit friendliness of the staff and with the pleasure they took in working together. Both authors mention an institution that survived long after their retirements, a putting golf course on the rough terrain of Observatory hill. This had been laid out by Petrie, and everyone played, every noon hour. Beals says: "Dr. Petrie was so enthusiastic about this, and so disappointed if he couldn't have his game, that we all played regardless of the weather. If it was pouring rain it didn't make any difference we were out with umbrellas. ... If the ground was frozen it made no difference – the only thing that would stop us was six inches of snow; then it was almost impossible to play golf." Beals must have been fonder of the game than this passage suggests; one of his first acts on coming to Ottawa was to lay out a small course there. The contagion didn't transfer.

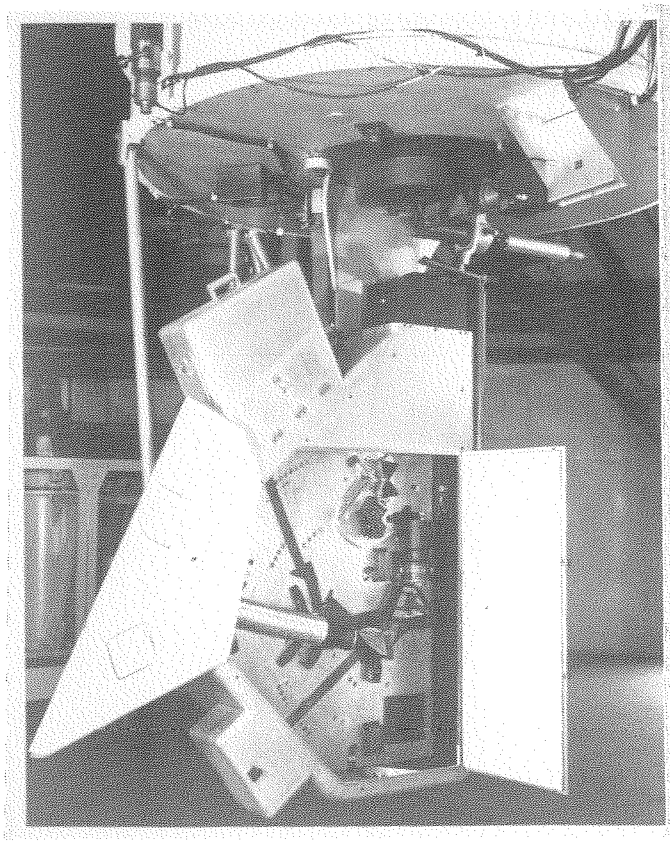
Staff Christmas party in the Director's house, early 1950s.

The daily golf game; the players, from the left, Jean McDonald, Eluned Jones, Andrew McKellar, Joan Jackman and Bert Petrie.

In 1951, W.G. Milne was transferred to Victoria to establish a seismological office and I had many occasions to visit the Observatory. On these visits I was usually included in the golf game; my ineptitude must have imposed a strain on the astronomical experts! Another thing I remember from those days is the excellent seminars; all staff members were expected to participate in these, and visitors were pressed into service. Discussions were lively. The thousandth seminar was held in 1965; Batten memorialized the event in an article in the Journal[7].

BUILDINGS AND EQUIPMENT

Paralleling the increase in staff there were constant improvements in the working facilities. An addition to the office building, with a seismic vault in its basement, was completed



The spectrograph for the 72-inch telescope as it was in the early 1960s.

in 1953, and in 1958 new machine and carpentry shops were constructed, thus clearing space on the main floor of the dome.

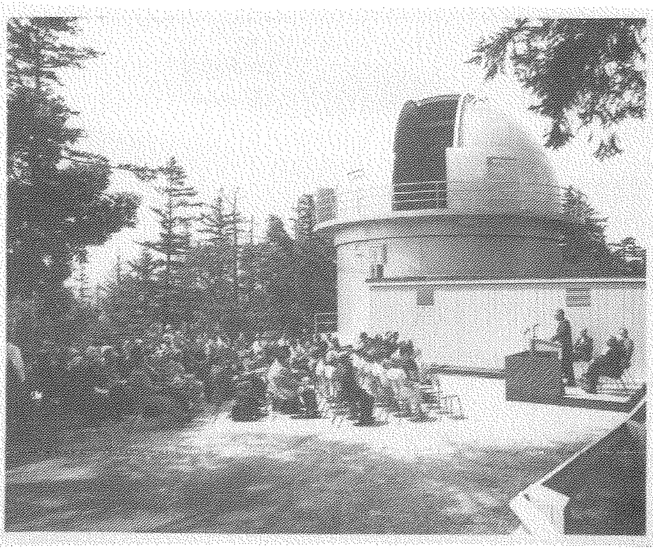
Improvements to the 72-inch Telescope

The telescope and its auxiliary equipment were under constant revision, with the developments of new spectrographs of greatly increased power, and of a new aluminizing plant. P.E. Argyle developed a multi-colour photoelectric photometer which fitted to the Cassegrain focus[8], and cooperated with J.B. Warren of the University of British Columbia in the development of an exposure meter[9] which rapidly became "an indispensable adjunct to efficient spectroscopy". Later Argyle developed an automatic guider that could keep the 72-inch telescope accurately following the motion of a star, even one as faint as the 11th magnitude[10]. He reports modestly: "Our general experience convinces us that the automatic guider is a desirable addition to the spectrograph in that it relieves the astronomer of much of the tedium and fatigue of observing and generally will result in improved quality of spectrograms."

The 48-inch Telescope

For some years pressure had been building for the construction of a second telescope. In September 1957, the order for a 48-inch reflecting telescope was placed with Grubb Parsons, of Newcastle-upon-Tyne, England. The contract for the design of the dome was awarded to A.B. Sanderson and Co. of Victoria and for its construction to the Commonwealth Construction Company of Vancouver. A principal feature of the telescope is its coudé spectrograph. In the coudé arrangement the light beam is reflected through the bearing axis of the telescope, which rotates but does not change position as the telescope moves. Since the spectrograph does not ride the

K.O. Wright at the controls of the 48-inch telescope.



The dedication ceremony for the 48-inch telescope. Van Steenburgh is speaking, the other platform guests are J.F.Heard, Petrie, Beals and Pearce.

Partial view of the coudé spectrograph of the 48-inch telescope. The slit is in the wooden panel to the right; the bright rectangular patch to its left is the grating for the short focal-length camera. The pillars and girder support the camera mirrors and plate holders.

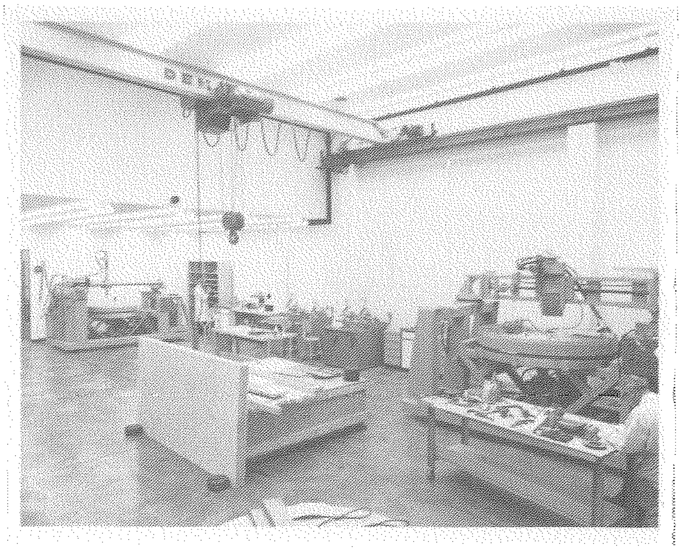
telescope it can be as large and massive as desired, and in the case of the 48-inch telescope the coudé room was an integral part of the dome structure.

The dome and coudé room were completed by the end of 1958, the telescope arrived from England in late October 1961, and the installation was completed by mid-January 1962. The telescope was officially opened at a meeting of the Astronomical Society of the Pacific on June 21, 1962, by which time 150 spectrograms had been obtained which established its excellence.

The coudé spectrograph of the 48-inch telescope bears the name of Andrew McKellar "who lived to see the completion of his unique horizontal coudé spectrograph room and the ordering of the optics"[11]. E.H. Richardson, who had left the Observatory staff some years earlier to take a doctorate in spectroscopy at the University of Toronto, returned to Victoria in September 1959, six months before McKellar's death. He completed the construction of the spectrograph following McKellar's plans, using a large mosaic grating, made up of four separate 6-inch gratings. He then went on to design an image slicer which, using multiple reflections, directs starlight, otherwise wasted, into the spectrograph. Improvements were also made to the telescope. A new, smaller, secondary mirror, was installed, which occulted less of the primary mirror. It and the other mirrors which directed the beam to the spectrograph slit were coated with high-reflectance coating which improved the performance greatly over the original aluminum coating. When all these improvements were made the 48-inch telescope rivalled the 200-inch Palomar telescope in the effectiveness of its coudé spectrograph![12]

Optical Shop

One of the last contributions of the Department of Energy, Mines and Resources to the well-being of the DAO, took place after the Queen Elizabeth project had been cancelled by the Government. This was the provision of funds to build an optical shop on DAO property to house the mirror-figuring equipment purchased for the larger telescope, and to continue the employment of the opticians and machinists. The mirrors and other improvements to the 48-inch telescope mentioned above were made and installed by this staff. Our final contribution was a new mirror blank for the 72-inch telescope, which was figured in the shop.



Partial view of the Optical Shop.

We noted toward the end of Part I that the concept of programs, in which all the staff contributed, had gone out of style and that each astronomer pretty well followed his own interests. As the staff increased, and as the efficiency of equipment grew, this policy expanded. Large numbers of papers were published, on a great variety of topics, too many to be described effectively in a review of this sort. At the risk of some over-simplification I am going to consider the research under three general headings: radial velocities, spectrophotometry and photometry.

THE SCIENTIFIC PROGRAMS:
I – RADIAL VELOCITIES

By 1951 the radial velocities of something well in excess of 15,000 stars had been published worldwide and only faint stars remained to be measured. The work became more difficult for these fainter stars, the accuracy of the measurements decreased. Many observatories, where radial-velocity surveys had been made in the past, reduced or abandoned their radial-velocity programs[13]. This did not happen in Victoria, but the work had ceased to be routine; it was aimed at illuminating specific problems.

The B-Star Program

One such program extended over some twenty-five years, and, at one time or another, involved the willing collaboration of everyone on the staff. This was an extension of the pioneering work of Plaskett and Pearce in determining the dimensions and rotation properties of the galaxy.

The original program had been limited to B stars brighter than magnitude 7.5; this limitation meant that only relatively near stars were included in the study. In 1942 Pearce and Petrie embarked on a program to study all B stars north of the celestial equator and with magnitudes in the range 7.5 to 8.5. This would extend the distance of the study threefold. Pearce was an important contributor to the program, obtaining more plates than any other observer, but it was Petrie who made it his major interest; half his published papers were related, directly or indirectly, to it.

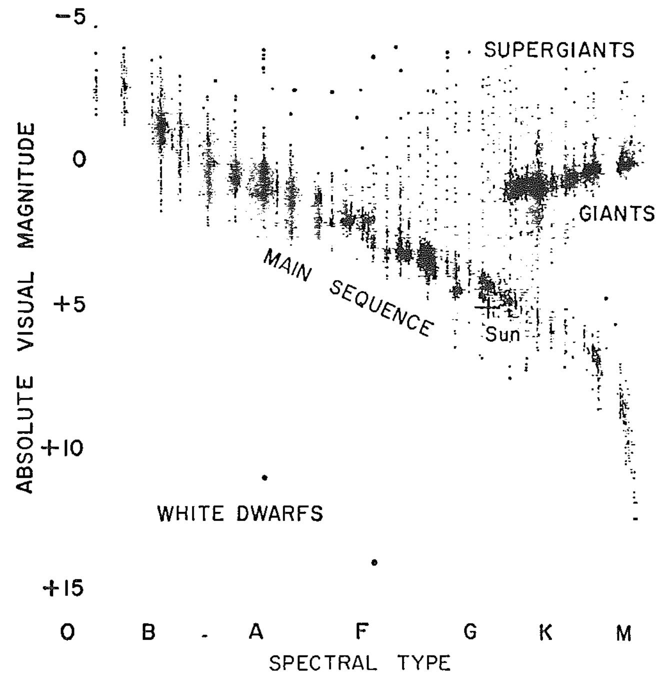
Why was the program limited to B stars? B stars are massive and hot; they are therefore highly luminous, and can be photographed at great distances. Because they are massive they evolve much more quickly than other stars but, as a consequence of this, they do not remain hot for long. It follows then that the B stars are young, less than about 10 million years old; if they were not young they would not be B stars. They therefore present a class of stars, all of about the same age, that can be observed to great distances. Another advantage is that they present simple spectra, so simple that the K line due to interstellar material can be measured accurately.

The Radial Velocities of B Stars

To determine the structure of the galaxy two sets of data are needed, the radial velocities of the stars and their position in space. The first step then was to determine the radial velocities of all stars in the program, that is all stars in the magnitude range 7.5 to 8.5.

Radial velocities are measured as the displacement of certain lines in the stellar spectra from their standard positions; these standard positions must be established precisely. We saw in Part I how Petrie, with the assistance of McDonald and Wright, established them for the different classes of stars with the greatest precision; this work was a necessary precursor to the studies here described.

The program of radial velocity measurements began in 1942 and continued until 1960, by which time the radial velocities of 79 B type stars, studied earlier, had been redetermined[14] and radial velocities for an additional 570 B stars[15], had been measured. Some of the stars showed evidence of variation in radial velocity; was this real or the result of errors in measurement? Petrie discussed the probable errors in radial velocity measurements and concluded that as many as 50% of stars had variable motion[16]. These stars were presumably double or multiple. He reviewed the techniques for determining radial velocities[17] and their accuracies[13] in two papers published shortly after the program was completed.



The spectra of stars form a continuous sequence characterized by the rise and decline in the strength of the hydrogen lines and by the appearance of the lines of other elements at different stages in the sequence. In the Harvard Observatory classification the types of the sequence are known as O, B, A, F, G, K and M; stellar temperatures decrease as we move in the sequence from O to M. When the absolute magnitude, or luminosity, of the stars are plotted against spectral type, as in the above diagram, most of the points fall in a broad diagonal line. These are the "main sequence" stars. Stars unusually luminous for their spectral class fall above the main sequence and are called "giants" or "supergiants". Stars of abnormally low luminosity, "dwarfs", fall below the main sequence. These matters were more fully explained in Part I.

The Determination of Absolute Magnitude

The second requirement of the study was to determine the distances of the stars. This brings us to the question of absolute magnitudes.

The apparent brightness of a star depends on two things, its absolute magnitude or luminosity and its distance. Apparent magnitudes are known for all stars; if the absolute magnitude of a star is known, its distance can be calculated; conversely, if its distance and apparent magnitude are known its absolute magnitude can be determined.

We saw in Part I, that the intensities of certain lines in stellar spectra correlate with the absolute luminosity of the star. Harper and Young had applied the method successfully to determine the absolute magnitudes of many stars in classes F to M, but when Harper tried to extend the work to A stars he found he was unable to estimate line intensities with sufficient accuracy to provide reliable results. Future work had to await the development of more accurate means. The microphotometer and the intensitometer supplied this need.

We also learned in Part I that the determination of stellar distances is a boot-strap operation, proceeding from the known to the unknown. In this case the known was provided by a substantial number of A stars for which distances had been established, either from trigonometric measurements, or because they were associated, in a binary pair or in a cluster, with late-type stars for which spectroscopic distances could be determined by the methods pioneered by Harper and Young. In establishing the absolute magnitude of these stars allowance had to be made for the dimming effect of interstellar matter; this is dependent on photometric measurements. Lack of this information limited the number of stars available for the study but in a first paper[18] Petrie was able to present data on 169 stars.

A stars differ from the later types studied by Harper and Young in that their spectra are relatively simple, with a clearly defined continuum and fewer lines. Petrie found that, instead of studying the ratios of several pairs of lines as had been done in the earlier work, it was sufficient to measure the intensity of one line only – the Hγ line. The measured intensities of the Hγ lines were plotted against the absolute magnitudes to produce a curve from which the absolute magnitudes of any star in the range studied, B8 to A3, could be determined given the intensity of its Hγ line. Since the microphotometer tracing could be made from any plate in the Observatory files for which a proper calibration spectrum had been recorded (post 1935), the absolute magnitudes of a large number of A stars could now be routinely determined. A group of stars known as white dwarfs did not fit the curve and were excluded from its application.

Petrie now turned his attention to the B stars, encouraged by papers by Underhill[19] and McDonald[20] which suggested that the Hγ line should continue to be an effective gauge of absolute magnitude for the B stars, although it might be less useful in O stars. In selecting stars of known parallax and photometric properties as his stepping stones to the unknown, he was aided by the fact that the earlier work allowed him to obtain a distance for any binary system or cluster which contained an A star. Petrie applied his method to 99 stars in the range O9 to B7 and established a relationship between absolute magnitude and the equivalent width of the Hγ line for stars in classes O9 to B7[21].

Armed with this relationship he could proceed to the unknown. In a first paper[22] he provided a catalogue of 124 stars of the class, and discussed the fact that the Victoria magnitudes were about one magnitude fainter than those determined at the Yerkes Observatory by photometric methods. By 1956 he had determined spectroscopic absolute magnitudes for about 400 B stars[23]. He compared the spectroscopic distances against values obtained in other ways and found a satisfactory agreement. A number of tests of internal consistency were devised, treating dwarf, main sequence and giant stars separately, and the results seemed to establish the validity of the spectrographic method. The Victoria magnitudes still differed from the Yerkes ones by a full magnitude but, in a footnote, Petrie mentions that the Yerkes values were being revised and that the two sets had been brought within 0.4 magnitudes. A further re-examination of the data[24] led to the discovery that one star stream, long regarded as a cluster, was not one, and that the assumption of cluster motion had led to errors in the apparent magnitude of its member stars, both from photometric and spectrographic determinations. This discovery permitted a correction to be devised which eliminated the earlier discrepancies.

Nine years later Petrie returned to the subject[25]. In the interval the absolute luminosity of many stars in galactic clusters had been accurately determined by photometric methods. Using these stars as a basis, Petrie developed a new relationship between the Hγ line intensity and absolute luminosity. He showed that his methods were insensitive to errors introduced by evolutionary processes in the star or by stellar rotation, and found that, by applying corrections for the different spectral types, he could match the photometric magnitudes closely.

A year later, in a paper released after Petrie's death[26], Petrie and Lee published equivalent widths of Hγ, and absolute magnitudes, for 571 faint B stars, as well as the equivalent width of the interstellar K line.

The large quantity of homogeneous data provided by Petrie's work tempted Walker and Hodge[27] to seek out other features in the spectra likely to be physically significant for the B stars, or for interstellar material. They selected two stellar absorption lines of Helium and one interstellar line, and tabulated the equivalent widths of these lines. Using a number of stars for which the rotational velocity was known, they established a relationship between stellar line-width and rotational velocity. With this they established the rotational velocity of 450 O to B5 stars.

The Parameters of the Galaxy

In the paper by Petrie and Lee[26] published after Petrie's death, they announced: "the absolute magnitudes given in this paper, together with the radial velocities already published, complete the observational material obtained to date from the spectroscopic observation of faint B stars. The analysis of these data has begun, and it is expected that galactic rotation parameters will soon be obtained from the stellar and interstellar velocities." The paper was Petrie's hundredth, and last; he did not live to finish the work that had occupied so much of his life.

Much of the analysis had been completed. In 1963 Petrie[28] analyzed the data then available for peculiar or random motions of the stars, that is, motions remaining after corrections have been made for solar motion and galactic rotation. He analyzed these motions in terms of their distance from the galactic centre and for different absolute magnitudes. He found no evidence of variation of peculiar motion with distance, but found some suggestion of greater mobility along a galactic radius than in a perpendicular direction. There seemed also to be a suggestion that the motions increased with increasing luminosity. He returned to this subject in a later, review paper, his Presidential Address to the Astronomical Society of the Pacific[29].

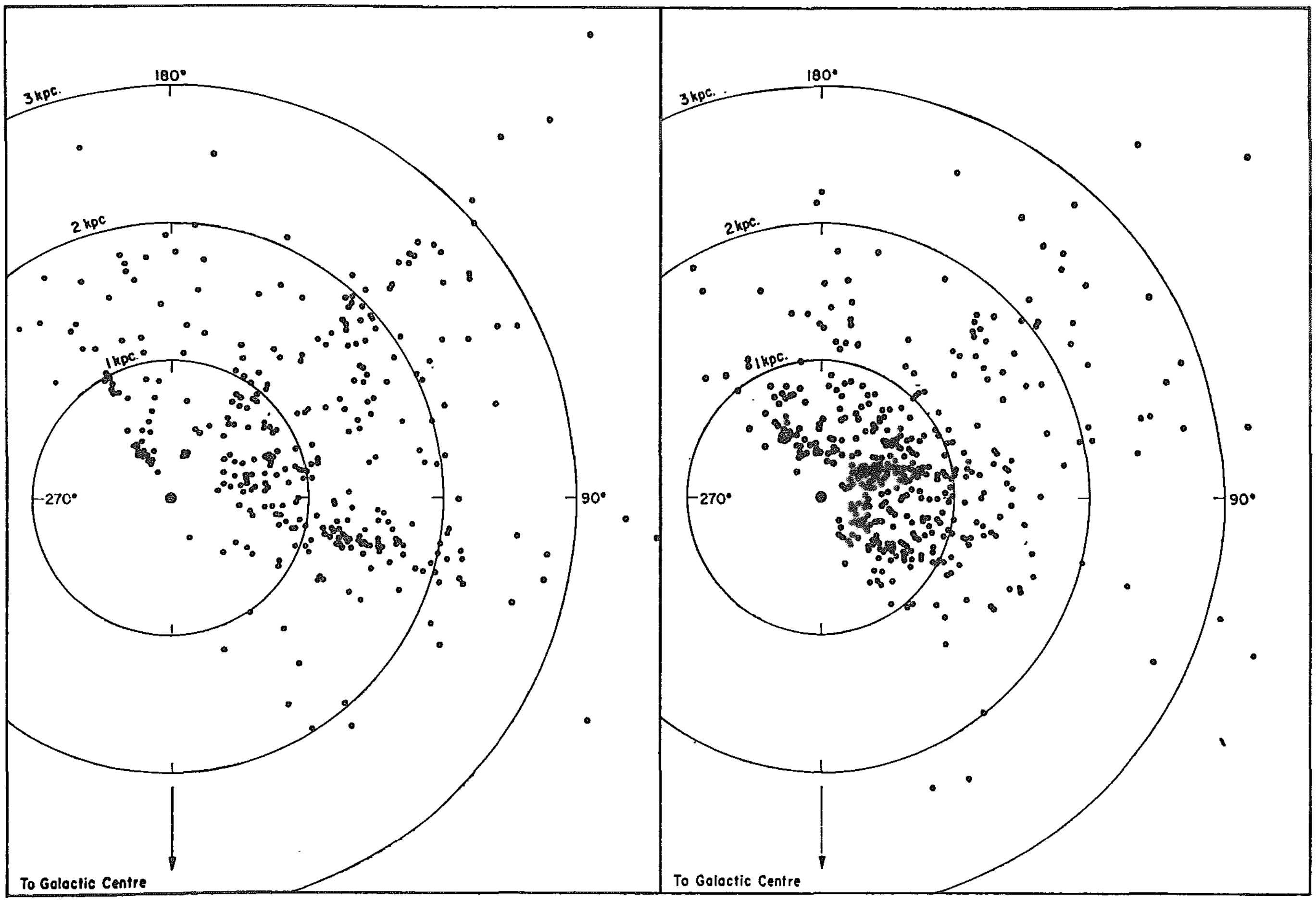
The final analysis of the data was completed by his widow, Jean McDonald Petrie[30]. Most of the B stars lie in, or near to, the galactic plane; the figure on the following page shows their projection on this plane, and gives a reasonable understanding of their distribution. Values of the galactic rotation parameters were obtained for 688 stars, ranging in class from O9 to B8, and for interstellar matter based on the spectra of 286 stars of classes O9 to B2. In both cases the K constant was sensibly zero, removing one of the puzzles remaining in the earlier work of Plaskett and Pearce.

In November 1969 York University opened its new Science Building which was named after Robert Petrie; Jean Petrie gave the inaugural address[31]. She traced the history of the B-star program and her late husband's part in it, and concluded with a quotation from one of his last papers. "The Victoria programs have made some advances in the methods of measuring radial velocities and absolute magnitudes of B stars and in investigating interstellar material. They have contributed a large volume of numerical data but these, by themselves, are not enough properly to explore the nearer parts of the galaxy. We must look forward to large additions so that we may in time exploit the advantages that continued observations of B stars offer."

Clusters

Petrie had used the stars in clusters as stepping stones in his measurement of the distances of the B stars. The discovery, noted above, that one star stream, long regarded as a cluster, was not one made all the conclusions based on clusters suspect. This led to an extensive study of clusters.

Baker[32] defines clusters as "assemblages of stars having their members less widely separated than are the stars around them. They are not temporary congestions in the celestial



traffic; the stars in each group are moving together, so that the clusters maintain their identities for a long time. Because the members of a cluster are all practically at the same distance from us, they may be compared fairly one with another."

One class of clusters, galactic clusters, are so called because they lie in, or close to, the galactic plane. The stars are moving on parallel tracks which, through perspective, appear to converge at a point. The clusters are so close to us that the individual stars can be distinguished and in many cases the proper motions of individual stars can be measured. When this is possible one can project the trajectories of the stars backwards in time to define the convergent point. Conversely, given this convergent point, the distance of any star for which the proper motion is known may be determined.

Work on the first two clusters studied, Pearce's on the Pleiades cluster, and Stillwell's on the Taurus cluster, were described in Part I. The next study was by Petrie[33] who considered the Ursa Major cluster. This cluster had originally been supposed to contain at least 100 stars, but Petrie showed that the radial velocities of the principal stars were not in accordance with the normally accepted convergent point; by treating the eleven principal stars as a separate group, he was able to establish a new convergent point. There was a strong implication that the remaining stars were not part of the cluster. In a later paper[34] this convergent point was refined, and its motion relative to the sun was established.

In a later pair of papers Petrie[35] showed that a supposed group in the region of Cassiopeia-Taurus was not in fact a group, but was part of the general population, and drew similar conclusions about a stream in Scorpio-Centaurus[36]. These studies reduced the numbers of stars available for studies of absolute magnitude, but increased the confidence that could be placed on those that remained.

Petrie and J.F. Heard, of the David Dunlap Observatory, joined forces and combined data from the two observatories on the alpha Persei cluster. They concluded that 13 stars, normally believed to be members of the cluster, were not. They measured the luminosity of the surviving stars, measured the cluster velocity, and noted the small number of double stars in the cluster membership[37]. The data on which these conclusions were based are given in a separate paper[38].

Stellar Associations

This will be a suitable place to mention work done on stellar associations. Associations are not clusters, but are groupings of similar stars that inhabit a relatively small volume of space, so that they are believed to be expanding from a common origin. Walker[39] studied one association, the Cepheus IV association, describing the spectra in detail and determining the mean distance of the association. All of the stars show heavy reddening, indicating absorption by interstellar material, and Walker suggested that other stars may exist in the association, completely hidden by this interstellar material.

A somewhat related paper by Crampton and Byl[40] studied two ring-like apparent associations of stars in Orion and in Aquila. They concluded that the Orion ring is part of a larger association, and is not distinguishable from it, and that the Aquila ring is an accidental configuration of stars at a variety of distances.

Binary Stars

Spectroscopic binary stars are recognized when the variations in radial velocities exceed the probable errors of the measurements. We have already noted Petrie's study[16] of the probable errors in a large number of radial velocity measurements covering all stellar classes in which he concluded that about 50% of stars, the percentage varying slightly with class, had variable radial velocity. Clearly there is a large amount of work to be done.

Between 1947 and 1970, the period of our interest, 52 papers were published giving the orbits of binary systems; this figure does not include 26 papers dealing with a particular class of binaries known as the Zeta Aurigae stars; this work will be treated later.

Most of the papers were reconsiderations of stars studied earlier in which the original work seemed deficient for some reason, or because it suggested some peculiarities which could not be resolved with then-existing equipment. Still others were of stars in which some variation, of luminosity or orbit, had been suggested. Toward the end of the period here reviewed, A.H. Batten[41] compiled a catalogue of all spectroscopic binaries with known orbits. There were 737 systems listed; 60 stars, formerly believed to be binary were rejected because of lack of reliability in the data.

On the basis of this catalogue Batten published a number of papers dealing with the statistics of double stars[42], with their masses[43], and with the orientation of their orbital planes[44]. One particularly interesting conclusion was that about 35% of so-called binary systems are, in fact, triple stars, and that some 30% of these are at least quadruple[45]. He summarised much of this work in a paper read at the meeting of the American Astronomical Society held in Victoria to mark the 50th anniversary of the Dominion Astrophysical Observatory[46].

McKellar studied an interesting double star, RY Geminorum, in 1949[47]. It is an eclipsing binary, consisting of an A2 star and a K2 star; the A2 is surrounded by a ring, the emission spectrum of which can be seen at full eclipse. McKellar estimated that the radius of the ring was about three times that of the star it surrounded and that the rate of rotation of the ring was about 1/5 that of the system.

We saw in Part I that, even when the two components of a binary star are of nearly the same brightness, so that the spectrum of each appears, it is still not possible to determine all the properties of the system; the masses and axes of the orbit are expressed in terms of the inclination of the orbit. In the late 1930s Petrie[48] developed a method for overcoming this limitation. When two such stars revolve about a common centre their spectra are mixed in a way that changes throughout the cycle; the intensity profiles of the spectral lines vary in a systematic way, and Petrie was able to relate this to magnitude difference between the two stars and their spectral types. Since the magnitude of the combination is known, the magnitudes of each component can be calculated. This is of course apparent magnitude. However, for those binary stars for which the distance or parallax is known, apparent magnitude can be translated into true magnitude or luminosity.

The masses of the two components are still not known, but their ratio is, and this value, combined with the magnitude difference, is sufficient to define the slope of the mass-luminosity curve. Using many stars, the best value of this slope may be determined; the actual curve can be fixed by the accurate mass and luminosity of one star.

From the time that Petrie developed the method he collected observations on stars with highly reliable observations on magnitudes differences. Eleven years later he had assembled data on 82 such binaries[49], and he proceeded to analyze these to determine the mass-luminosity curve[50]. The curve agrees remarkably well with those established by other research based on completely unrelated data.

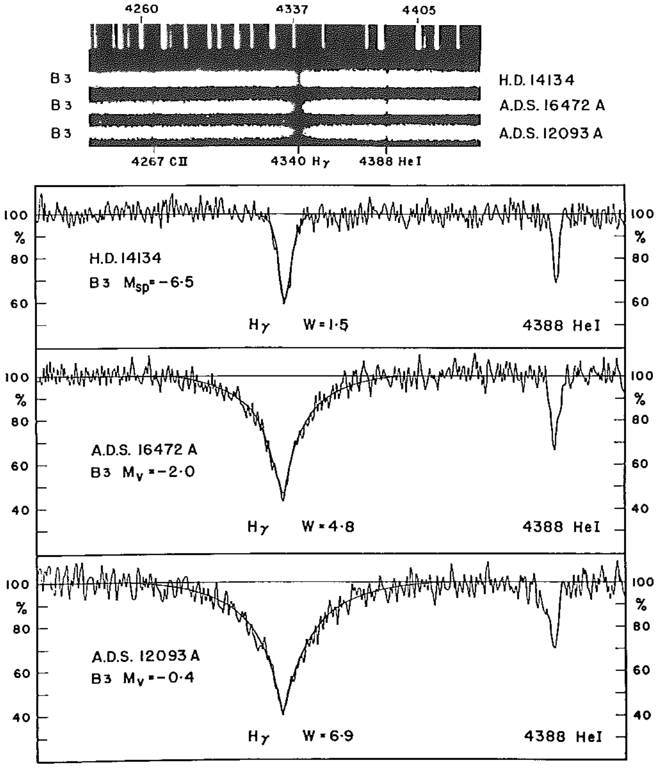
It should be noted that orbits of binary stars began to be determined by computer. The first reference I have found is in a 1965 paper by F. Gutmann[51], in which the author expresses his indebtedness "to S.C. Morris for computing the spectroscopic orbit with the IBM 1620 computer at the University of Victoria."

The National Science Foundation of the United States sponsored a conference on Binary Stars at the Dominion Astrophysical Observatory in August 1956. The proceedings of the conference were published in the Journal of the Royal Astronomical Society of Canada[52].

THE SCIENTIFIC PROGRAMS: II – SPECTROPHOTOMETRY

We saw in Volume I that the study of line profiles through the microphotometer and intensitometer, a technique known as spectrophotometry, changed the nature of astrophysical work. The figure on the following page[53] reproduces sections of the spectra of three stars, all of class B3, and the intensitometer tracing of each. We note first that the wave-length coordinate has been expanded so that the lines have become broadened to form "wings". The shapes of these wings are characteristic of the stars, and related not only to their class but also to there speed of rotation, their magnetic fields and to any expanding clouds of gas that might surround them, so that the tracings for stars of the same class can vary widely. The intensity is determined by measuring the area under the intensitometer curve with a planimeter.

Astronomers speak of the intensities of both absorption and emission lines, and express intensity in terms of equivalent width measured in angstroms (Å). Imagine a line in which the stellar radiation is completely absorbed between two nearby wave-lengths, γ and $\gamma + \Delta\gamma$, and not at all outside these



limits. Then a real spectral line at this wave-length that subtracts the same amount of energy from the spectrum is said to have equivalent width Δγ.

The O Star Program

It is something of a misnomer to speak of an O star program; the O stars were never studied in such a wide-ranging way as the B stars, but because of their high temperatures they did provide a laboratory for the study of nuclear phenomena beyond the range of laboratory physics.

Petrie[54] made a thorough study of O stars toward the close of the Stewart administration. Making use of the microphotometer, he determined the equivalent widths of all the prominent lines in 20 O stars, and showed that the ratios HeI/HeII, HeII/H and SiIV/HeII all varied in a systematic way over the range of star types O5 to B0. Each of these ratios could be used to define the stellar classification and their average accomplished this accurately. He was less successful in relating line intensity to absolute magnitude; there was no trustworthy correlation. He was able, however, to use the ratio of the two helium intensities to measure the temperature of the stars. These ranged from 36,000°K for O5 to 28,600° for B0.

From this point on, the study of O stars was almost the exclusive domain of Underhill. Her earlier theoretical work[19] had suggested that the Hγ line would not provide a useful correlation with luminosity, and she studied a number of other hydrogen lines in the spectra of O stars of established parallax without finding a satisfactory correlation. She suggested, tentatively, that all early O stars might have the same absolute magnitudes[55].

Her next approach was to propose two models[56] for the atmospheres of early type stars, each with its own surface temperature and surface gravity; one of these models approximated to an O9.5 star, the other to an O5. She investigated the properties of these models[57], particularly the intensity of the Hγ and HeII lines. She found that the intensity of Hγ could not be used to distinguish between her two models and, hence was unlikely to be useful as a diagnostic tool in the study of O stars. The intensity of the HeII line was shown to depend on the relative abundance of H and He, and was estimated for a variety of values of H/He. Checking against the measured equivalent width in a number of O stars she concluded that the H/He ratio lay in the range 20-25. In a later paper[58] she compared this result with values found by other methods, and found good agreement.

As a further test of her models, Underhill measured the relative strengths of the H and He lines in 39 O stars, combining her observations with those for the 20 stars already measured by Petrie[54]. Her observations confirmed the good correlation between H, HeI and HeII and stellar types in the range O5 to B0, but there were discrepancies with her model stars that suggested a number of modifications[59].

A major study of an O7 star followed. Twenty-one plates, taken over the seven year period 1950-1957, were studied carefully for the wave-lengths[60] and intensities[61] of all the lines that could be observed. These totalled 1256, and all but 335 of these were identified. One of these lines, an H emission line, showed a marked variation in intensity over the seven years. She postulated that the star is surrounded by a shell of moderate extent, like the B-type shell stars to be discussed below, which is mildly unstable and is ejecting material at a slow rate.

In her observations of the O stars, Underhill observed various unexpected lines: a CIII emission line[62] which had previously been observed in some Wolf-Rayet stars and which was difficult to account for in nuclear physical terms[63]; and a possible absorption line of Boron, BIII[64].

Spectrophotometric Standards

We saw the great amount of effort that went into establishing standards for radial velocity measurements for stars of different classes. The need for an equivalent system of line-intensity standards, which would permit the intercomparison of results from different observatories, had long been recognized and in 1948 the IAU established a Sub-Commission, chaired by K.O. Wright, to study this question.

With the encouragement of this Sub-Commission, and with the assistance of E.K. Lee and T.V. Jacobson, Wright embarked on an extensive program, in collaboration with J.L. Greenstein of the California Institute of Technology. He selected twelve representative stars covering spectral classes B to G, and made intensity tracings of their spectra as obtained on a variety of spectrographs. He spent six weeks as a Research Associate of the Mount Wilson and Palomar Observatories making tracings and measurements of their spectrograms of the same stars. He then measured the equivalent widths of all these tracings, 1790 in all. All of the measurements and many of the tracings were issued in a major publication in 1964[65].

The study showed that measurements at each of the observatories was remarkably consistent, within 5%, but that they differed systematically from each other.

With the cooperation of the members of the IAU Sub-Commission, comparisons were made with spectrograms for other observatories. It was concluded that consistent results could only be obtained at high dispersion or, for lower dispersion, for lines with equivalent width greater than 100 mÅ. With this proviso the results of the study were recommended to the Union.

Late-Type Stars

There are a number of complications about late-type stars that must be understood before we begin to discuss the work done at Victoria concerning them. The first is illustrated in the spectrum-luminosity diagram shown on page 29. Beginning at about class G there is a band made up of high-luminosity stars called giants, there are some stars of still greater luminosity called supergiants, and some stars with luminosity between the giants and the main sequence stars. Clearly, knowing the spectral class of a late-type star is not sufficient to place it in the stellar sequence. A luminosity classification is needed as well.

Five classes of luminosity are recognized:

Class I – Supergiants
Class II – Bright giants
Class III – Giants
Class IV – Subgiants
Class V – Dwarfs (or main sequence stars).

There is an additional, chemical, difference in the giant stars. Most of them, of classes K to M, are characterized by absorption bands of TiO, but there are a few stars showing bands of molecular carbon and of certain carbon compounds. Carbon stars are placed in a separate spectral sequence, R to N, paralleling classes K to M. In addition there are a few giant stars, otherwise equivalent to the M spectral class, that are characterized by absorption bands of ZrO. These are called S stars.

We may repeat the information on the temperatures of the different classes from Part I:

O5	50,000°K
B0	25,000
B5	15,600
A0	11,000
A5	8,700
F0	7,600
F5	6,600
G0	6,000
G5	5,520
K0	5,120
K5	4,400
M0	3,600.

For early stars the temperature is so high that only spectral lines associated with high levels of ionization are found; there are no lines corresponding to the lowest state of the elements, nor to molecules or compounds. At the late end of the scale, where the temperatures are low, all of these lines can exist. The spectrum is a jumble, with lines superimposed on each other so that it is difficult or impossible to determine the shape of the line profiles or the intensity of the lines, or the general level of the continuum. Because of these complications not much work has been done on the standard lines of the late-type stars, and some well-developed lines have yet to be identified[66].

However, in 1966 Wright and Jacobson[67] returned to the investigation begun years before by Harper and Young of the value of line-ratios as suitable parameters of intensity in late-type stars. By this time the luminosity of a large number of these stars had been measured by photometric methods. They used the same spectral lines as Harper and Young had done, but used the microphotometer to measure the intensities rather than depending on visual comparisons. Rather than depending on absolute intensities, they measured the ratios of their luminosity-sensitive lines to those neighbouring lines most independent of luminosity. In this way they hoped to eliminate errors due to the indeterminacy of the base line and the effects of interstellar absorption. They substantiated the early work but found that the use of the microphotometer did not produce better results than the eye estimates had done.

Molecular Spectroscopy

If the complexity of the low-temperature spectra proved discouraging to other observers, they provided a splendid opportunity for McKellar, with his interest in molecular

Dr. Andrew McKellar, 1910-1960.

spectroscopy. We learned in Part I something about how he identified molecular lines with interstellar matter, a work of outstanding importance[68].

Between the time that work was done and his death in 1960, McKellar published, alone or with co-authors, eleven research papers[69] plus two or more review papers[70] dealing with the molecular spectroscopy of late type stars. In these papers he examines the spectra particularly of the carbon stars, types R – N, seeking to explain their characteristics in terms of lines deriving from molecules or free radicals. Some of his papers make an attempt to identify particular lines or bands reported by other observers, which had piqued his curiosity.

McKellar's last paper, published posthumously, was a review – "Isotopes in Stellar Atmospheres". The Editor, J.L. Greenstein, appends this footnote: "The editor records with regret the premature death of Dr. Andrew McKellar on May 6, 1960. His personality was notable for kindness and co-operation with others. His scientific work will be remembered for precision and imagination".

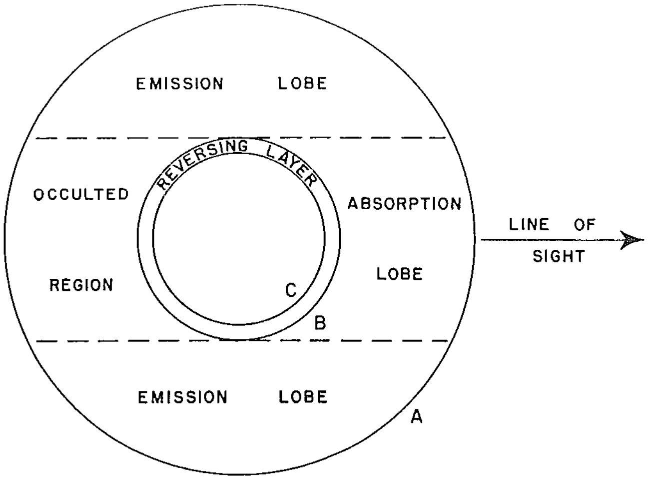
McKellar's work on the carbon stars was continued after his death by two Japanese astronomers. The first of these, Y. Fujita, came to Victoria in 1960 to work with McKellar and arrived just in time to attend his funeral. Fujita studied one carbon star exhaustively[71], reporting on the position and intensity of all lines and identifying a large number of them; his work was based on plates taken by McKellar, and was completed on his return to Japan with the cooperation of several of his Japanese colleagues. Y. Yamashita, a post-doctoral fellow for the years 1964-1966, again with the assistance of Japanese colleagues, compared the spectra of a number of late stars and attempted to rationalize his findings with the standard classification of carbon stars[72].

Emission Line Stars

Many stars of type B and earlier appear to be surrounded by an extensive envelope of gas at low pressure, lying above the usual atmospheric layers. The envelope is inferred from the presence of emission lines in the stellar spectrum and, if there is much material in the envelope, by sharp and deep absorption lines corresponding to lower degrees of excitation than that of the underlying stellar spectrum.

The figure below[73] presents a simplified model of a star and its extended atmosphere. The surface C represents the photosphere of the star, which is the source of continuous black-body radiation. The surface B represents the outer layer of the star proper, called the reversing layer in the diagram, which we may think of as the stellar atmosphere; it is in this atmosphere that the absorption lines are developed. The surface A represents the outer limit of observable material, the limit of the extended atmosphere.

The extended atmosphere is ionized by radiation from the star and itself radiates in certain wave-lengths. When we look right through the atmosphere, in those areas called emission lobes in the diagram, we see this radiation as emission lines. The star itself is hotter than the atmosphere, and those regions of the atmosphere through which the starlight must pass to reach us, called the absorption lobe in the diagram, absorb just the same wave-lengths that they are emitting. The total amount absorbed and emitted must be approximately the same but the absorption is of light travelling directly at us while the emission takes place in all directions. The net result is that we see a combined absorption and emission profile whose precise profile depends on whether the atmosphere is rotating or expanding.



In a review[74] of early type emission-line objects Beals lists the following types:

i	Gaseous Nebulae
ii	Novae
iii	Wolf-Rayet stars
iv	P Cygni stars
v	Be stars

The last three of the above categories fall into the general subject of this review – they are all of type B or earlier, and they all have extended atmospheres. Objects of all of these classes, particularly of the middle three, had been studied by Beals over many years; during his first year in Ottawa he found time to summarize his studies in a major paper[75]. The work itself has been covered in Part I. In the following paragraphs we review later work.

Novae

A nova was discovered in the constellation Hercules in February 1963. It was observed on every clear night, on both the 72 and 48 inch telescopes, until early April, and then less intensively until October; all members of the staff cooperated in the observing program.

It will be recalled (Part I, 147-149) that in studying Nova Lacertae 1936 Beals had been able to determine the distance of the star from the intensity of the interstellar H and K lines. This method was used to determine the distance of Nova Herculis 1963 – 500 parsecs. Batten[76] summarized the radial velocity measurements made from plates from the 72-inch telescope and determined the maximum absolute visual magnitude as -7.8.

In July 1967 the 12th magnitude blue star Nova Delphini began an outburst that saw it rise to magnitude 5 by December 1967. J.H. Hutchings took spectrograms of the nova at regular intervals over an eleven month period. These[77] showed a typical P Cygni spectrum, the analysis of which suggests that the shell thrown out by the outburst attained a maximum diameter in excess of 10^9 km and that the ejection of material ended after about two months.

The next event, Nova Vulpeculae 1968, No. 1, was discovered in mid-April 1968; it had a peak visual magnitude of 4.5. It was observed at regular intervals until late July, at which time its magnitude had dropped to 9.5; as usual, several staff members took part in the program. Hutchings[78] analyzed the spectra, both for radial velocity and for the line profiles. He suggested that the outburst consisted of the ejection of two main shells from the extended photosphere; they accelerated away from the photosphere, which collapsed and heated up after maximum light.

Wolf-Rayet Stars

Underhill made two intensive studies of Wolf-Rayet stars. In the first[79] she analyzed the spectra of two such stars in great detail, measuring and identifying a large number of emission lines in each case, and interpreting the intensity profiles in terms of motions of the emitting gases. In one case there was some evidence of two different emitting velocity fields.

Four years later she studied four more Wolf-Rayet stars[80], describing the peculiarities of their spectra and interpreting these in terms of motion in the stellar atmospheres and the levels of excitation in the different stars. It is interesting that one of the stars studied was a binary star, the companion of the Wolf-Rayet being a shell star, a class to which we will come shortly. In another of the stars studied she found that the shell was expanding at a rate of 3000 km/sec, which suggests a substantial mass loss.

D. Crampton and some associates[81] observed a Wolf-Rayet star that had shown variable magnitude over a period of some seventy years. A study of the spectral lines led them to suggest that the star might be surrounded by several condensations of matter moving away from the star at high velocity. They speculated that they might be observing the formation of a small nebula.

Shell Stars

In 1938 the spectrum of Pleone, a bright B star in the Pleiades, underwent a sudden change, with the development of emission lines of hydrogen and drastic modification in its absorption lines. This was attributed to a ring of gas which had developed about the star; a number of other stars with similar spectra were known, although the development of the ring had not been observed. These stars were known as shell stars. All seem to be binary stars, the shell being associated with the earlier type star in each case.

Underhill studied three shell stars[82], making use of old plates to study variations with time. Two of the underlying stars were, conventionally, of class B; one was an A5 giant; all were rotating rapidly. Two showed variation over a period of several years, the third did not. These variations, and the unusual appearance of the hydrogen lines, led her to suggest[83] the existence of tenuous streamers, moving at great speeds and random directions far above the star and its shell.

In 1957 Wright[84] studied another shell star, 17 Leporis, which had been under observation at various observatories since 1932; these observations suggested that major outbursts of the star appear at fairly regular intervals with a mean interval of about 150 days. Wright's series of observations was too short to confirm this, but he was able to identify the part of the cycle to which they belonged. He also verified the velocity of the outbursts found by earlier investigators.

Stellar Atmospheres

The study of emission line stars is concerned with the extended atmosphere of very hot stars. What about the atmosphere itself? In a sense, all studies of stellar spectra are studies of the stellar atmospheres, for it is the atmosphere which, either by absorption or emission, gives rise to the observed lines.

One way to study the stellar atmospheres is through the curve of growth method. Sets of curves are drawn relating the abundance of various atoms in a stellar atmosphere of selected pressures and temperatures to spectral line intensities. By comparing these curves with the observed line intensities the appropriate values of pressure, temperature and atomic abundance can be selected.

Wright's doctoral dissertation dealt with curve of growth studies of the sun and of three other solar-type stars; later he applied the method to an F5 supergiant and to F stars in general. These papers were discussed in Part I. His interest in the method continued, and he published a number of papers[85] relating to the spectra, and inferred atmospheric structure, of stars in the classes A through K.

Another way to study stellar atmospheres theoretically is through models. We have seen how Underhill developed two models for O stars, one corresponding to an O9.5 the other to an O5. In a series of papers published between 1960 and 1963[86] she developed the necessary computer techniques and investigated a variety of models, representing stars of various classes, largely of the early types. This work involved the use of computers of steadily increasing capacity, initially at the University of British Columbia, later at various computers controlled by the United States National Aeronautics and Space Administration.

Many measurements of radial velocities, too numerous to list, have shown variations that could only be explained in terms of turbulence in the stellar atmosphere. Turbulence has been likened to the thermals that appear in the earth's atmosphere on a sunny day[87], although the causes of the two phenomena are not the same. One method of studying turbulence is by the use of curves of growth; displacements from the selected curve are a measure of the turbulent motions in the star's atmosphere. In a review paper read at meetings of the IAU, Wright[88] summarized the knowledge about turbulent motion and showed that it was greatest in the early-type supergiants (Class I), least in dwarfs, but admitted the observations were few and biased in favour of the brighter stars.

One of the more interesting spectra suggesting turbulence, although of a more dramatic kind, is of BW Vulpeculae, a Beta Cepheid, which has a discontinuous radial velocity curve. Odgers[89] studied this curve over many cycles and found that it had two short periods of rapid and large change in each cycle. This was accompanied by equally prominent changes in the spectrum. Odgers postulated that these changes were due to the expulsion of material from the atmosphere at a velocity of 100 km/sec or more for a short interval of perhaps 20 minutes and to its fall back into the star. In a later paper[90], in which he was joined by postdoctoral fellow R.S. Kushwaha, he investigated whether this motion could be the result of a shock wave. The results were not conclusive, but did show that the observed dissipation of the motion was consistent with the decay of a shock wave. The theory was later applied to a second star with plausible results[91].

Underhill made an extensive study of the spectra of three supergiants[92] and found evidence of turbulent motion with a radial velocity of as much as 200 km/sec. At about the same time, in a paper[93] read at a symposium on astronomical turbulence, she brought the picture up-to-date and provided a thorough review of the subject. Much later, in a series of five papers produced while he held a postdoctoral fellowship[94], J.B. Hutchings considered the "Expanding Atmospheres of Supergiants."

Interstellar Material

There are two sorts of interstellar material, gas and dust. The presence of gas is known from the absorption lines that it imposes on starlight, the intensity of the absorption lines being proportional to the distance of the star. There are also certain nebulae that appear to consist of clouds of gas made luminescent by radiation from a nearby star. It is known that the gas is emitting light, rather than reflecting it, since the spectra exhibit emission lines.

In their work on the rotation of the Galaxy, Plaskett and Pearce showed that, on average, the radial velocities of the interstellar lines were about half the values of velocities derived from stellar lines in the same spectrum. This is compatible with the assumption that the gas is uniformly distributed throughout the Galaxy.

The presence of dust is proved by many examples. There are areas in the Milky Way in which there seem to be few, if any, stars. Since stars are believed to be uniformly distributed in space, there must be clouds obscuring those areas. The fact that these areas lie in the Milky Way is only one of many reasons to believe that dust is concentrated in the Galactic plane. The presence of dust is also shown by reflection nebulae, where whole areas of space glow in the reflected light of a nearby star.

The most telling evidence for interstellar dust is the reddening of stars. Just as the sun appears red at sunrise and sunset due to the scattering of blue light by the atmosphere, the light from distant stars appears red, due in part to scattering, in part to the absorption of blue light by interstellar dust. We have already seen how a positive colour index can indicate the presence of interstellar dust. The effect of absorption is to reduce the apparent magnitude of a star by about 0.8 magnitudes for every 1000 parsecs (3260 light years).

The effect of absorption is not the same at all wave-lengths, and the variation with wave-length defines the interstellar reddening curve. To develop such a curve it is customary to compare the intensity distribution of pairs of stars of the same spectral type which suffer different amounts of reddening. By attributing the differences to the reddening, the shape of the interstellar reddening curve can be established.

We spoke above about Underhill's development of a series of model stars of a variety of types. In order to test these, she and Walker[95] computed the intensities of a large number of lines for each model, calculated the difference between these and observed values, and so constructed a reddening curve. The results were in good agreement with observation.

The study of interstellar matter had long been an Observatory interest. We saw in Part I that McKellar had identified certain molecular lines in the spectra of B stars as being due to diatomic molecules in interstellar space, and that Beals had studied some broad interstellar absorption bands, which contrasted strangely with the normally sharp interstellar lines. More and more of these broad bands were discovered, as well as lines that were diffuse rather than sharp.

In 1957 a postdoctoral fellow, Robert Wilson, studied the spectra of two reddened stars, using the spectra from two blue (unreddened) stars of similar types as comparisons to define the continuum. He confirmed the existence of two broad bands already known or suspected, and found two additional ones[96]. In a subsequent paper he discusses the physical basis of scattering[97].

Much later, in 1969, by which time some 25 of these bands had been discovered, a team from the Observatory used the recently-developed photo-electric scanner mounted at the Cassegrain focus of the 72-inch telescope to scan a broad spectral range, covering a number of these bands, for nine different red stars located in widely differing galactic regions. Again, they used unreddened stars of the same spectral class to define the continuum. They confirmed the existence of many of the bands, but found marked irregularities from star to star[98].

The Atmosphere of Supergiants

A small group of binary stars, known as the Zeta Aurigae stars after the prototype, have proved to be invaluable in the study of the atmospheres of supergiants. Each of these binary systems consists of a giant late-type star and a much smaller early-type star. The larger, cooler, star eclipses the smaller, hotter, one at regular intervals. As the secondary star moves into eclipse behind the primary one it provides a source of radiation which is selectively absorbed by the chromosphere of the cool star. The conditions can be compared during the ingress and egress phases, and the period of totality provides a pure spectrum for the larger star. Most of the observations use the K line of calcium. The periods of the eclipses range from 972 days (2.7 years) to 9864 days (27.1 years) and are reasonably regular, so that observing programs can be set up to obtain spectra throughout the ingress and egress stages.

The IAU promoted cooperative observing programs, both spectroscopic and photometric, of these stars, in which the DAO was a most active collaborator. Each of the stars was observed at the critical periods and no less than twenty papers were published giving the results. Wright has reviewed the work up until 1970. He gives the masses and dimensions of the stars, the orbits and the spectra of each, and documents the variations in these from event to event in some detail. These point to variations in the extent of the atmospheres and suggest the existence of condensations within the chromosphere of the larger star.

The Zeta Aurigae stars are a principal source of information on stellar atmospheres, and will undoubtedly continue to provide data obtained from many more eclipses.

THE SCIENTIFIC PROGRAMS: III – PHOTOMETRY

The light emitted by a star is a function of its temperature; the technique of photometry is to measure the ratio of energy at two or more points of the continuum, as, for example, in the ultraviolet (U), in the blue (B) and the visual, (V), usually centred on the yellow. If apparent brightness is indicated by f, then f is a function of the star's intrinsic luminosity, (absolute magnitude) which in turn is a function of the star's temperature and radius, and of its distance. The ratios f_V/f_B, or f_B/f_U are functions of temperature only. Not quite; they are also a function of the interstellar absorption, but since this is different for the different colour ranges, astronomers are able to allow for it and so determine the true values of f_U, f_B and f_V and the temperature of the star.

A useful concept arising from the same measurements is that of colour index. This is the difference in magnitude measured at two or more different colours in the sense B-V or U-B. Since a red star appears brighter in yellow light than in blue, red stars generally have a positive index, blue stars a negative one. A scale has been developed in which the index is set to zero for stars of class A0. Stars earlier than A0 have an increasingly negative index, stars later an increasingly positive one. Measurement of the colour index is an accurate way of placing a star in its proper spectral class, unless interstellar matter intervenes to redden the star's light. In that case the difference between the class as determined by the colour index and by spectroscopy provides a measure of interstellar absorption.

Photometry was never a major interest at the DAO for two reasons: photometry does not require a large telescope, and it would have been wasteful to devote time on the 72-inch telescope to this purpose; more importantly, conditions at the DAO, with its low elevation and nearby city lights, were not suitable for it.

However some work was attempted. In the mid-1930s Beals developed a photometer for use with the 72-inch telescope, that used a caesium photo-electric cell. After an initial period of testing it was not much used.

A much more sophisticated instrument, using a photomultiplier tube was developed by Argyle in 1957[8]. It could be mounted permanently at the Cassegrain focus of the 72-inch telescope; a prism could be rotated into the optical path of the telescope to deflect the light into the photometer as required. The photometer had a number of apertures that could be selected simply by turning a turret, and a number of filters – ultraviolet, blue, green and yellow – that could similarly be changed by revolving a turret. This was important; the output at the various colours had to be measured sequentially and it is necessary to do this as quickly as possible so as to minimize changes in atmospheric conditions. Recording was by pen and ink on a strip recorder.

The instrument was not much used, for the reasons given above.

Ten years were to go by before there was renewed activity, and this was brought on by the site-testing needs of the Queen Elizabeth II telescope. A team under the direction of G.A.H. Walker[100] developed a five-colour photometer for use, on Mount Kobau, with a 16-inch telescope. A grating spectrograph provided a spectrum; different sections of this were examined by windows in the focal plane of a camera which fed the data to four separate photomultipliers. The data were punched out on paper tape for subsequent computer processing. The fact that the observations at the various colours were made simultaneously eliminated any error due to changing atmospheric conditions. The results of this survey will be considered in the final chapter.

Toward the close of our period of interest a number of papers in photometry appeared, involving DAO astronomers, but describing work carried out at other, more favourably located, observatories. Hill, Walker and Morris were allotted times on the Cerro Tololo Inter-American telescope, in Chile[101], and Hill observed at the Kitt Peak Observatory[102].

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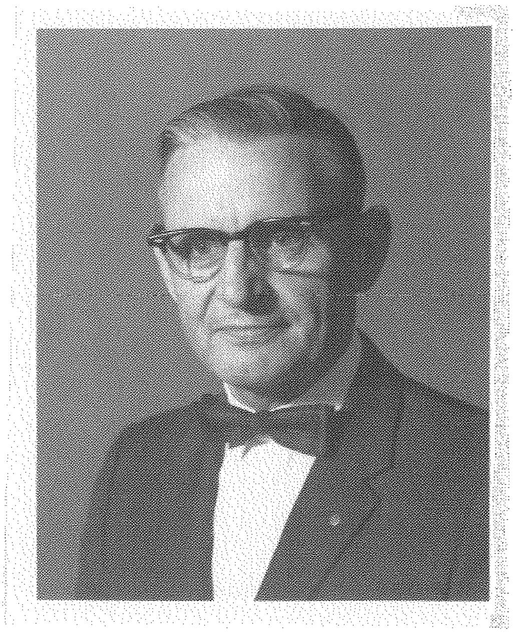
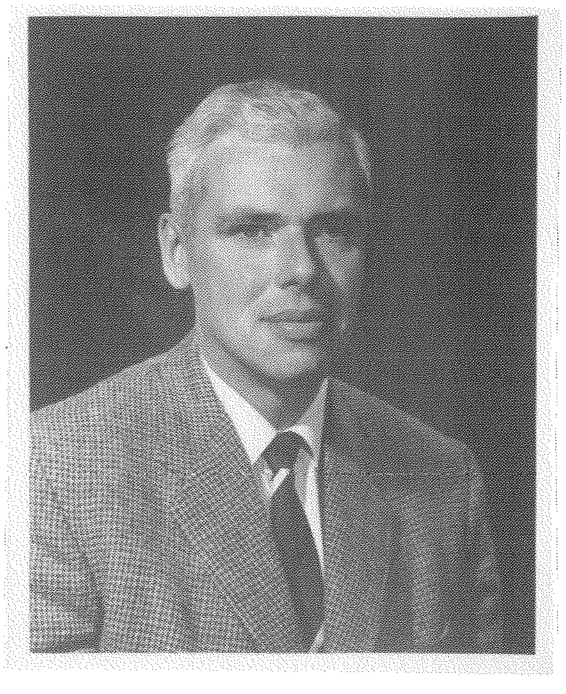
W.S. McClenahan, Chief, Positional Astronomy and Time Service, 1945-1957.



M.M. Thomson, Chief, Positional Astronomy and Time Service, 1957-1966; Chief, Astronomy Division, 1966-1970.



Dr. J.L. Locke, Chief, Stellar Physics Division, 1955-1966.

VIII – THE DOMINION OBSERVATORY – ASTRONOMY

INTRODUCTION

In this chapter we shall consider the work of two Divisions, Positional Astronomy and Time Service, and Stellar Physics. The former had changed neither form nor objective since the founding of the Observatory. Stewart had been the founding Chief of this Division; on becoming Director he was succeeded by C.C. Smith as Division Chief but continued to devote his research energies to the interests of the Division during his term as Director. Smith retired in 1937, to be followed by D.B. Nugent. W.S. McClenahan succeeded in 1945. This succession of Chiefs had brought the Division into an era of modern time-keeping and transmission, and it was considering the development of a new instrument, the photographic zenith telescope. McClenahan continued as Chief of the Division until 1957. He was succeeded by M.M. Thomson.

Things had not gone so well in the astrophysics section. The development of the 72-inch telescope, and the migration of many astronomers to the Dominion Astrophysical Observatory, had been a blow from which the Ottawa astronomy group never recovered. Beals made no attempt to revive the 15-inch telescope, except for public viewing, although he did attempt, unsuccessfully, to use the objective-prism telescope in the small dome to study stellar temperatures. Instead, he extended the solar-physics program and supported the development, already begun by Millman, into the field of meteor physics. These several interests were combined into a new Division of Stellar Physics, under the direction of P.M. Millman. Beginning in 1951, the Division, along with nearly every other group in the Observatory, became involved in the study of meteorite craters, and in 1957 it began the development of a radio astronomy observatory.

Millman moved to NRC in 1955 and was succeeded as Chief of the Stellar Physics Division by J.L. Locke; Ian Halliday, recently transferred from the Positional Astronomy Division, became responsible for the meteor program and a new recruit, Victor Gaizauskas, joined the Division in 1955 to assist in the solar studies.

In 1966 Locke moved to the NRC to direct the scientific program of the Algonquin Radio Observatory. The two astronomical Divisions were combined into a single Division of Astronomy, under the direction of Thomson. This amalgamation was demanded by two considerations; in the rapidly growing Department of Energy Mines and Resources there was no place for small Divisions, and we were anxious not to complicate the future arrangements for the proposed Institute of Astronomy.

POSITIONAL ASTRONOMY

We left the Division of Positional Astronomy on the threshold of a new era: "there is no doubt that the modern methods of photographic observation and crystal-clock time keeping are likely eventually to result in increased accuracy"[1]. This was a gross understatement, as we shall see; it was quite impossible in 1948 to imagine the advances that would be made.

The Meridian Circle

We saw in Part I that observations with this basic instrument declined during the war, partly because of the shortage of staff but largely because the publication of a large catalogue by Boss (The General Catalogue, 1937) showed "that observations by a single meridian circle instrument could provide no improvement in stellar positions except for those stars whose positions were poorly determined"[2]. Nevertheless the program was continued on a limited basis until 1950, at which time the results were prepared for publication[3]. A new program was then commenced, in cooperation with the US Naval Observatory and the Herstmonceux Observatory, to obtain precise positions for stars needed in the Photographic Zenith Tube (PZT) programs of Greenwich, Richmond (Florida) and Ottawa.

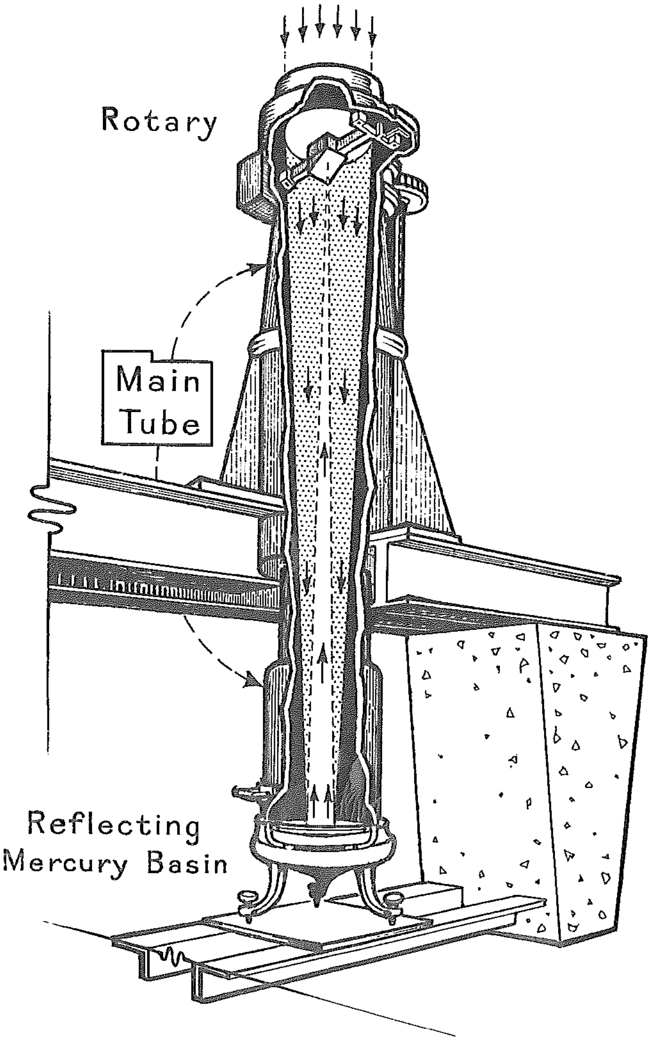
In preparation for this important program a number of improvements were made in the instrument[4]. A reversible motor replaced the hand crank that had been used in moving the telescope in declination. Thirty-five mm cameras were installed to photograph the settings on the graduated circles, replacing the microscopes used originally, and a projection machine[5] was developed to read the resulting photographs.

The program of PZT stars was completed in 1953. A new program of 3000 stars, selected by the Astrometric Committee of the IAU to provide well-determined positions for surveying in northern latitudes, was begun. By 1956 the IAU had embarked on a massive new program to recatalogue the position of 180,000 stars. Star positions would be derived mainly from photographic plates of star fields, but accurate positions of reference stars were required; the Observatory was asked to observe 3754 of these. This requirement was combined with the earlier, 1953, program and observations were continued until the end of 1962. The meridian circle transit was then de-commissioned, bringing an end to 52 years of useful service. Results for the later years were published[6].

It is proper to ask what value the contribution of the Observatory work had to the establishment of international astrometric standards. Woolsey[2] discusses this question at some length. He groups the work into five programs:

1. 1911-1923	Latitude stars for Canadian surveys	Published 1949
2. 1922-1935	BH stars, first half	Published 1952
3. 1935-1950	BH stars, second half	Published 1954
4. 1950-1953	PZT stars	Published 1957
5. 1954-1962	AGK3 reference stars	Published 1966

The results of programs 1 and 2 were used by Morgan in a catalogue published for the US Naval Observatory in 1952; those of programs 1 to 4 were used in the revision of the international catalogue of fundamental stars FK3; program 5 was used in the further revision of the FK3. The results of observations for 1935-1950, and the positions of the fundamental stars observed during 1950-1953, were also sent to the Rechen-Institut, Heidelberg, Germany, which publishes an annual series "Apparent Places of Fundamental Stars"[7]. The accuracy of the Ottawa data compared favourably with that of other observatories; the probable error, over the years, in a single observation of right ascension was 0.28 sec of arc, of declination 0.38 sec of arc. This compares with the international catalogue mean error of 0.29. We may therefore conclude that the efforts of so many people over so many years were well spent; the fact that the astronauts in their space vehicles reached their destinations testifies to the accuracy of the star catalogues.



OTTAWA P·Z·T·

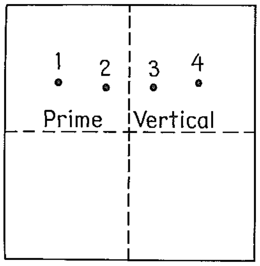
The Photographic Zenith Tube

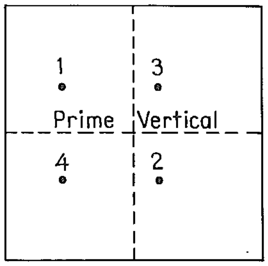
By the end of the war it had become apparent that "the transit observations with the Cooke transit formed the weakest link in the determination and maintenance of fundamental time at Ottawa[8]." The evidence was provided by crystal clocks, which were already demonstrating a day-to-day precision of one or two milliseconds. The solution lay in the use of the Photographic Zenith Tube (PZT), a prototype of which had been developed at the US Naval Observatory in Washington.

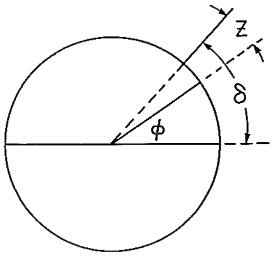
In 1946 a recommendation was received from the executive committee of the IAU advocating the location of a PZT in Ottawa[8]. On his arrival in Ottawa, Beals asked the Division to begin planning for the instrument and arranged for funding during the fiscal year 1949-1950. The US Naval Observatory, which was constructing a new PZT to be located at Richmond, Florida, made a full set of drawings available to the Observatory[9]. These plans were modified to provide a 10-inch objective and a focal length of 14 feet. Casting of the body of the instrument was by Vickers Ltd of Montreal, and the objective was figured by the Perkin-Elmer Company of Glenbrook, Connecticut. All the other precision components were made in the Observatory machine shop.

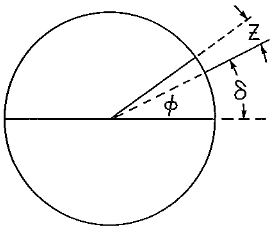
This instrument is illustrated herein. Light from the star enters through the objective at the top of the telescope, is reflected at the mercury basin at the bottom, and comes to a focus on a small photographic plate just below the objective. The mercury mirror, since its surface is level, defines the vertical direction; it not only serves to reflect the beam but, by moving the basin up or down, the focus may be made exact. To minimize the vibration of the image the basin floats in a second, larger, basin of mercury.

The objective and the plate holder are mounted in a rotary head which rotates back and forth through exactly 180 degrees between each of four observation of the star. This has the same effect as the rotation of the meridian circle on its pivots; it eliminates most of the errors inherent in any telescope. The effects of this rotation will be apparent from the diagram below. The points 1 to 4 in the first figure represent the positions that a star image would have at thirty second intervals as it tracks across the meridian, indicated by the vertical dashed line; their distance from the prime vertical, indicated by the horizontal line, is a measure of the zenith distance, z, of the star. The points do not appear in this way because, instead of being stationary, the plate is rotated through 180 degrees between each exposure. The actual position of the points on the plate is shown in the right-hand figure. Again the distances of the points from the prime vertical is a measure of the zenith distance. This is usually expressed by









saying that the distances 1-4 and 2-3 are each twice the zenith distance. The zenith distance is an important measurement, as we shall see.

There is one further complication. The exposures of the stars to produce the images on the plates are not instantaneous; there is not sufficient light to permit this. The exposure in each position lasts 20 seconds; the plate is moved at the velocity of the star to produce the stationary image.

What is the time of transit of the star? The time at which each exposure begins is recorded on a chronograph against the output of the master sidereal clock, and therefore the time taken for the star to transit from position 1 to position 4 is known to a high degree of precision. The distances of the corresponding points from the meridian can be measured from the photographic plate with a comparable precision. If these distances are identical, the instant of transit is the mean of the times at the two extreme positions. If they differ, an adjustment can be made since the velocity of transit is known. The difference between the transit time and the master clock time is the clock correction.

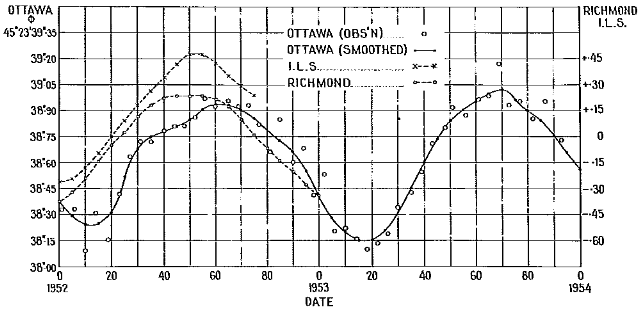
Let us return now to the question of the zenith distance. As we see in the diagram above, the latitude φ of the observing station, the declination δ of the star, and its zenith distance Z are related by the formula

$$\phi = \delta \pm Z$$

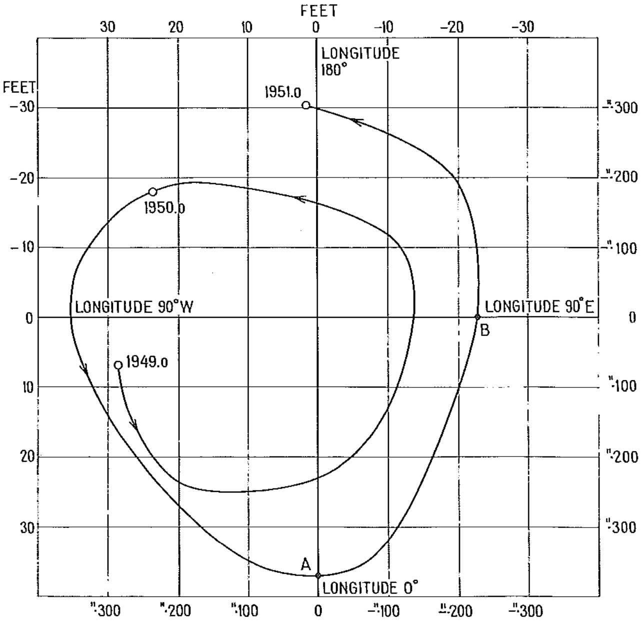
the sign depending on whether the star passes to the south or to the north of the prime vertical. The zenith distance is not a constant for a particular star; it varies up and down by several tenths of a second of arc. There are several sources for this variation. One of them is that the pole of the earth's rotation is not fixed; it varies irregularly around a mean position. The equivalent apparent variation in the station's latitude amounts to several tens of feet. A fortnightly average based on all the stars in the program is reported to the International Latitude Service, which is then able to produce graphs of latitude variation for the contributing stations, and to plot the path of polar motion. Typical plots are shown in the figures below. The determination of latitude variations has been an important part of the output of the PZT group from the inception of the program[10].

The installation of the PZT was completed in June 1951, and continuous operation began in October. After initial experimentation a catalogue of 80 stars was selected, covering 24 hours of right ascension. Programs were drawn up consisting of 10 stars each, with equal numbers north and south of the zenith, and two or more of these programs were observed on every clear night. It was not necessary for the "observer" to actually be at the telescope. Since the time of passage is known from the catalogues, he could sit at a console in a warm room and operate the instrument by remote control. This was done at the start of the program, but after initial testing a "program" machine was developed by V.E. Hollinsworth. Driven by a synchronous motor controlled by the master clock it used 35 mm film which, through perforations, operated a number of contacts at precise times. The machine could run for 120 hours on a 1000 foot reel of film, and was accurate within one-half second.

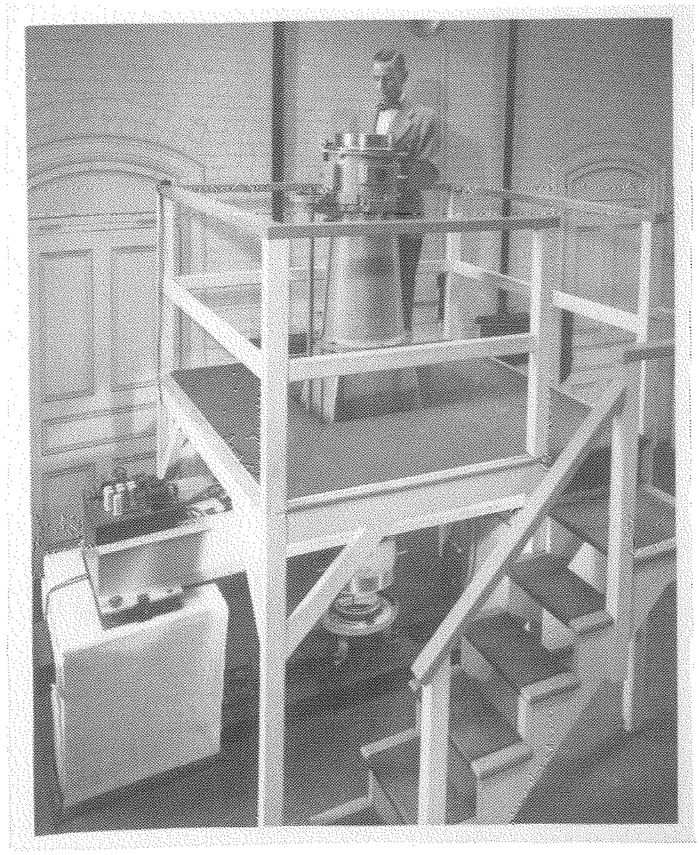
Initially the PZT was set up in the transit room of the Observatory but the results were not as accurate as had been hoped. It was recognized that this was due to unstable temperature gradients around the telescope, caused by the massive walls of the transit room. In 1960 the PZT was moved to a specially designed building set up on Experimental Farm

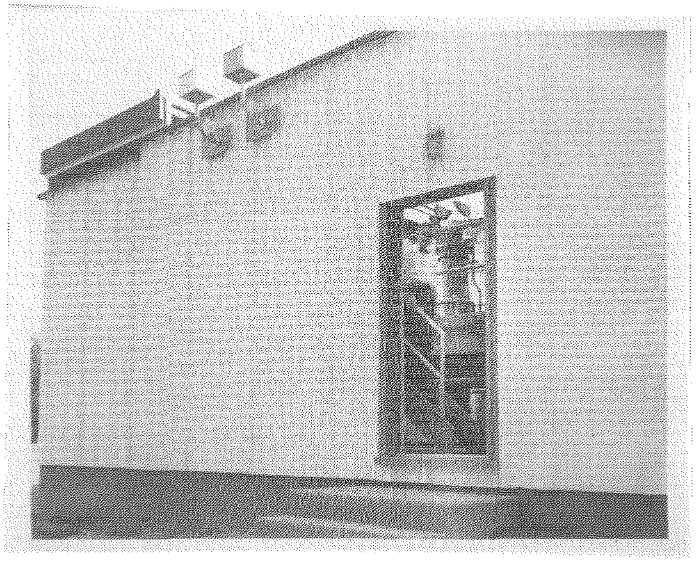


Latitude variations at Ottawa, 1952 and 1953; variations at Richmond, Florida, and as issued by the International Latitude Service, are given for comparison. From Journal, Royal Astronomical Society of Canada, 48, 173.



Motion of the pole, 1949-1951, as given by the International Latitude Service. From Journal, Royal Astronomical Society of Canada, 48, 172.





M.M. Thomson with the PZT: left – as set up in the Transit Room; above – in its special building on Experimental Farm grounds facing the Observatory.

property facing the Observatory. The building consisted of asbestos siding attached to a metal framework, and the roof rolled back so that the telescope was exposed to the outside air. There was a marked improvement in the results.

With the new building, operations became completely automated. A 24-hour timer opened the roof at dusk and closed it at dawn. If a sensor mounted above the roof detected precipitation the roof would close; conditions were retested every twenty minutes.

Thomson designed a projection-type measuring machine which was built in the Observatory shops. It provided a magnification of 15 times, and permitted the accurate measurement of both coordinates of the star position. Initially these coordinates had to be noted by hand on forms appropriate for computation. Later, when calculations were made by computer, the machine automatically prepared the punched cards.

The location of the PZT hut on Experimental Farm property was intended as a temporary expedient and in 1962 a search began for a permanent location. A site in the Gatineau Park was proposed and a small hut was built as a base for site testing. Seeing was not appreciably better there than in the vicinity of Ottawa, and the hut was abandoned. It was taken over by the Ottawa Ski Club as a shelter on one of its remoter trails, and is still known as "Observatory Hut". A location about ten miles west of Ottawa, at the Quiet Site of the Department of National Defence, was ultimately selected.

Before the move was made there was another development. The US Naval Observatory, in cooperation with Australian astronomers, had begun a study aimed at detecting continental drift. Identical PZTs would be set up at precisely the same latitudes, one at Mount Stromlo in Australia, the other at Pinto Indio, Argentina. The two would observe the same programs of stars; if continental drift were taking place the latitude, as shown by the time difference between the two observatories, would gradually change. It was decided that an experiment should also be conducted in the northern hemisphere, between Herstmonceux in England, and a point to be selected in Canada. Arrangements were made to have the US Naval Observatory construct two telescopes, one for the Argentinean station, the other for Canada.

A number of sites were considered for the new observatory, all in western Canada. The location finally chosen was at Priddis, Alberta, about 12 miles southwest of Calgary. The original Ottawa PZT, which more nearly matched the Herstmonceux one, was moved to Priddis, the new instrument was installed at the National Defence Quiet Site. The Priddis observatory was opened on May 18, 1968, in connection with a General Assembly of the Royal Astronomical Society of Canada[11].

With the breakup of the Observatories Branch in 1970 the PZT program remained with the Earth Physics Branch. As we shall see in a later section, modern methods of time keeping had shown that the rotating earth, which had been our clock for so long, was in fact an imperfect one. The vagaries of its motion are a geophysical problem.

Woolsey[12] analyzed the Ottawa PZT output for the period 1956-1970 and showed that values of latitude and longitude determined from its measurements had a probable error of 0.01 seconds of arc. He determined residuals for the Ottawa data by comparing the Ottawa values of latitude and longitude with those determined by the Bureau International de l'Heure

(BIH), using many stations from around the world. A spectral analysis of these residuals shows that secular variations in the position of Ottawa of 10 cm or less could not be detected.

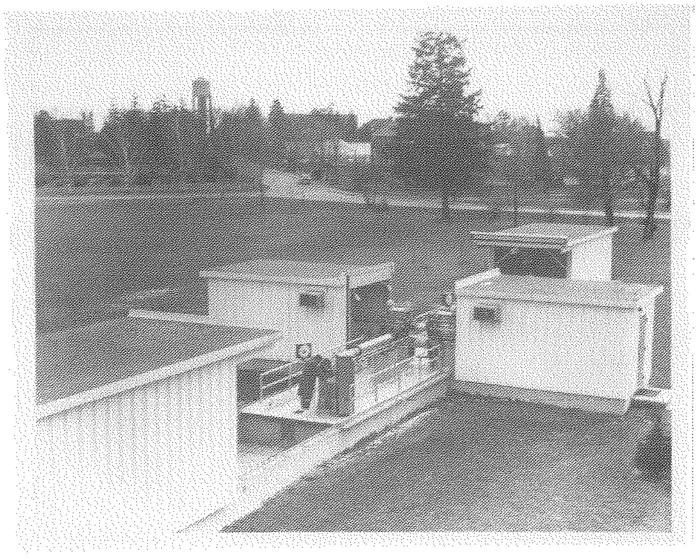
The Mirror Transit Telescope

The limitations of the meridian circle, not just of the Ottawa one but of all such instruments, have been documented in an earlier section: single observations had a probable error of about 0.30 sec. of arc. This fact became evident in 1937, with the publication of the Boss catalogue, and by the early 1950s, when the PZT had shown the effectiveness of modern instrumentation, planning began for an instrument to replace the meridian circle.

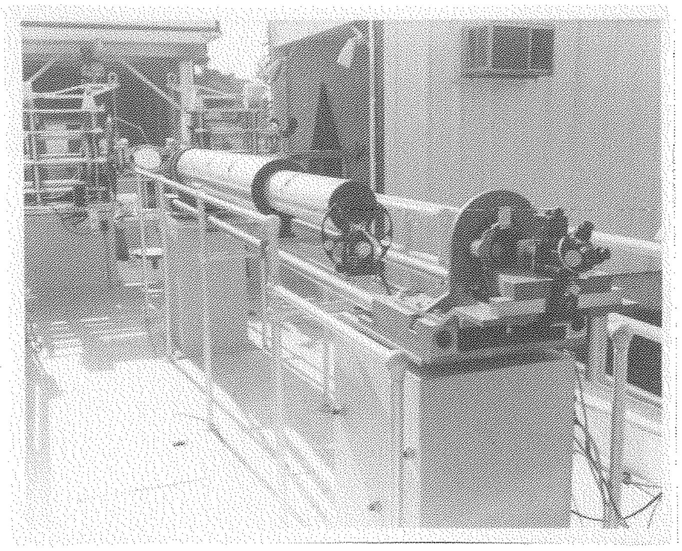
The new instrument would have to overcome the deficiencies of the Meridian Circle: "tube flexure, instability of the micrometer slide, thermal disturbances caused by the proximity of the observer, and by the human error of the observer himself"[13].

The program began in 1953. Ian Halliday was responsible for the initial planning of the instrument, which was much influenced by a design already published by R.D'E. Atkinson of the Herstmonceux Observatory. Upon Halliday's transfer to the Stellar Physics Division in late 1954, the work was taken over by G.A. Brealey, assisted by R.W. Tanner, J.J. Labrecque and C. Morbey. They worked under the interested eyes of the Director, Beals, and the Division Chief, Thomson.

The instrument would consist of two telescopes mounted in a north-south line, pointing at each other and separated by a distance of 15 feet. Midway between them would be a plane mirror pivoted on an east-west axis, to reflect the star image into one or other of the telescopes, the north telescope for stars transitting north of the zenith, the south telescope for those transitting to the south. The design promised to overcome all the problems present in the Meridian Circle: since the telescopes did not move, their supports could be made as massive as necessary to prevent relative motion of telescope objectives and cameras; modern optical materials and methods of mounting could ensure absolute rigidity of the mirror, the only moving part; the instrument would be operated by remote control and the registration would be photographic.



The Mirror Transit Telescope.



Closer view of the telescope. The pivoted plane mirror, with its counterweights, seen to the left rear, is reflecting into the north telescope.

Construction of the instrument began in 1959 on Experimental Farm grounds facing the Observatory. The telescopes had 10-inch objectives, with focal lengths of 170 inches. The mirror was an optical flat, 11 inches in diameter and two inches thick, of fused quartz. It was mounted in a stainless steel shell, part of a 36-inch casting that carried the pivots on which the assembly rotated around the east-west axis. A system of counterpoises reduced the pressure on each pivot to 25 pounds. Two 30-inch disks were attached to the axis, one on each side of the mirror; one was a gear wheel controlled by a servo-mechanism for remote setting of the mirror, the other carried a declination scale graduated at three-minute intervals, which was photographed by six cameras at each setting[14].

In operation the mirror reflected the star image into one or other of the telescopes. A fixed reticule at the focal plane was etched with horizontal and vertical lines to define the stellar coordinate system in right ascension and declination. As in the PZT, the photographic plate on which the passage of the star was recorded, moved with the star. The duration of the exposure was about 40 seconds. The photographs of star position and of declination setting locate the star in direction and time. They were read on a measuring machine that produced computer-ready output.

The Annual Departmental Reports over the next ten years tell a sad tale of high hopes steadily disappointed. The instrument was not a success, and was finally abandoned in 1969. Tanner provided a technical discussion of the problems faced in the project[15].

The first concerned the graduated circle for the measurement of declination, and the cameras that recorded its position. The circle was marked at three minute intervals on a gold band, and the six cameras were arrayed in a configuration intended to provide a close check on the readings. They did not do so; the readings varied with the amount of light being reflected from the gold band and with the density of the film. Strict control of exposure and of developing did not eliminate the problem.

A second problem arose because of poor local "seeing". Even successive photographs of the same graticule, taken with millisecond exposures for the determination of instrumental constants, were often displaced by as much as one second of arc. Various combinations of building arrangements, and the use of fans to provide an even temperature distribution, did not improve the situation adequately. In retrospect this result is not surprising. With the roof open and the walls rolled back, the instrument was exposed to a near-hemisphere of sky, with resulting radiative cooling and the movement of air of differing temperature by any casual breeze.

Finally, the axis of the mirror did not remain constant; this was traced to movement in the piers. Insulation was applied to the portion of the piers above ground and thermostatically controlled heat was applied to the part below. There was some improvement, but not enough. The piers supporting the two telescopes also showed evidence of movement.

Tanner makes the point that the principle of the telescope was not at fault; in particular there was no evidence of flexure in the mirror or its mount. The problems outlined above were mechanical and might have been overcome given sufficient money and adequate engineering support. Unfortunately this support was unavailable.

The failure of the telescope was a great disappointment to everyone, not only in Ottawa but in astrometric observatories everywhere; the improvement by a factor of two or three in the accuracy of stellar positions would not only have enhanced the value of star catalogues but would have been of the greatest importance in defining the proper motions of stars in our galaxy and the perturbations of the earth's motion.

Satellite Observation[16]

While the launching of an artificial satellite during the IGY had been hoped for, it had been expected that it would be launched by the United States, rather than by the Soviet Union, and that it would not be visible from Canada. The Division had therefore made no preparation for satellite observations when *Sputnik I* appeared on October 4, 1957. First observations were made with binoculars, but within a week a camera had been mounted to photograph the satellite trail against a background of stars. One such photograph yielded two trails, separated by a few degrees, presumably of the satellite and of its launching rocket.

By January 1958, an aerial camera with an 8-inch f/2.9 lens had been mounted on an equatorial drive. A one blade sector, mounted on a shaft turned by a synchronous motor, occulted the lens at every revolution, so that the trail of the satellite across the sky appeared as a sequence of dashes. Each occultation was precisely timed by a chronograph. This camera produced excellent results, but a second aerial camera was ordered in which the rotating sector was replaced by a between-the-lens shutter that opened and closed at controlled times to produce the broken trail.

Calculations consisted in determining the position of the breaks in declination and right ascension by an examination of the star field, and measuring the time of the break from the chronograph. Each observation gave one point on the satellite's apparent path in the sky.

As more satellites were launched, both by the USSR and by the USA, international programs were set up for their observation and results were reported to World Data Centres where the orbit of each was established and its gradual changes monitored. The information obtained was of geophysical importance since aberrations in the orbit reflect anomalies in the earth's gravitational field, and the rate at which the orbit changes provides information on atmospheric density.

The Division had embarked on the international program with the understanding that the work would not interfere with its normal responsibilities. This became increasingly difficult to ensure and by 1963 it was necessary to drop the satellite work. The decision was not based simply on work load. Under the best circumstances the Observatory equipment produced observations accurate to 35 seconds of arc, but as the demands grew for more precise orbits this was not adequate. Labrecque began the construction of a Schmidt camera that would provide the necessary accuracy[17].

The camera developed by Bernhard Schmidt in 1932 has revolutionized astronomical photography because it has a very fast optical system and small optical aberrations. It consists of a concave spherical mirror with a corrector plate mounted at its centre of curvature. The design and figuring of this plate for any particular application is of course the major part of the operation. This work was done by Labrecque who aimed to obtain an accuracy of 1 micron in plate measurements. Unfortunately, before the work had been completed, commercial cameras were available with even better accuracy than this, and the work was abandoned in the name of economy.

The Meteor Observatories at Meanook and Newbrook, with their large Super-Schmidt cameras, were able to produce excellent photographs of the satellites; indeed, on October 9, 1957, Griffin secured the first Sputnik photograph in North America. However, satellite observation never became a major part of their work.

The Moon Camera[18]

Before discussing the Moon Camera it will be well to review some fundamental concepts of timekeeping[19]. The earth is the basic clock. As it moves around the sun it defines the Tropical Year; as it rotates on its axis it defines the day. The length of the day varies slightly throughout the year but its average value defines the Mean Solar Day. The second was defined as 1/86,400 of the Mean Solar Day, and time so measured was called Universal Time, UT for short.

In practice, time is measured by the passage of stars over the meridian. Successive passages occur with each rotation of the earth, but an additional passage is introduced each year because of the earth's annual revolution about the sun. This "sidereal" year has one day more than the solar year; the mean solar day is equal to 1.00273909 sidereal days. The transfer from one time system to the other is a purely mechanical matter but since catalogues of stars always list their positions in sidereal coordinates a sidereal clock is a basic instrument of any observatory.

There is a third sort of time which has become important since the development of crystal and atomic clocks. We now know that the earth is not a reliable time keeper; some means of measuring time independently of its rotation is necessary. This is provided by measuring the time of passage of the moon and planets through the field of the fixed stars. In such measurements the aberrations in the earth's rotation have a negligible effect. Time so measured is known as Ephemeris Time, or ET. There is a corresponding ephemeris second. Because ephemeris time does not vary with the earth's rotation, it provides an absolute scale to link events of the remote past with the present and with events forecast for the future. The difference between ephemeris time and mean solar time is considerable, showing a loss of about 35 seconds in the last 60 years[20].

Dr. William Markowitz, of the US Naval Observatory, proposed that a study of ephemeris time be made during the IGY, by studying the moon's position among the stars. The project was feasible because recent studies had precisely defined the moon's orbit and its profile, with its mountains and valleys. Markowitz designed a camera that could take precisely-timed pictures of the moon against its background of stars. Because the moon moves very rapidly through the field of stars the camera had to be designed to "stop" this motion. This was accomplished by a tilting plate, the motion of which caused a displacement in the lunar image exactly equal to the motion of the moon. One of these cameras was given to the Observatory and mounted on the 15-inch equatorial telescope. Ottawa was one of a dozen or so observatories taking part in the program, which continued from mid-1957 until 1963.

A principal problem in interpreting the results arose from the difficulty of determining the centre of gravity of the moon, and hence of its position relative to the star field. This could be done at full moon, but at other times it was difficult to determine the centre with only one limb of the moon visible. However Markowitz himself[21] speaks of the results as "definitive". It is doubtful however that the Ottawa observations were of much value. "The 15-inch had a longer focal length than the ideal one for the size of the plate-holder. This reduced the number of stars per plate to a number that was dangerously small[22]."

Advances in Timekeeping[23]

The use of crystal clocks had already begun towards the end of the Stewart administration; indeed Thomson[24] tells us that the first such clock, manufactured by General Radio, was received in 1942, with an improved version delivered in 1944, and that standard frequencies were delivered to the Observatory from crystal clocks maintained by the Department of Transport at its monitoring station and by the NRC. The Observatory clock was called Co, to distinguish it from that of the Monitoring Station, Cm, and of NRC, Cr. Co was powered from the mains and it was thus unreliable as a standard, particularly because of the frequent, post-war, power cuts imposed by Ontario Hydro. A 20-kw gasoline-powered generator was installed in the machine shop with the necessary wiring connecting it to the main building. This generator was sufficient to supply all the emergency power needs of the Division. Co now became the primary Observatory timekeeper.

The availability of Co conferred another advantage. Hollinsworth[25] designed a frequency converter that transformed the 50,000 cycle output of the crystal to 60 cycle. This could be used to drive the chronographs which timed star transits on the meridian transit telescope. It also provided a precise drive for the telescope in the dome.

The Departmental Reports document constant improvement in the timekeeping equipment of the Division. A second General Radio clock was purchased in 1950 and Western Electric Frequency Standards arrived in 1951 and 1953. The Western Electric standards were installed in the clock vaults, the General Radio clocks were moved to the time room where they were used as time signal controls. The old designation, Co, was dropped; the clocks became known as X1, X2, X3, and X4. In 1954 a "ring crystal frequency standard", developed in the Research Laboratories of the British Post Office, was acquired and designated R5. After a period of testing it too was placed in the clock vault and all five clocks were connected by co-axial cables to a comparison panel in the time room. A variety of techniques was used in this comparison which eventually attained an accuracy of 1/10,000 of a second per day. One result of such testing was to show that the General Radio clocks, which controlled the time signals, were being influenced by temperature variations and vibrations. They were shock mounted and housed in a temperature-controlled environment.

The frequency laboratories of NRC were also experimenting with the ring standards. These proved so satisfactory that additional ones were ordered so that within a short time there were nine of them in the Ottawa area, two of them at the Observatory. It was agreed that "this resource should be pooled to provide a Canadian standard of time and frequency."

Two problems were involved, the intercomparison of the frequency output of the nine rings and the correlation of this standard with UT; the former was the responsibility of the NRC, the latter of the Observatory. Neither problem was successfully overcome. The oscillators were found to have unexplained frequency shifts of one part in a billion and the errors in the astronomical data were of the order of five parts in a billion. It was however a rather magnificent failure; how far the two disciplines had moved in a few years!

The bank of ring oscillators continued to provide the Canadian standard frequency for some time, but techniques which would yield an additional order of accuracy were already under development. They utilized the natural frequency of the cesium atom. The development of the device had been begun in the British National Physical Laboratories; by 1956 they had developed a working model and, in cooperation with the US Naval Observatory, had established the "value of the ephemeris second as 9,192,631,770 ± 20 cycles of the frequency of the central absorption line of the cesium 133 atom."

The NRC was also developing a cesium standard. It came into operation in 1958, with an accuracy of a few parts in ten billion; it was immediately adopted as the Canadian standard, and Observatory clocks were checked against it daily. The NRC produced a succession of cesium clocks, each more accurate than the last; these were accompanied by the production of two hydrogen masers which had a short-term frequency stability of one part in 10^{14}.

At about this time, in 1965, with the abandonment of the meridian circle and the transfer of the PZT to its location on Experimental Farm grounds, the transit wing of the Observatory became available for other use. It was completely insulated, the ventilation louvres were replaced with windows, and it became a modern Time Laboratory, air conditioned, and with all the equipment rack mounted.

Meanwhile cesium standards had become commercially available. By 1969 the Division was using five Hewlett-Packard frequency standards, three as their standard clocks and two to control the frequency of the CHU transmitters; microsecond accuracy was now the standard. Parallel developments were, of course, going forward in other observatories. How were the standards of the different observatories to be coordinated? Radio time signals were inadequate; they weren't sufficiently precise, and the travel times of the radio waves were uncertain within the demands of the problem. "Flying clocks" were the answer. In 1964 Hewlett-Packard made two of its standard clocks portable and, over the following several years, transported them by air to the various observatories, including both the Observatory and NRC in Ottawa. They tested the comparability of the clocks and the errors inherent in the radio transmission of time signals. The various observatories agreed to the tenth of a microsecond in time and to one part in 10^{12} in frequency!

These accuracies had far outstripped any attainable in the astronomical measurements. A new standard of time was necessary; it became the ephemeris second as defined by the experiments at the NPL, that is 9,192,631,770 periods "of the radiation emitted by the transition between two hyperfine states of the cesium 133 atom in the ground state". The uncertainty figure was dropped; the second of ephemeris or atomic time is now defined with an accuracy of one part in 10 billion. This standard was adopted by the General Conference on Weights and Measures in 1967.

There was of course a continuing need for mean solar time in the everyday service of mankind, but even this had to be redefined[26]. It depended on the period of rotation of the earth, which suffers a number of irregularities. We have already discussed polar wandering and the variation of latitude, but there is also a seasonal change – the rotation slows down in the spring and speeds up in the autumn – a gradual secular slowing down, and random unpredictable variations. The Bureau International de l'Heure, in Paris, provides a mean solar time, UT0, based on the short-term averaging of the data submitted to it by contributing observatories. When this is corrected for polar wandering the time is known as UT1; an additional correction for seasonal variations yields UT2. This is the time that observatories maintain in their mean time clocks, and transmit in their radio broadcasts.

Because UT2 is determined by the earth's rotation it is not uniform; since the turn of the century it has been running slow compared to ephemeris time. It follows that the cesium clock runs fast with respect to UT2. How are we to keep the two in step?

Initially the rate of the cesium clock was offset at frequent intervals so that it always approximated UT2. This was unsatisfactory. The cesium clock was then left to run at the same rate as ephemeris time. As it gained on UT2, a "leap second" was introduced to back it up. This has proved to be a highly successful technique. The leap second is introduced into atomic time only on June 30 or December 31, and only if the atomic clock has crept ahead of UT2 by about half a second. This is carefully monitored by the Bureau International de l'Heure.

As of July 1, 1983, ephemeris time was ahead of UT2 by 54.184 seconds. The difference will increase (or decrease) in steps of one second as each leap second is added (or subtracted).

The Distribution of Time

The distribution of time underwent improvements which, while not comparable to those in timekeeping, were nevertheless impressive. One of the changes might be regarded as regressive. The system of clocks that the Division maintained in Government buildings throughout the city was gradually dropped. According to the Annual Reports there were 750 of these clocks in 1947, "nearly 700" in 1951; by 1953 "the general tendency is for ... new clock systems to operate by synchronous motor control related to the Observatory time service by indirect methods." By 1956 "the number of clocks in government offices for which the Observatory is responsible has continued to decrease owing to the increased use of 60 cycle a.c. frequency dials." An era was over.

Modern techniques also overtook the Stewart time machine which had worked so well in providing suitably coded signals to a variety of clients. It was superseded by crystal timing devices in 1951. As the radio time signals increased in power and accuracy there was less need for other means of time distribution until, eventually, only the CBC was provided with a dedicated line for its 1 p.m. signals.

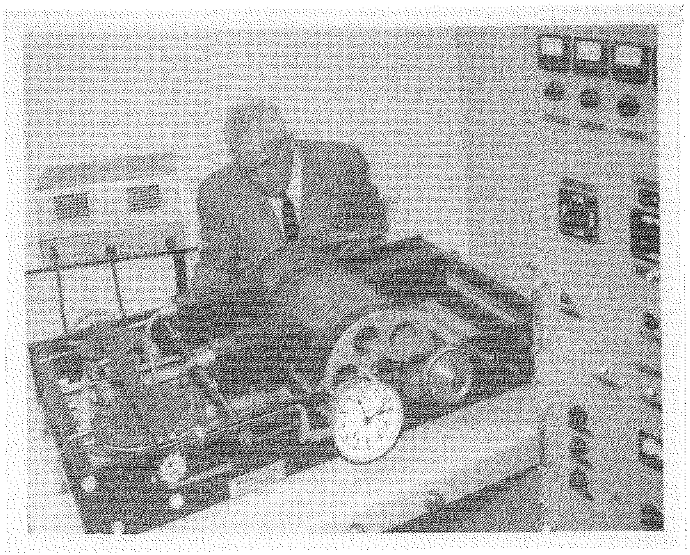
Up until 1946 CHU was operating from the Observatory with a power of only 50 watts on each of the three frequencies. No increase of power could be contemplated at that location. Arrangements were made to move the transmitters to a Department of Transport transmitter site on Greenbank Road, and some war-surplus 300 watt transmitters were obtained from the RCAF. These began operation in late June 1947. They transmitted through simple dipole antennas that did not permit the selection of an optimal radiation pattern.

When Beals became Director he supported the Division in its drive for a more powerful transmitter, and, beginning in January 1953, the 7335 kHz signal was being radiated by a 3 kilowatt transmitter from a new building.

The next major change was the introduction of a speaking clock, late in 1954. Before this, each minute within a five minute interval in the time signal had been coded; if one knew the time within a minute or two it was easy to identify the true minute. With the speaking clock, a voice announcement of the hour and minute was made each minute of the twenty four hours. The clock was of French manufacture, but the voice was Canadian, recorded by Frederick Meach of the Canadian Embassy. As Thomson remarks[27] "for a few years thereafter his voice was on the Canadian airwaves more than that of any other Canadian."

The recording was made on two strips of photographic film, one for each of the 60 minutes, the other for each of the 24 hours; they were wrapped around a drum and were read by a photo cell by means of reflected light. The quality of the reproduction was not satisfactory, particularly after it had passed through the various circuits to the transmitter. A new talking clock, manufactured by and leased from Audichron, a United States firm, was in operation by May 1960. The clock was installed in the transmitter building to avoid any loss of quality in the transmission line. Initially the voice reading was in English only; in 1964 a bilingual announcement was in place, Harry Mannis and Claude Miguet, both of CBC Toronto, being the speakers.

The original speaking clock was adapted to an automatic time announcement over the Ottawa telephone system; the service began in 1959 over an unlisted line. The line may have been unlisted, but the number soon became widely known, and calls soon overloaded the line. The telephone company insisted that additional lines be rented, but their proposed terms were not acceptable to Treasury Board. It was necessary to discontinue the service. This was done in March 1968, to the considerable annoyance of the Ottawa public.



W.S. McClenahan with the first speaking clock, manufactured by Ateliers Brillié of France.

V.E. Hollinsworth in the Time Laboratory established in the revamped Transit Wing of the Observatory in 1965.

From the time that the operations were moved to Greenbank Road there was a constant effort to improve both the frequency stability of the transmitters and the precision of the time signals. This was important not only to the Observatory, but also to the NRC which needed a source of accurate time in its research into frequency standards, and to the DOT which needed an accurate frequency against which to monitor the output of transmitters. Thomson[28] gives the details of the developments, in which all three agencies cooperated, and which led to the standardization of all three frequencies and to time signal pulses which were accurate to better than 1/1000 sec. There was also a steady improvement in output power, that of the 14,670 MHz transmitter being increased in 1959, and in the antennas; tuned vertical antennas were installed for all three transmitters by 1969.

ASTROPHYSICS

Beals had no intention of becoming simply an administrator; he would continue his scientific work. The first task was to complete his study of the spectra of the P-Cygni stars. This was described in Chapter VII. He next turned his attention to the temperature of these stars, with the cooperation of a theoretical physicist, R.D. Hatcher. The intensity of the emission lines was measured relative to the continuous spectrum, a method that Beals had already applied to the Wolf Rayet stars and to several novae. Using spectrograms and intensity measurements obtained earlier at Victoria, the authors established[29] a temperature scale for O, B and A type stars: A0 – 16,000°; B0 – 35,000°; O5 – 40,000°.

Next, with the cooperation of Miriam Burland, he discussed the spectrum, obtained before he left Victoria, of a star with P Cygni character[30]. In order to obtain the required spectra, Beals had observed on two occasions; the first plate was exposed for three nights and a total of nearly 18 hours, the second plate for four nights and a total of 25 hours. As a result of this lengthy and careful work he obtained spectra showing more than 100 lines not previously observed. These lines were carefully measured and identified. The results suggested that the shell of this particular star has a relatively small diameter.

At about this time The Royal Society of Canada convened a symposium on "The Atmospheres of the Stars and Planets". As President of Section III of the Society, Beals helped arrange the symposium, and his Presidential address[31], in which he considered the atmospheres of the early type stars, summarized much of his life's work in this field.

Under Beals' direction, J.A. Rottenberg, who joined the Division in 1949, made a theoretical study of the line profiles in P Cygni stars[32]. He set up a simple model of a star, emitting a continuous spectrum and enclosed in an expanding envelope, and found that emission lines of the form observed would result.

We spoke in Part I of Beals' interest in interstellar material, in particular of his idea that the intensities of the interstellar lines should be a measure of the star's distance. In collaboration with J.B. Oke, a summer student, he made a detailed study of this question[33] based on the intensities of interstellar K and D lines measured at Victoria over an 18-year period. They found clear relationships of the intensity of both lines with distance and concluded that the method would give stellar distances with an error rarely exceeding 25%.

This pretty much marked the end of Beals' direct involvement with conventional astrophysics; this was not a question of lack of interest, but rather of the intrusion of a new interest – meteorite craters, to be discussed in Chapter X. He was, however, constantly in demand as a speaker both to professional and amateur groups and many review papers derived from these talks[34].

PLANETARY STUDIES

A series of papers by Halliday deal with the diameter and mean density of the planet Pluto. At the time of the first paper[35] the accepted diameter of Pluto, as measured by Kuiper with the 200-inch telescope, was 5900 km, and the mass, as derived from its perturbing effect on Uranus, was about equal to that of the earth. Combining these two values one obtains a density of 50 gm/cm^3, an unreasonably high value. Which factor was wrong, the mass or the diameter?

Halliday proposed a much more accurate method of measuring the diameter – to measure the times at which Pluto occulted well-located fixed stars. To overcome parallax each occultation would have to be observed by at least two telescopes. By correlating the orbit of the planet with star maps he showed that such occultations would occur infrequently and would be observable only on large telescopes.

An international program was agreed to, and during the next two years Halliday, assisted by A.A. Griffin, refined his calculations forecasting probable occultations. Finally an occultation was forecast for the night of April 28/29, 1965[36]. An observing program involving several observatories ranging from Victoria in the north to Fort Davis, Texas, in the south, was arranged[37]. Unfortunately occultation did not occur within this range; it would certainly have been observed in Mexico, but an attempt to alert the one astronomer there who could have made the observation was not successful when he did not receive the telegraphed message. The US Naval Observatory at Flagstaff took a series of photographs bracketing the time of expected occultation, which showed a grazing incidence. This put an upper limit of 6800 km on the diameter of the planet.

Some time later, when new observations on the perturbations of Neptune suggested a mass value for Pluto only 0.18 that of the earth, Halliday[38] reconsidered the question of mean density, obtaining a value of 7.7 gm/cm^3. He concluded that the mass estimate was still too high, and pointed out that many sources of error still existed in the determination of Pluto's mass.

One other matter of planetary interest appears in the Contributions. A claim had been made that variations in the brightness of Jupiter's "great red spot" could be correlated with sunspot numbers. Edward Argyle, of the Dominion Radio Astrophysical Observatory, took exception to the statistical methods of the study in two strongly worded papers[39].

METEOR PHYSICS

Introduction

The story of meteor physics in Canada must begin with Peter Millman[40]. In 1929 he graduated from the University of Toronto and moved to Harvard to begin work on his doctorate. He already had extensive experience in astronomy, having worked at the DAO for three summers assisting Plaskett in his study of B stars, and he was expecting to be assigned a problem in radial velocities.

In the rotunda of the Observatory office building there were a number of back-lighted photographs of various astronomical objects. One of these was of a meteor spectrum. This had been obtained in the course of Harvard's program of stellar classification. For years they had been systematically photographing the skies, both in Cambridge and in a southern observatory, using an objective prism that produced tiny spectra instead of stellar images. The meteor spectrum had been obtained by chance as part of this program.

Millman was surprised to find that no one had been able to interpret the spectrum and asked the Director, Shapley, if he might have a try at it. Shapley not only agreed, he made available plates of a number of other meteor spectra obtained during the survey. Millman was launched on his lifetime career. His thesis, in which he identified more than fifty lines in the spectra and detailed the laborious steps necessary in the study, was published in 1932.

It is interesting that Millman credits Harry Plaskett for much of his success. It will be recalled that the younger Plaskett was granted leave from the DAO in 1928 to take up a lectureship at Harvard. One of the courses he gave was on the theory of atomic spectra; here Millman learned about the most recent developments in atomic physics, including the theory of multiplets. Radiation is emitted whenever the energy level of an atom jumps from an excited level to a lower one. There are a number of ways, depending on the particular atom, in which this energy loss can be accomplished, and each gives rise to a spectral line. Since there are large numbers of atoms in any source, all such lines arising from one excited level may be seen at the same time and the assembly is called a multiplet. The interpretation of the meteor spectrum involved the identification of many multiplets.

While at Harvard Millman began a program of meteor photography. Returning to the University of Toronto, he began to encourage a Canadian program in the observation of meteors, using the techniques he had developed. He published a regular feature, "Meteor News", in the Journal of the Royal Astronomical Society of Canada, and he helped coordinate observations by the different Centres of the Society; Malcolm Thomson and Miriam Burland took a leading role in the work of the Ottawa Centre.

Millman enlisted in the Air Force at the start of the war and eventually became Scientific Advisor to the Chief of Air Staff. In 1946, a meeting was held at Cambridge, Massachusetts, to consider the desirability of a joint program in meteor photography[41]. This was a matter of military interest, since it was a possible source of information on atmospheric density gradients, but we may imagine that the interest was fanned by Millman. "Tentative agreement was reached on a general basis for co-operation in this work between the United States and Canada. Meteor observation stations established in the two countries, would make possible a large latitude spread in the study of the properties of the upper atmosphere; the Harvard Observatory, supported by the US Navy, would take the major responsibility for the development of new meteor cameras; the reduction of direct meteor photographs for heights and velocities would be carried out by MIT; and the headquarters for the reduction of meteor spectra would be in Ottawa. ... After consultation with Mr. R.M. Stewart, Dominion Astronomer, it was agreed that the Dominion Observatory in Ottawa would conduct the Canadian share of this research program, and in July 1946, the writer [Millman] joined the staff of the Dominion Observatory to plan the development of meteor research there."

Within a year, this co-ordinated program was well advanced and at the same time a parallel program of visual, photographic, spectrographic and radar studies of meteors had been put in place in cooperation with the NRC.

Some Preliminary Considerations

Meteors may be separated into two classes – shower meteors and random or sporadic meteors. Shower meteors are believed to be due to debris from comets, some still active, but most broken up long ago. When the earth, in its annual passage around the sun, passes through the plane of the comet orbit it encounters this debris which becomes luminous on entering the earth's atmosphere. Because the orbits, both of the comet and the earth, are fixed, the shower appears at the same time and in the same position in the sky each year. The showers are named after the constellation from which they appear to come, in which their "radiant" lies, to use the technical expression, as Geminid, Lyrid, Draconid, etc. The table below lists the principal showers visible in the northern hemisphere, with their dates and durations.

Annual Meteor Showers

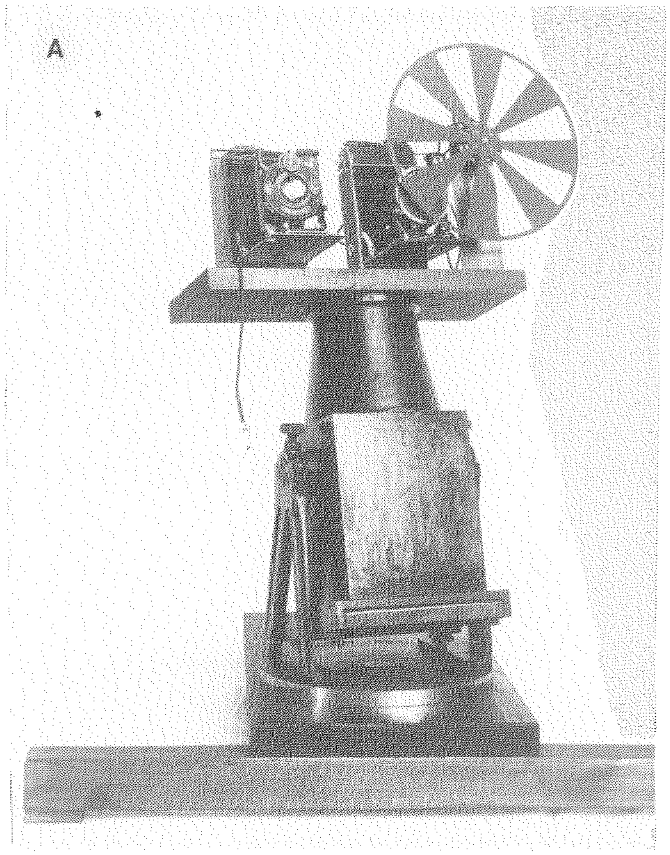
The velocity with which the meteoric particles enter the earth's atmosphere depends on the relative positions of the two orbits and the motion within them. The size of the particles, or "meteoroids", is amazingly small considering the brilliant displays they make. That producing a zero magnitude meteor has a weight of about one gram and a mean density somewhat less than that of water[42].

"Random" meteors, that is those not associated with recognized showers, are believed to derive also from cometary destruction, but from such ancient ones that no trace remains of the orbit.

SHOWER	DATE	DURATION DAYS	STRENGTH	VELOCITY km/sec
Quadrantid	January 3	1	Medium	42
Lyrid	April 22	1	Weak	48
Eta Aquarid	May 5	2	Weak	66
Delta Aquarid	July 29	15	Weak	42
Perseid	August 12	5	Strong	60
Draconid	October 10	1/4	Irregular	22
Orionid	October 21	2	Medium	66
Taurid	November 5	30	Weak	30
Leonid	November 17	1/4 - 2	Irregular	72
Geminid	December 14	4	Strong	36

Meteors are not to be confused with the brilliant "fireballs" that are seen in the sky from time to time, and from which meteorites sometimes reach the earth. These are usually considered to be debris left from collisions between asteroids. These asteroids, which circle the sun in planet-like orbits, have, like the earth, a differentiated structure, with an interior of nickel-iron enclosed in a stone mantle. The debris may come from either core or mantle, which yield respectively the iron and stony meteorites. This "core/mantle" explanation of the two types of meteorites is not universally accepted; some experts believe that the iron occurs as "raisins" distributed throughout the whole "loaf".

Meteor viewing on the roof of the Observatory.



The Ottawa Program

While the joint Canada/USA venture was developing, an important research was carried out in Ottawa in cooperation with D.W.R. McKinley, of the NRC. From the earliest days of ionospheric studies random echoes had been received which, in the absence of other explanation, were attributed to meteors. However no study combining radar and visual observations had been made, and the joint program with the Observatory was designed to settle this fundamental question. The Observatory would observe meteors visually and photographically, the NRC with radar.

The visual program was an extension of the work that Thomson and Burland[43] had begun, under Millman's stimulation, before the war. Groups of observers, usually in teams of six, lay on cots arranged in such a way that each of the observers viewed a different section of the sky. They timed the sightings by a signal on a multipen recorder[44]. Visual sightings yielded meteor positions with an average error of about 3° and a

Left, 1937 version of camera and recording shutter and, **Above,** a more advanced, 1946 version. The 1946 system has five cameras, two spectrographic and three direct, behind a large rotating shutter.

timing error of about one second. Miss Burland organized these groups, which usually included volunteers from the Ottawa Centre of the Royal Astronomical Society of Canada.

The observing programs were coordinated with the principal showers for a number of reasons, the most obvious one being that they presented the most fruitful time for large numbers of sightings. An equally important reason is that since the position of the radiant is accurately known for each shower the angle and velocity at which the meteor enters the earth's atmosphere are "givens" in any computations of path.

In addition to the visual recording, an array of cameras and spectrographs was mounted at each observing session. These were modifications and extensions of those used by Thomson and Burland[43], in which a synchronous motor rotated a shutter in front of the cameras to provide a broken trail in the photographic record. Five cameras, two spectrographic and three direct, were placed behind one rotating shutter that had ten vanes and rotated at 60 rpm[45]. The lenses were thus occulted ten times per second. The relative sizes of the vanes and open spaces were so chosen that the film was actually exposed to the sky for only one-third of the exposure time; this feature made it possible to operate on moonlit nights when the exposures would otherwise have had to be very short.

This array was set to face the direction of the expected shower and the film was exposed for successive periods of 15 or 20 minutes, depending on the numbers of meteors observed. Each exposed film would exhibit a number of traces, either of the meteor trails or of their spectra, which could be identified and timed from the reports of the visual team. Since the meteors were photographed against a background of stars, the position of any section of the broken meteor trail could be determined with an error of about 0.05°.

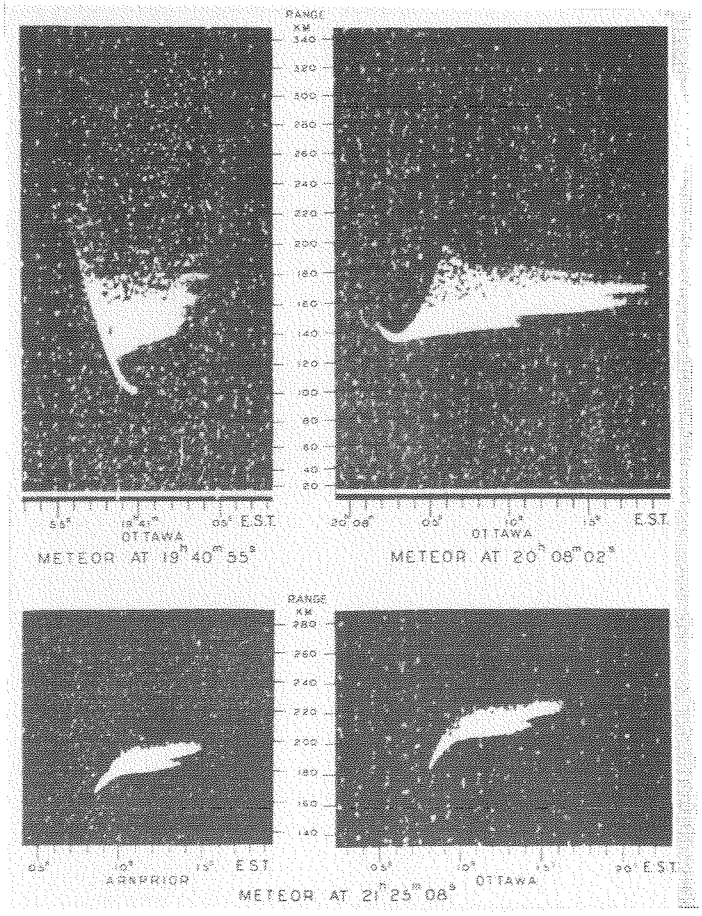
Two teams of observers and cameras were normally set up at some considerable separation, such as at Ottawa and Arnprior. This made it possible to estimate the height of the meteors by triangulation.

The radar systems in use operated at a frequency of 32.7 megahertz, with a peak power of 150 kw and a pulse length of 8 microseconds. The receiver had a sensitivity of 5×10^{-14} watts. A range-time display on a long-delay cathode ray tube was photographed to provide a permanent record[44]. There were normally two systems in operation, one at Ottawa, the other at Arnprior.

Before the joint Observatory/NRC program was well established the Draconid shower of 1946 provided an opportunity for a test run. The story reads like a melodrama[45]. The shower was expected on the night of October 9, and plans had been completed to have it observed visually and photographically at two points, as well as on the 6 and 8-inch photographic telescopes in the refurbished small observatory. On the morning of October 9 it was raining, and the forecast held no promise of improvement. Millman turned to his old colleagues in the Air Force; they placed a Dakota aircraft at his disposal, with permission to fly to whatever airport in Canada seemed to have the best chance of good observing conditions. Weather reports suggested Kapuskasing as a likely site, and the expedition set off, equipped with the rotating shutter mounting and its accompanying cameras and one of the graticules for visual observing. En route the weather pattern modified and they landed at North Bay, where they obtained 209 photographs of 206 different meteors.

The plates were measured at MIT. Using equipment designed for the international project they were able to compute both the mean radiant of the shower and the radiants of many individual meteors. Given the radiant and the meteor's apparent velocity as shown by its broken trail, its true velocity could be computed, and from this the height of the various segments became known. It was possible to measure the deceleration for some meteors. These proved to be high, and from this it was inferred that the masses were 100 times smaller than that of a typical meteor of the same brightness. Finally, by comparing the brightness of the meteors with that of known stars in the field, the magnitudes and the changes of magnitude with time could be established for some of the meteors.

Combined operations began with the observation of the Perseid shower of August 1947[46], the Geminid shower of December, 1947, and the Lyrid and Aquarid showers of April and May 1948[47]. The volume of data obtained was phenomenal: about 3000 visual meteor plots, 65 meteor photographs and more than 100,000 radar echoes.

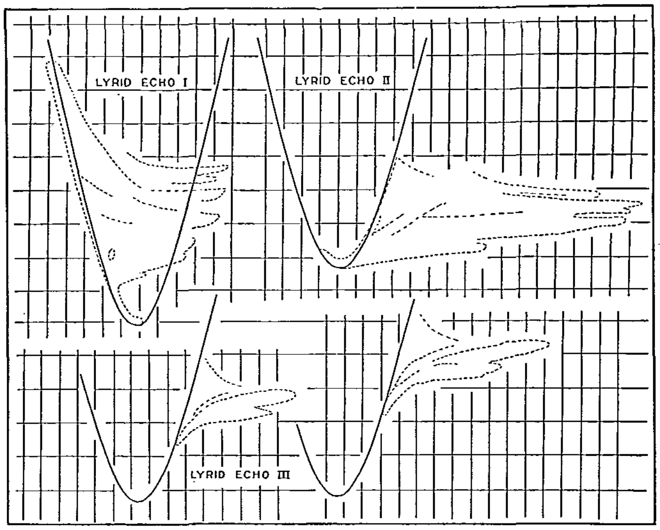


Radar echoes of three Lyrid Meteors, April 21, 1948. From Journal, Royal Astronomical Society of Canada, 42, 121-130, 1948.

The form of the echoes, as displayed on the radar screen, was interesting[47]. Echoes recorded at Ottawa for two events, and at Ottawa and Arnprior for a third, are shown in the figure on the preceding page. In these diagrams the vertical axis represents the "range" of the meteor, that is, its distance from the observing station; the horizontal axis represents the passage of time. As the meteor comes closer and closer, it reaches a minimum distance, and then recedes. If it is moving along a straight line at constant velocity the echo will be in the form of a hyperbola, and this hyperbolic form is clearly seen in the examples in the figure. The two arms of the curve correspond to the approach and recession of the meteor respectively, and the figure shows examples of both.

Visual and spectroscopic observations were available for one of the meteors; the radar echo began when the meteor became visible and continued as long as visibility. This fact, combined with the hyperbolic shape of the echo track, confirmed that its motion corresponded to that of the meteor. While there was no visual confirmation in the case of the other echoes, the shape of the reflections suggested that they too were moving with the meteor. The visual observations for the one meteor, and the double radar observations for another, all suggested that the echoes were centred at a height of 90 km.

The shape of the hyperbola depends on the angle of approach and the velocity of the meteor. In the figure reproduced below the authors plotted the hyperbolas appropriate to a Lyrid meteor echo in solid lines and have indicated the radar echoes, including the complex enduring sections, as dashed lines. The fit of the initial part is clear, but what is the significance of the following echoes? These examples were by no means the most complex; in some cases there were long-lasting echoes at a variety of ranges. These complexities were believed to be related to physical conditions of the atmosphere and a classification was developed to describe them.



Theoretical curves for a Lyrid meteor echo, with the observed echoes superimposed. From Journal, Royal Astronomical Society of Canada, 42, 121-130, 1948.

In a companion paper[44] McKinley and Millman reconsidered the observations obtained in all the showers discussed in the earlier papers. Their first interest was to establish that the radar echoes were in fact coming from meteor trails. To do this they compared the hourly rates of echoes during a shower, the Geminid shower of December 12-13, 1947, with the random rate obtained during a non-shower period of equivalent duration, and found a different pattern for the two. The "shower" pattern would of course contain a random element as well. By statistically "subtracting" one curve from the other they established the true shower pattern. The radiant of the shower, computed using these data, agreed with the known Geminid radiant. There could be no doubt that the echoes were coming from the meteor trails.

They recognized five basic types of echoes, and a number of secondary characteristics; the basic types were shown to depend on the angle between the line of sight and the meteor track, the secondary characteristics depended on this angle but also on the fine structure of the ionospheric layer.

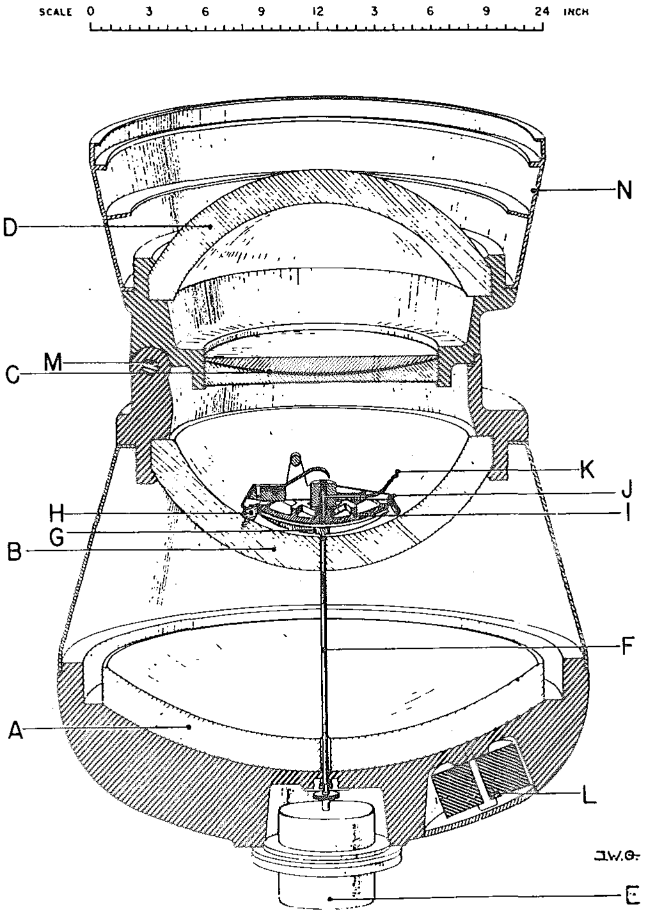
Finally they considered why meteor trails provide radar echoes. They postulate an M (for meteor) region, 15 or 20 km thick and centred at an elevation between 90 and 100 km, that "sustains the ionization caused by the passage of a meteor". The ionization may be caused by a transfer of kinetic energy through collisions of meteoric and air particles, or of radiant energy produced by ultraviolet light from the meteor. Within the M layer there are striae and patches which are responsible for many of the secondary characteristics defined in their classification.

These results were so encouraging that it was decided to set up a three-station radar array[48] during the Perseid shower of August 1948. The stations were at Ottawa, Carleton Place and Arnprior, and they operated continuously throughout a two-week period bracketing the shower. Excellent echo photographs were obtained of a daytime meteor. There were of course no visible observations, but the authors were able to compute not only the meteor path and velocity, but also the meteor's orbit in space. The classification of the echo forms was consistent with the computed path.

Millman[49] reviewed the literature on meteor echoes seeking a further explanation of the ionization which made it possible. He showed that there was a definite correlation between the duration of the radar echo and the brightness of the meteor, reviewed evidence that suggests that the loss of kinetic energy in the meteor is due to loss of mass as it enters the atmosphere rather than to decreased velocity, and recognized the two phases mentioned above, a primary ionization due to kinetic energy transfer, and a secondary ionization probably due to a radiant energy transfer from the glowing head of the meteor. He showed that the explanation of the primary ionization cannot account for the long duration of meteor echoes, often as much as 15 seconds. Perhaps there are zones in the atmosphere particularly favourable for enduring ionization.

Millman and McKinley[50] returned to the question of the relationship between echo duration and visual magnitude using data recorded for 3283 meteors during the field seasons of 1948, 1949 and 1950. They reduced echo durations and visual magnitudes to absolute values, defined as those for a meteor at the zenith at a distance of 100 km, and showed that a straight-line relationship existed between the absolute magnitude and the log of absolute duration. From this relationship they were able to deduce the electron density per cm of path.

Radar echoes unrelated to meteors were noted in the course of the work described above, and they were discussed in a separate paper[51]. A number of them were associated with aurora; they lasted from a few minutes to several hours and were usually at minimum ranges of 300 to 500 km. Six types were recognized but there was no evident correlation between these types and the visual form of the aurora. A second unusual phenomenon, a semi-permanent echo with a relatively constant range between 80 and 100 km, was interpreted as due to reflection from some temporary layering in the ionospheric E layer. Finally the paper reports on a number of meteor echoes of very long duration – of the order tens of minutes. These were attributed to a drifting cloud ionized by the meteor.



Cross-section of the Super-Schmidt camera. A main mirror; B rear glass shell; C correcting plate; D front glass shell; E shutter motor; F shutter shaft; G rotating shutter; H focusing post; I film holder; J film holder hinge, K vacuum line; L counter weight; M hinge for opening camera; N dew cap. From Journal, Royal Astronomical Society of Canada, 53, 15-33, 1959.

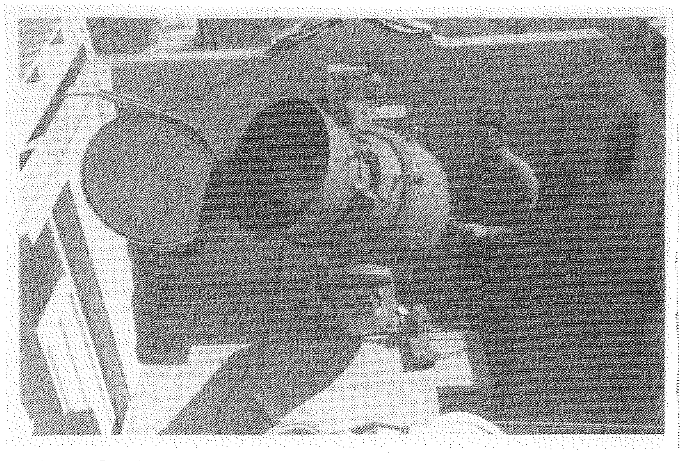
The aim of the joint Observatory/NRC program had now been met; the correlation between meteors and radar echoes had been established beyond question, and its mechanism was understood. Millman published an excellent "state of the art" review[52] about radio observations of meteors.

The International Program

Meanwhile, what of the international program? I follow Millman[41]. "The primary purpose of a direct meteor photograph ... is to provide accurate co-ordinates for the meteor path, as measured against the star background, and to make possible an accurate measure of angular velocity. A secondary aim is the photometry of the meteor trail to give a light curve along the meteor's path and to provide information about the train or wake. Two photographs of the same meteor, taken from either end of a suitable base line, make possible the determination of accurate heights, velocities, and decelerations, and a reasonably good estimate of the absolute luminosity at all points on the photographed path. These observed values ... make possible the determination of atmospheric density gradients over the range of heights where meteors are visible."

The combined US/Canada program was designed to provide a comparison of these atmospheric properties over a large latitude range. In order to provide this large range, the United States instruments were to be placed in New Mexico, the Canadian ones as far north as practicable. Practicability was defined by a number of factors – ease of access, duration of daylight during the summer solstice, but particularly by lack of interference from aurora. Since the auroral zone lies farther north in the west than in the east, a western location seemed indicated, and the existing magnetic station at Meanook provided the ease of access and operation. Ten acres of land surrounding the station, previously leased, were purchased, and the meteor station was placed in the existing complex, 250 feet from the magnetic observatory. The second station, was established at Newbrook, 41 km SE of Meanook.

The meteor camera selected for the project, known as a Super-Schmidt, was designed at Harvard and manufactured by the Perkin-Elmer Corporation, of Norwalk, Connecticut. It follows the normal Schmidt design, combining a concave



A.A. Griffin with one of the Super-Schmidt cameras.

spherical mirror with a correcting plate at its centre of curvature, but surrounds the plate with two hemispherical shells of thick glass, spherically symmetrical with the main mirror. The design retains the high speed of the conventional Schmidt camera but provides an enlarged field of improved accuracy.

The left diagram on the preceding page, taken, with caption, from Millman's paper, shows the details of the design. Note the shutter, which occults the film with a frequency of 60 cycles per second. Note also the film holder, and the fact that the film is moulded to a spherical surface. In order to read the spherical photographs they were re-photographed by a special lens on to glass plates, and read on a measuring machine designed for the purpose. Initially this work was done at Harvard, but by 1953 the Observatory had acquired the necessary lens and measuring machine and the Alberta films were analyzed in Ottawa.

The camera mount, a fork-type equatorial mounting, weighs about 5500 pounds. It was manufactured by the Hartford Special Machinery Company, of Hartford, Connecticut.

Two of the completed instruments were purchased by the Observatory and were installed at Meanook and Newbrook during the summer of 1952. A number of modifications had to be made to the cameras to adapt them to the extreme cold of northern Alberta. A staff of three was provided. In the beginning, in 1951, G.A. Brealey was based at Meanook as the Officer-in Charge of the project, assisted by A.A. Griffin at Newbrook and J.M. Grant at Meanook. Brealey transferred to Ottawa in 1954 to work on the Mirror Transit and was succeeded by Griffin as OIC, with D. Corness as assistant. In 1959 Griffin too was transferred to Ottawa and Grant became the OIC, assisted by T. Chmilar in Newbrook and V.N. Beck, and later A.J. Page, at Meanook.

In addition to the Super-Schmidt cameras, both stations operated conventional aerial cameras equipped with rotating shutters; the mounts for these cameras were built at the NRC[53]. Initially they were used for direct photography, but they were soon adapted to spectroscopic photography with the addition of *replica* transmission gratings. These gratings are made by depositing a thin film on an original master grating; this film, when removed and mounted on glass, is a perfect copy of the master. The gratings were ruled in such a way that, contrary to conventional gratings, they concentrated three quarters of the incident light into the first order spectrum. Halliday[54] has provided an excellent analysis of the cameras and gratings employed in the Meanook/Newbrook array.

Problems with cold weather and an extensive building program delayed the effective opening of the two stations until 1954. By the end of 1957 about 1800 paired photographs had been produced, which included 600 meteor photographs, 165 of them paired. More than 8000 exposures had been made with the spectrographs, producing 41 spectrum photographs, and 3800 meteors had been observed visually.

Meteor Spectra

While the Super-Schmidt cameras provided a continuous review of meteor activity, and permitted the definition of paths for any meteors recorded, it was the arrays of aerial cameras with their replica gratings that produced the more interesting data. The study of the spectra they produced gave detailed information on the materials of the meteoroids and of the upper atmosphere, and some data on conditions in the upper atmosphere.

It is much more difficult to obtain meteor spectra than stellar ones. Since the meteor is moving one cannot make an extended exposure; the intensity of the spectrum depends on the brightness of the meteor and only the brighter ones provide usable spectra. The light from a star is striking the photographic plate normally, but a meteor is streaking across the sky at an angle that cannot be predetermined and the intensity of its image varies with the position on the plate. The sensitivity of the plate varies over the range of wavelengths being emitted by the meteor, and must be adjusted by careful plate calibration. The recording of a satisfactory spectrogram is a matter for considerable satisfaction.

Millman had been collecting data on meteor spectra for many years, and in 1952 he published a table listing all known examples[55]; the first meteor he studied in his work at Harvard is number 5 on the list. The table has been updated in a series of annual appendices[56].

An excellent spectrum was obtained during the 1950 Perseid shower, as part of the work described earlier. Three locations, Ottawa, Arnprior and Carleton Place were occupied during the shower, and while this particular meteor was observed visually and photographically only at Ottawa, radar echoes were obtained at all stations. It was thus possible to determine the height of the meteor throughout its entire path. The spectrogram was obtained using a transmission grating without a rotating shutter. Millman made a preliminary study of the spectrum during which its colour index was determined[57].

A.F. Cook, of Harvard College Observatory worked in Ottawa during the summers of 1952 and 1953[58] as part of the international program. He joined Millman in the main analysis of the spectrum[59]. They made corrections for the position of the image on the film, and for the combined effects of the lower atmosphere, the grating, the optics and the sensitivity of the film on the intensity of the recorded spectra at various wave-lengths. When all corrections had been made it was possible to identify many atoms and ions – HI, NaI, MgI, MgII, SiI, SiII, CaI, CaII, FeI and FeII. From the intensities of the lines, estimates were made of the abundances of atoms in the various excited states. There was no clear evidence of change in the spectrum with height.

The Perseids are among the faster moving of the meteors (60 km/sec); how would their spectra compare with that of a slow moving object? An opportunity to study this question arose when the spectrogram of a slow moving fireball was recorded at Meanook in 1952. Millman and Cook[60] collaborated on its study. In addition to the spectrogram recorded at Meanook, a direct photograph was obtained at Newbrook. It was thus possible to determine the height, velocity and deceleration of the meteor; this work was done by L.G. Jacchia at Harvard College Observatory. The meteor split into two pieces during its period of visibility, and this split made for some difficulties in the identification of the spectral lines.

However, this was accomplished by comparison with a similar spectrum recorded in New Mexico. Lines of MgI, CaI, FeI, NaI, N2, and CrI were identified.

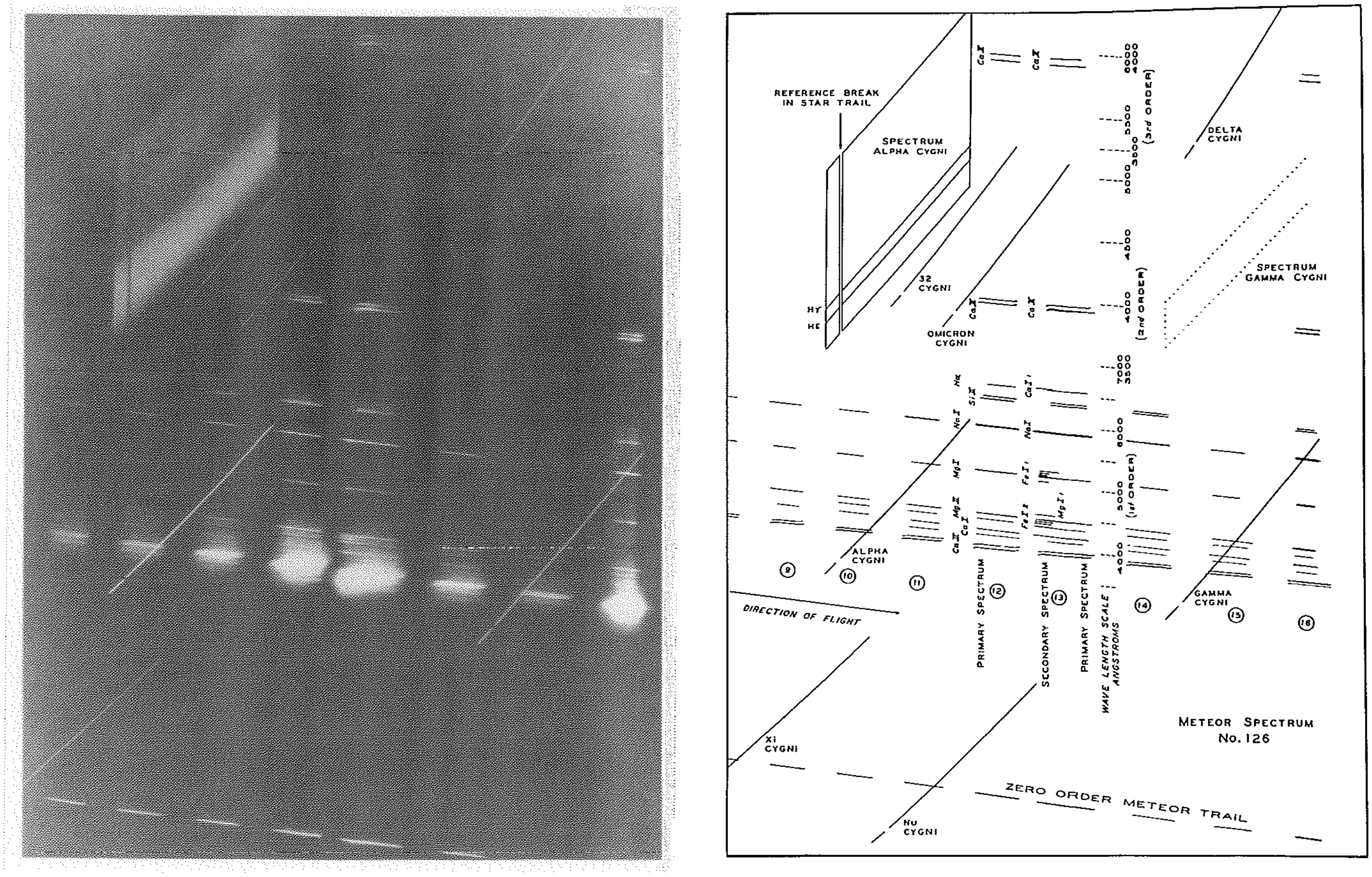
The more interesting part of the work involved the changing magnitude of the meteor. This was measured at nine points along the trail and, after correction for a variety of factors, a light curve was established. It showed irregularities that correlated with the observed break-up of the meteor. From the known magnitudes it was possible to determine the absolute intensity of the continuum.

An excellent Perseid spectrum was obtained at Meanook in 1952 which, in addition to the usual lines showed multiplets of nine neutral elements – FeI, MnI, CaI, SiI, AlI, MgI, NaI, NI, and HI; and of four ionized elements – FeII, CaII, SiII and MgII[61].

By 1953 Millman[62] was able to review the work done on Perseid spectra and to show that all Perseid spectra are similar within the limitations of photographic resolution and emulsion sensitivity. He listed the lines and multiplets found in their spectra, including, for the first time, the spectra photographed in wakes. We shall discuss wake spectra later.

Halliday returned to the question of Perseid spectra in two papers published some years later. In the first[63] he makes a definitive study of the spectra of Perseid meteors, based on the best spectra obtained at the Alberta stations over the years 1958-1960. He identifies 229 lines, covering the region from 3680 Å to 8710 Å; these included lines of NII, OII, and SiII, and probably of SrI, BaI, and BaII. The coverage was less that adequate for both the ultraviolet and the infrared, but Halliday returned to the study of the ultraviolet end of the spectrum in equal detail in a paper published in 1969[64].

A very bright meteor, not associated with any shower, was photographed by the Super-Schmidt cameras at both Meanook and Newbrook on October 20, 1955. From the two photographs Halliday[65] determined the height and velocity of the meteor, and then the radiant and orbit. As expected, the radiant did not agree with any shower radiant, and the orbit, which was similar to that of a comet, suggested a return period of many centuries. The meteor had a spectacular terminal burst; a spectrogram obtained at Newbrook showed a well developed spectrum deriving from this burst with lines corresponding to FeI, MgI, MnI, CaI, NaI, MgII, CaII, and SiII.



The Perseid spectrum obtained at Meanook in 1952. The exposure lasted for 22 minutes. The direct image of various star trails can be seen running from the lower left to the upper right of the photograph. The brightest of them, Alpha Cygni, has produced a spectrum seen as a curtain in the upper left. The meteor came in from the left and its trail is seen at the bottom of the photograph as a line, broken into segments by the rotating shutter. Each section of the trail has produced its own broad spectrum. The chart on the right identifies the lines listed in the text. From Sky and Telescope, 15, 445-448, 1956.

Meteor Wakes

In looking at photographs of meteor trails one may sometimes forget that they are time-lapse photographs and that the image at any instant is a point. Not always a point. In photographs taken with a rotating shutter a luminous *wake* will sometimes appear in the blank areas of the interrupted meteor trail. The wake lasts about 10^{-2} seconds; longer enduring luminous effects, lasting from one to 10^{3} seconds, are sometimes seen; these are called meteor *trains*.

Photographs of both wakes and trains are fairly common, particularly on the Super-Schmidt cameras, but spectrograms of wakes are rare. Millman[66] observed one during the Perseid shower of 1949 and tentatively identified several lines, and he discusses two additional wake spectra in his summary paper on Perseid spectra[62] but observations were not numerous. In a later paper[67], written in 1958, Halliday was able to list only seven known observations. Recognizing that the wake must be produced by material within one km of the main meteoroid, two sources have been proposed: it may be due to trailing dust particles or fragments of appreciable size, or to individual atoms, ions or molecules left behind by the main mass. Considering the lines that have been found in wake spectra, Halliday concludes that the second explanation is the more probable.

In a short note Halliday[68] announces finding the "forbidden" line of OI (λ5577) in three different spectra. This particular line, also known as the auroral green line, is forbidden because it cannot arise as an energy level transition under normal conditions; it demands an environment in which the oxygen atoms can be left alone, without collisions, for a second or more. Its existence in the spectra tells us that this condition obtains at auroral heights. Unfortunately the spectra were obtained without a rotating shutter, and therefore it was not possible to observe the line in the meteor wake, which might have allowed a study of its decay rate.

This initial note resulted in correspondence with other investigators in which four additional examples of the forbidden line were announced. In the meantime five additional examples had been found in spectrograms produced at the Alberta stations. Halliday discusses all twelve spectra in a major paper[69]. All the spectra were produced by fast-moving meteors: there were "four Orionids (66 km/sec), seven Perseids (60 km/sec), and one Lyrid (48 km/sec)". The green line was produced only during the initial part of the trail, at heights between 120 and 95 km; when the line was found at the lower heights in this range it was always at, or just after, times of increased solar activity. Because the new spectra had been obtained with the occulting shutter it was possible to study the appearance of the line in the meteor wake; it did not suffer any apparent reduction in intensity in the $6x10^{-2}$ seconds of the occulted period, which put a limit on the decay rate of the ionization. Halliday discusses the implication of all these findings in attempting to define the excitation mechanism, but concludes that it is obscure.

In order to study the question further Halliday and Griffin designed a new camera[70] that permitted several photographs of the spectra to be obtained during its decay period. The film could be made to "jump" a small amount as often as 150 times a minute. The different photographs appeared one after the other on a single film and produced a sort of moving picture of the line. Because the green line is in an uncluttered area of the spectrum, the successive lines could be distinguished even with this small separation.

Some years later W.A. Gault[71] analyzed the data obtained with the new cameras. He found that a finite time (0.1 to 0.3 sec) is required for the radiation to reach its peak, and that the decay rate increases as the meteor enters more dense atmosphere. This is consistent with the increased rate of de-exciting collisions.

In the next paper to be considered, Halliday[72] studies the spectrum of what is clearly an asteroidal meteor. The zero-order spectra at both Meanook and Newbrook were sufficiently developed to permit the calculation of the meteors track through the atmosphere, and of its radiant and orbit. The orbit, which was similar to a planetary orbit, was clearly asteroidal.

The spectrum contained only lines of neutral iron, and a weak continuum; even the D line of sodium was absent. It is usually present in meteor spectra, and it had been assumed that this was because of the presence of atomic sodium in both the meteor and the atmosphere. Its absence from this particular spectrum is a puzzle.

Much later it was discovered that the event had been recorded on the Super-Schmidt cameras. Griffin[73] redefined the orbital elements of the meteoroid, studied its varying magnitude and its deceleration. His study supports Halliday's conclusion that the meteoroid was of iron and of asteroidal origin.

One of the projects proposed for the IGY was a count of meteors, and an international program was drawn up defining the periods of observation. To meet this requirement the radar/visual/photographic program of the Observatory and the NRC was reinstituted during the period July 1, 1957 to December 31, 1959[74]. In addition, Millman managed a much larger program in which amateurs from all over the world observed regularly and reported their meteor counts to him. This larger program was so successful that it was continued for many years beyond the close of the IGY. The Observatory withdrew permanently from the observing program at the end of 1959[75].

These IGY observations led to an interesting study of the infrared spectra of meteors. Such spectra were obtained only rarely; Millman had been the first to record them, in 1950. By 1960, largely through the IGY work, nine such spectra were available and Millman and Halliday[76] discuss all nine, eight Perseids and one sporadic. A total of 15 atomic emission lines are identified in the infrared region, due to NI, OI, and CaII. No infrared lines have been observed in slow meteors, although only one such meteor had been observed in the infrared.

The spectrum of a spectacular Geminid meteor was photographed at Meanook on the night of December 12/13, 1960. Although the meteor was observed at only one station, Halliday[77] was able to determine its initial height and velocity within reasonable limit, as well as its deceleration. A total of

95 features, individual lines or blends of lines, were identified. There was a variation in luminosity with a period of some 300 c/s; this was attributed to the oscillation of the meteoroid. The wake spectrum was well observed and most of the strong lines were split into two. This suggested an almost explosive expansion of the meteoric column in the form of a thin hollow cylinder.

In a paper presented following the 1967, Prague, meetings of the IAU, Halliday[78] considered the influence of the orientation and duration of the meteor trail on the spectrum obtained. A bright Perseid meteor spectrum had been photographed at Meanook through a rotating shutter, and at Newbrook without a shutter. The meteor flared several times, and the position of the flares relative to the shutter had a strong influence on the intensity of the various lines, as shown by comparison with the Newbrook spectrogram. Intensity ratios of as much as 6:1 were found between the two stations. When the light was occulted at the appropriate time it allowed the spectrum of the flare to be recorded without contamination from the wake; at the other times the reverse occurred and a pure wake spectrum was obtained. One particularly pure wake spectrum showed 28 multiplets, some of which decayed rapidly, others which were more long-lasting. Halliday designed a two-dimensional classification for these multiplets – according to duration and to intensity. There was strong evidence that the wake was produced by a coasting gas rather than by trailing droplets or particles.

The Search for Meteorites

The Division was alert for any opportunity to retrieve meteoritic material, and for this reason surveys were always made when the appearance of a particularly bright fireball suggested that a meteorite might have reached the earth. The first meteorite fall of our period occurred at Benton N. B. on January 16, 1949. There was a field investigation and a section of the meteorite weighing 1340 gms was purchased for the Geological Survey[79]; later a second section weighing 1500 grams was discovered and this was purchased by the Observatory[80]. Another interesting fall occurred at Abee, just south of the Newbrook Meteor Observatory, on June 10, 1952, shortly after the station was opened. It was dug up by the local school teacher, acquired for scientific study through the initiative of the station operator, A.A. Griffin, and purchased for the Geological Survey[42].

Shortly after this fall Millman began a catalogue of Canadian meteorites as part of his "Meteor News" published in the Journal of the Royal Astronomical Society of Canada[81].

Brilliant fireballs were observed in the Montreal-Ottawa area on November 2, 1950[82], and north of Parry Sound on January 13, 1954[83]. In both cases data on sightings were collected through the cooperation of radio and press, but no meteoric material was found in either case. A more successful operation involved a fireball generally visible over northern Alberta, which resulted in a shower of stony meteorites scattered over an area of several square miles near Bruderheim, Alberta[84]. Many fragments, totalling over 300 kg, were recovered.

While the staff at the Alberta Meteor Observatories assisted in the investigation of this fall, the find was made possible by the enthusiastic cooperation of amateur astronomers from the Edmonton Centre of the Royal Astronomical Society of Canada and scientists from the Research Council of Alberta and the University of Alberta[85]. Impressed with this, the National Research Council established an Associate Committee on Meteorites, with fifteen members giving a broad national coverage, to coordinate future meteorite searches.

Over the next seven years there were three more meteorite falls within 500 km of Edmonton: at Peace River in 1963[86], near Revelstoke in 1965[87], and at Vilna in 1967[88]. All of these were investigated by the Edmonton group. The fact that four falls occurred in such a limited area had an important implication for the future of the science, as we shall presently see.

The effectiveness of the Associate Committee program for the study of fireballs was tested on the night of April 25, 1966, when a fireball, initially identified as a UFO, was reported by Canadian Forces Headquarters. A request was made through the local newspapers and radio stations for information about sightings, and these were evaluated by interviewing teams from the committee, consisting of scientists from the NRC, the Geological Survey and the Observatory. Analysis showed that the meteorite entered the atmosphere between Baltimore and Philadelphia and, moving north, crossed the Canadian border a few miles west of Huntington, Quebec. McIntosh and Douglas[89] analyzed all the data, described the appearance and sounds of the fireball in detail, and established its trajectory and height and its probable point of impact. A number of photographs of the fireball and its trail, including a motion picture, had been obtained in the United States. Combining measurements from these photographs with the data obtained from the visual observations, Griffin[90] was able to measure the velocity of the fireball – 17 km/sec – and to establish its orbit. Both the orbit and the velocity were consistent with an asteroidal origin.

A brilliant fireball was visible over southwestern Ontario on the night of September 17, 1966. A press photographer made an excellent photograph of the event at Guelph. Using this photograph and visual data, Halliday[91] was able to define the path closely. He showed that any meteorite that survived the passage through the atmosphere would have been lost in Lake Huron. He was also able to compute the velocity of the fireball – 17.1 km/sec – again typical of an asteroidal origin.

We have noted earlier the four meteorite falls that occurred within 500 km of Edmonton over a seven year period. The Associate Committee on Meteorites was encouraged by this to recommend that a network of photographic stations be set up in western Canada to record bright fireballs. The primary aim of the network would be to provide accurate data on the path of the fireball in the atmosphere and so to permit a prompt determination of its probable point of impact. The multiple photographs of the complete meteor trail would also permit a calculation of the orbit of the meteoroid. Further, the all-sky, continuous nighttime coverage would provide statistics on the numbers and orbits of bright meteors.

The MORP network. From Journal, Royal Astronomical Society of Canada, 72, 18, 1978.

In 1961 the IAU had passed a resolution calling for the establishment of photographic fireball networks and two had been established – in Czechoslovakia and the United States. The Canadian one, designated MORP for "Meteor Observation and Recovery Project", was the third such network to be established.

Halliday *et al* have described the network[92], which consisted of 12 stations spread over the southern prairie provinces as shown in the figure above. Each station was equipped with identical instrumentation, consisting of five cameras, each covering about 54° of azimuth near the horizon and from 1° to 55° in elevation. The cameras were housed in a pentagon-shaped building, shown in the figure in the right column, raised high enough above the ground to avoid turbulent motion of dust or snow. The cameras were of special design, using wide-angle lenses, and they operated behind chopping shutters to provide the usual broken trail.

Inside the building there was a steel structure which carried the floor and a frame to which the cameras were attached. This structure was separate from the building and was supported on a central pillar extending deep into the ground to provide stability. Each camera viewed its section of sky through a transparent window, seen in the sloping section of the roof in the figure. The building is flat-topped and is surmounted by a meteor detector.

In order to time the occurrence of each meteor photographed it was necessary to detect them. This wasn't easy since, while the meteor trails appear bright, they do not raise the total sky illumination appreciably. The meteor detector was designed to recognize the passage of a meteor, to record its time, and to advance the film. It consisted of two coaxial cones of perforated metal, one larger and completely covering the other. When a meteor flashed across the sky, its light found a variety of combinations of these holes through which to pass. It was detected by a light-sensitive photo-multiplier that provided a signal to print the time on the film and, after a suitable pause to let the meteor complete its passage, advanced the film. The meteor detector was built by Spar Aerospace, of Canadarm fame.

The network was supervised by a headquarters at the University of Saskatchewan, directed by Alan Blackwell, assisted by a technician, Norman Dyrvik. They inspected each station about once a month but the routine operation was carried out by local residents who visited the stations about twice a week. At each visit tests were performed to ensure that the systems were functioning properly and, when necessary, the film was changed in each camera. This came in 100-foot rolls and normally lasted from three to six weeks. Exposed film was sent to the network headquarters where each frame was examined both for meteors and for sky conditions, the data being entered directly on a computer terminal. If eyewitnesses reported a meteor of special interest, operators within the area of sighting could be instructed to change and forward the film immediately.

The network was in place by the end of 1970 but it was not completely operational until 1971; its accomplishments thus lie in the post-transfer period. However, a brief account will be of interest. The network operated effectively over a period of fourteen years but in all that period only one meteorite fall was found. This occurred on February 5, 1977[92]. The associated fireball was widely observed over Alberta and was recorded on two of the MORP stations. From these records it was possible to determine a well-defined orbit for the meteoroid and to determine its point of impact. A two-kg meteorite and eight smaller pieces were recovered at Innisfree, Alberta. Three years later, on February 5, 1980, a fireball having an orbit almost identical with that of the Innisfree fall was detected by the network. A fall is believed to have occurred but no meteorite has yet been recovered[93].

Another spectacular fireball on February 22, 1984, was photographed on seven cameras at four stations; the orbit and probable point of impact were determined but no meteorite has yet been found[94].

While only one meteorite has been found as a result of the MORP program Halliday (Personal Communication) points out that this should not be regarded as "a major disappointment. In one sense this may be true but once we appreciated

A MORP station at Neilburg, Saskatchewan. From Journal, Royal Astronomical Society of Canada, 72, 19, 1978.

the difficulty of finding them, it became a major triumph to find even one". MORP did as well as the other detection networks, and the Canadian find was in many ways the best documented.

The network has provided interesting information on the frequency of meteoric fireballs and of meteorite falls. During the 30% of night hours that are essentially clear "the network recorded one or two fireballs per week ... and, over a nine-year period, observed the fall of a small meteorite about once in two months". ... The interval between ... 'great' events is typically a few years rather than months"[93]. Two sophisticated statistical analyses[95] have been published, which provide detailed information on the distribution of meteorites in time and in location.

On February 5, 1977 a large fireball was observed over Alberta. Photographs were obtained at two stations of the MORP network, from which a point of impact was calculated. A search was mounted, using snowmobiles, and a 2-kg meteorite was found near Innisfree, Alberta. The meteorite is seen in front of the third snowmobile from the left. From Journal, Royal Astronomical Society of Canada, 72, 33, 1980.

The search party, with the meteorite. The four MORP staff are in the back row – Eldon Hubbs, Alan Blackwell, Ian Halliday and Arthur Griffin; in the front row, two youths from Vegreville who assisted in the search. NRC photograph.

The network has made an important contribution not related to astronomy: it has shown that, contrary to popular belief, the sky in western Canada is not particularly clear or cloud free. In the two and one-half years from 1974 to 1976 the skies were completely cloudy 63% of the time, partly cloudy 20% of the time and cloudless for only 17% of the time[93].

SOLAR PHYSICS

Some Preliminary Considerations

When we look at the sun in white light we see a brilliantly emitting surface with a well-defined limb; this surface is called the *photosphere*. With a powerful solar telescope, on a day of good seeing, the surface appears grainy, with bright granules separated by darker intergranular lines. It is on this surface that sunspots are normally observed.

During an eclipse, at the instant when the photosphere is covered, the sun appears rimmed with a narrow red band, above which ruby red prominences shoot up like flames. This is the *chromosphere*; it is, of course, always present, but is normally outshone by the sun's brilliance. With a powerful telescope the red rim is seen to consist of individual spike-like jets, called *spicules*.

As the eclipse becomes total the chromosphere disappears, except perhaps for the peaks of the highest prominences, and the *corona* becomes visible, pale white in colour. The shape of the corona varies from one eclipse to another, being more symmetric at the time of sunspot minimum.

Since the corona and the chromosphere come between us and the photosphere, we may wonder why the granules are visible, and why the limb of the sun appears so sharp. It is because the corona and the chromosphere are transparent to the white light of the photosphere; the limb marks the point where the solar atmosphere becomes so dense that transparency ceases.

Great advances have been made in the understanding of the sun since the days covered in Part I[96]. Many of these have been made possible by the development of techniques for studying the properties of the sun at discrete depths within the solar atmosphere. One way of doing so is by means of a spectroheliograph, a spectrograph which can be tuned exactly to a desired wave-length. If a picture of the sun is taken with the spectroheliograph wave-length set exactly to that of the Hα line, one obtains a picture of the Hα clouds at a height of about 1000 km; the material below these clouds is invisible. If the wave-length is slightly displaced from the centre of the line, some of the light coming from below the Hα clouds is seen. The more the wave-length is displaced, the deeper the penetration until, when the setting has been displaced to the continuum, the photosphere becomes visible.

The same results may be attained through the use of filters which transmit only a very narrow range of wave-lengths; filters now exist with band-passes as narrow as 0.1 Å. Filters that correspond to the exact wave-length of an absorption line may of course be used to study the properties of the obscuring gas in the light of its emission; the H filter which we shall presently discuss is one such filter.

Another instrument that has led to major advances over the years is the solar magnetograph. The spectrum given off by a hot gas, such as that in the sun's atmosphere, is changed if the gas is in a magnetic field. The spectral lines are shifted slightly and sometimes split by the magnetic field; this is known as the Zeeman effect. Given a high-dispersion spectrograph, the shift in the spectral lines, and their splitting, can be detected and used to determine the strength and polarity of the magnetic field at any point on the sun's surface.

Making use of these techniques solar physicists have now shown that the granules on the photosphere and the spicules of the chromosphere are due to convection systems, symptoms of larger, underlying, convection cells that transmit energy from the deep interior of the sun. They are in constant motion, moving up and down at speeds of as much as twenty km/sec. Other motions of the solar surface have also been established, including meridional ones. These motions all affect the Doppler shift of the spectral lines. It is not remarkable that DeLury was confused with inexplicable line shifts!

The solar magnetometer has shown that most of the processes observed on the sun are controlled by strong magnetic fields. For example, sunspots normally occur in pairs, one magnetized positively, the other negatively. This suggests that a magnetic loop connecting the two spots exists within the sun. The fields are so strong, 10,000 times that of the earth, that they quench the granules and their underlying convection cells. Energy dissipated in the sunspots produces glowing emission clouds high in the chromosphere.

The prominences seen on the chromosphere are made up of threads of H alpha emitting gas. Sometimes material falls back into the main body of the chromosphere, but it is replenished, and the prominence is maintained, sometimes for weeks at a time, high above the solar limb. The solar magnetometer shows that the prominences are supported by very strong magnetic fields; sometimes, in the collapse of these fields, material is flung outward from the chromosphere to supply material to the corona.

Solar flares are "explosions" that occur in the high chromosphere or the low corona due to the sudden release of energy stored in the magnetic field of an active solar region. They are associated with the more active convective areas in the solar surface, and are therefore, frequently but not necessarily, associated with sunspots. Noyes draws an analogy to the release of the electric energy stored in a thunderhead as lightning. Because of the altitude of the flare it is not usually visible in white light; hence the Hα filter to which we shall presently turn. It is the discharge from large flares that is responsible for magnetic storms and that disrupts communications and causes aurora.

The corona consists of a hot ionized gas containing free electrons; the light of the corona is sunlight scattered off these electrons. The electron density of the low corona represents a vacuum about as complete as the best attainable in the laboratory, and this density drops off rapidly with increasing distance from the sun. Spectral studies of the coronal light have confirmed the fact, based on other evidence, that the temperature of the corona must be in excess of 1,000,000°K. Its spectrum is dominated by three lines: a green line at 5303 Å, a red line at 6374 Å, and a yellow line at 5694 Å. These lines were not found in any other source and for many years it was thought that they derived from a new element, tentatively named "coronium". Finally they were identified respectively as due to thirteen-times ionized iron (FeXIV), nine-times ionized iron (FeX), and fourteen-times ionized calcium(CAXV). These lines could only be produced at a very high temperature.

Introduction

By the times Beals arrived in Ottawa the solar research program was essentially dead, although John O'Connor took regular sunspot photographs. These photographs formed the basis of cooperation with the National Research Council where A.E. Covington was beginning to apply radio astronomy to studies of the sun. Covington observed a partial eclipse of the sun on November 23, 1946 and, by using a series of photographs of the eclipse obtained at the Observatory[97], was able to relate variations in solar noise to the passage of the moon over the solar disk.

It is difficult to understand why Beals revived the solar program[98]. He must have known of DeLury's difficulties and frustration. Much of this was due to solar complexities which no one yet understood, but much was also the result of the location. The solar telescope lay to the north of the Observatory, which blocked the sun from it for much of the year; the nearness of the Observatory and of other buildings made for much atmospheric turbulence and consequent poor seeing, a condition that had worsened over the years; the laboratory space available for the spectroscope was small and inconvenient. Why not abandon the program?

There were probably two reasons. If Beals was to revive astronomy in Ottawa, he had to work with what was available. The solar telescope may not have been a great hope, but it was a better one than the 15-inch equatorial. A more probable reason is the influence of his old friend, and brother-in-law, R.O. Redman, who was Director of the Solar Physics Observatory at Cambridge. Redman was involved in a controversy about the structure and the temperature of the chromosphere and would undoubtedly have welcomed additional input to its study. It is significant that shortly after his arrival in Ottawa Beals was negotiating for the purchase of a Lyot H filter, the obvious purpose of which would have been to study the chromosphere and its flares. It is also significant that, in recruiting someone to take over the program, Beals selected J.L. Locke, whose doctoral thesis had involved infra-red spectroscopy.

Beals must also have been influenced by discussions with his old friend Gerhard Herzberg who joined the NRC and set up his spectroscopic laboratories at about the same time that Beals was developing the Observatory program. Through that laboratory, and its visitors, Locke was kept informed of the latest developments in filters, which was useful to him in the redesign of the Observatory spectrograph.

Infrared Spectroscopy

Locke joined the staff in the spring of 1949 and immediately began to modernize the instrumentation[99]. The old rotational spectrograph was discarded and a new one built. Sunlight from the coelostat was reflected to a grating by a collimating mirror; a second mirror reflected the spectrum to the camera. The two mirrors were of 20-foot focal length. The telescope produced a solar image 23 cm in diameter, and a guiding system made it possible to place any part of the solar image, in any desired direction, over the slit of the spectroscope. This was an improvement over the original design, in which the entire spectrograph rotated on its axis to present different diameters to the slit; the difficulty of restoring the spectrographs optical performance after such a rotation was another of the problems that had plagued DeLury[100]. The spectrograph was enclosed in an insulated case. Different gratings could be used, depending on the region of the spectrum to be studied. Lead sulphide and lead telluride filters, refrigerated with liquid nitrogen, served as detectors in the infrared. By chopping the light at 1080 cycles per second, an alternating current was produced; this was amplified and passed to a Brown electronic recorder, which provided automatic photo-conductive recording for the infrared region of the spectrum. The spectrum could be scanned by rotating the grating. The new spectrograph was completed and placed in full operation late in 1951.

The Observatory was fortunate in being able to obtain the services of Dr. Louise Herzberg, who was precluded by government regulations from working in the same laboratory as her husband. The first work done with the new spectroscope, in which she and Locke collaborated, was a study of the infrared absorption by carbon monoxide in the earth's atmosphere[101]. Most molecules have well-defined regions of absorption; the regions for carbon monoxide centre on 2.4 and 4.7 microns [1 micron = 100 Å] and these regions were scanned using suitable detectors. Many absorption lines were recorded in both regions and these were explained within the theories of molecular spectra. The intensity of one of the lines in the 4.7 region was compared with that of the equivalent line obtained in laboratory measurements to define the abundance of carbon monoxide in the atmosphere, and this was used to monitor the variation of carbon monoxide over a period of several months in 1952. This variation could be correlated with meteorological conditions in only one case. A reexamination of data from other parts of the world suggested that all observations lay within the limits of the Ottawa values.

The General Theory of Relativity predicts a shift of solar spectral lines, proportional to the wave-length but independent of the position on the solar disk. In searching for the effect the shift due to the sun's rotation must be allowed for. Earlier studies suggested that the limb values closely approximate the predicted value, but that an additional wave-length shift to the violet perverts values elsewhere on the disk. There was no agreement on the cause of this shift. Mrs. Herzberg undertook to investigate the problem. The research aimed at providing additional data by measuring the shifts at infrared wave-lengths where the total displacements might be expected to be large.

The ability of the telescope to present any part of the disk to the spectroscope slit was an asset to the experiment; measurements were taken at the centre, and at opposite ends of diagonals 22.5° apart. Six infrared lines, belonging to two multiplets of SiI, were studied, and it was found that the shift varied from one line to another of the multiplet. It was thus dependent on the energy level of the line, which suggested that the observed variations were due, at least in part, to collisions or field effects.

Mrs. Herzberg returned to the subject[102] with observations made during the summers of 1958 and 1959, extending her observations to include FeI and CaII lines. Again the shift varied from one line to another, and it was impossible to confirm the shift predicted by Relativity.

Two postdoctoral fellows, both from Italy, worked in the Solar Physics section. The first was G. Godoli. He measured the equivalent widths of four iron lines in the infrared spectrum at a variety of points on the solar radius. He corrected these observations by applying an "apparatus function" determined by laboratory measurements of the CH_4 and CO_2 bands in the same region. He showed that the Ottawa values compared reasonably well with published ones, but were not adequate to permit a study of anisotropic variations across the solar disk[103].

Godoli was followed as a postdoctoral fellow by Mario Rigutti. His study involved the positive identification of certain lines in the solar spectrum believed to be associated with the CN molecule. By comparing the lines with CN red lines produced in the laboratory he was able to establish the identity of 38 previously unidentified lines[104].

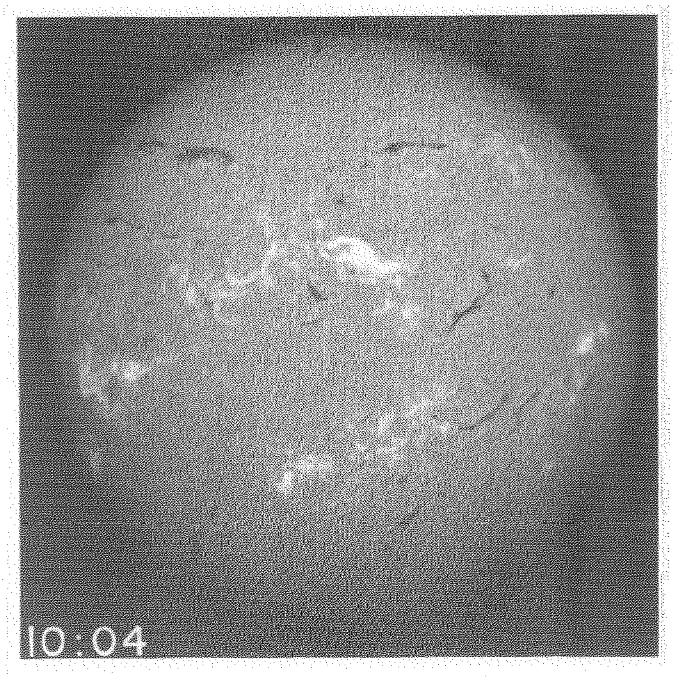
In 1955, when Millman left the Observatory to become head of a section on Upper Atmosphere Research at the NRC and Locke succeeded him as Chief of the Stellar Physics Division, Victor Gaizauskas joined the staff. Like Locke, his doctoral thesis had involved infrared spectroscopy. He became increasingly responsible for the solar program, ably assisted by technician Anton Kryworuchko.

The Solar Flare Program

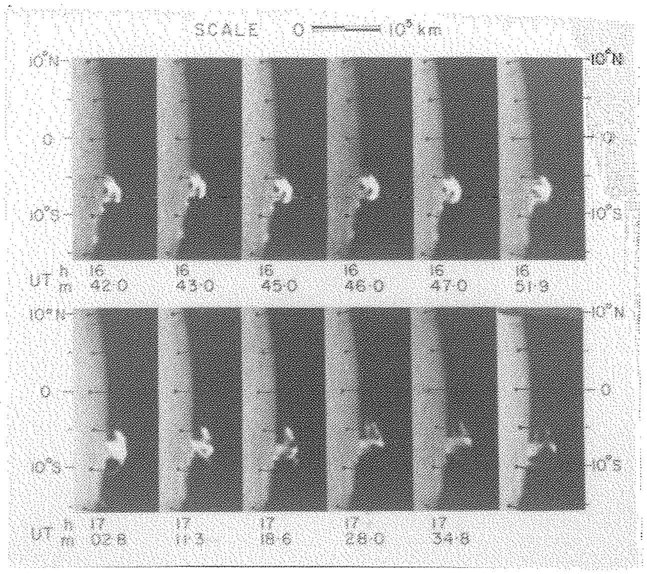
Gaizauskas' first task was to prepare for Observatory cooperation in an IGY study of solar flares. Since the sun influences conditions on the earth in so many ways, solar studies were an important part of the IGY, which coincided with a sunspot maximum. A large number of observatories throughout the world collaborated, under the direction of the Comité Spécial de l'Année Géophysique Internationale (CSAGI), to make a systematic study of solar eruptions and their influence on such terrestrial phenomena as magnetic storms, aurora and radio fade-outs. The solar flare patrol was an important part of this program.

The heart of the solar flare recorder was the Lyot monochromatic filter that Beals had ordered shortly after coming to Ottawa. As we saw earlier, the flares lie in the low corona which may best be observed in the light of the H line, and the filter transmitted light at this wave-length with a band width of 0.75 Å. It was illuminated by a small refractor, equipped with a 35-mm time-lapse camera, and mounted on the 15-inch equatorial telescope, which was used as a platform to follow the sun in its daily motion. It was placed in almost continuous operation on clear days from September 1955 on, but the first year of its operation involved a great deal of experimentation with different photographic materials and the installation of additional equipment aimed at making the operation automatic[105]. These modifications had been completed by the beginning of the IGY in July 1957. Timed photographs of the sun were taken at short intervals throughout every clear day. The observations were reported to IGY Data Centres, where they were available for comparison with magnetic and ionospheric data[106]. This program was extended beyond the IGY and observations covered most of the period 1957-1966; for the final three and a half years it was funded in part by a grant from the US National Aeronautics and Space Administration. During this period, time lapse photographs were obtained of 1800 flares and 350 other transient chromospheric events[107].

The solar flare program was primarily a geophysical one, and there was little analysis of the photographs by solar physicists. I have found only one paper deriving from the Ottawa observations[108]. A small but well defined increase in cosmic ray intensity was observed on July 20,1961 and was recognized as coinciding with a flare surge that had occurred on the western limb of the sun. The flare had been well-documented in the patrol photographs, and Gaizauskas and Covington were able to correlate its form, frame by frame, with bursts of activity recorded on radio telescopes in Texas and at Algonquin Park, and with changes in the cosmic ray level.



A large flare, August 22, 1958.



Sequence photographs of a flare on the Sun's limb, July 20, 1961. Times, to the nearest 0.1 minute, are given for each photograph. This flare produced ground-level cosmic rays.

Later Spectroscopic Development

When his responsibilities to the solar flare program permitted, Gaizauskas began a study of the spectrum of carbon monoxide in the solar spectrum. The window in the earth's atmosphere through which to view this spectrum was limited; it was bracketed on either side by spectra of water vapour and carbon dioxide. The poor site made progress difficult, and no publications resulted.

A new problem was suggested by McKellar during a visit to Ottawa. As we have seen, McKellar was interested in the molecular spectra of cool stars. He suggested that a comparison of molecular spectra from the cool centre of sunspots with those from the hotter areas outside the spots might cast some light on the atmosphere of the cool stars. If the lines studied included those for C_{12} and C_{13} it might be possible to determine how temperature affected the ratio of these isotopes and so reflect on nuclear processes in the sun.

The attempt to make these observations showed how inadequate the telescope was and how inferior the site. The guiding mechanism for the spectrograph, which was adequate for studies of larger areas of the solar surface, was unable to place the slit of the spectrograph firmly in the centre of the sunspot. Even if the slit were centred, turbulence in the atmosphere might bring in light from outside the sunspot. Improvements were made in the guiding mechanism but reliable results could not be obtained. The effort was useful, however; it forced the realization that the future of solar research lay not in studies of the general solar surface but in its minutiae.

The Solar Magnetograph

By this time the importance of magnetic fields in the solar process was realized and observatories were increasingly turning to their study. Gaizauskas decided to adapt the equipment to a solar magnetograph. This would measure the magnetic field by observing the Zeeman effect on the solar spectra in the infrared region, where the effects are much stronger than in the visual. The magnetograph was designed to record automatically and to scan an active region in the sun in a sufficiently short time that changes in the magnetic field associated with flares could be followed. The instrument was put into operation in 1965, but problems plagued it for many years.

Gaizauskas has detailed these problems in a personal communication[109]. First, "retardation plates", which operated under high voltage to measure the retardation between + and – quarter waves, broke down repeatedly due to the high humidity of the underground tunnel in which the spectrograph was housed. Secondly, the thermal enclosure around the spectrograph, which had provided adequate control for photographing spectra with exposures of seconds, proved quite inadequate for recording the minute displacements of lines that could vary position within a second. These temperature effects were minimized by the use of fans to establish a quasi-laminar air flow through the instrument case, but the problem of the failing retardation plates remained.

Gaizauskas decided to modify the instrument and to consider an important problem within its new capabilities. It had been established that the photosphere is not at rest, but is oscillating with a random radial motion with a period of about 300 seconds. Did the strong magnetic field within a sunspot inhibit this oscillation? Opinions differed, but most studies suggested that it did. Rice and Gaizauskas[110] studied this question.

The instrument was redesigned to operate somewhat like the spectroheliograph described earlier, except that two secondary slits were provided. They could be adjusted to examine two narrow sections in the wings of a spectral line. If motion is occurring in the line of sight it should be out of phase on the two wings. By measuring the radiation through the two slits, and taking their difference, a signal was obtained showing the true Doppler effect. This was recorded as an analogue signal on magnetic tape for subsequent computer analysis. The sun's surface, in the sunspot and in its surrounding area, was scanned by translating the concave mirror of the coelostat in two ortho- gonal directions. The scanning aperture was 1.6"x4.0", and the scanning of an area 80"x85" was accomplished in forty seconds. This scanning speed was necessary because of the rapidly changing field.

The study found that the 300 second oscillation existed within the boundaries of the sunspot and thus that it was not inhibited by a strong magnetic field. This was an important step in understanding the details of the sun's structure and the forces controlling it. In addition, oscillations of both shorter and longer periods were found, in particular one with a period of about 180 seconds.

This was the last work done with the solar telescope. A new, modern, instrument was necessary if solar research was to continue, and the Observatory campus was not the place for it. The location of the telescope to the north of the Observatory had always limited its usefulness, and the steady encroachment of tall buildings made matters worse. A new era had begun, as we shall now see.

The Establishment of Modern Instrumentation

The first step in the new program was to arrange for Gaizauskas to spend a year at the Kitt Peak National Observatory, near Tucson, Arizona. Here the McMath Solar telescope, the most advanced in the world, was available for research, and the staff who had developed it were available for discussions. Gaizauskas returned to Ottawa with clear ideas of what was needed. No site in Canada could equal those in the deserts of southwestern United States, and the program must recognize this. Instead of studies of the corona, research would concentrate on the study, through cinematography, of the active regions of the photosphere and chromosphere, to study those small structures that were believed to play an essential role in transferring energy in the solar atmosphere.

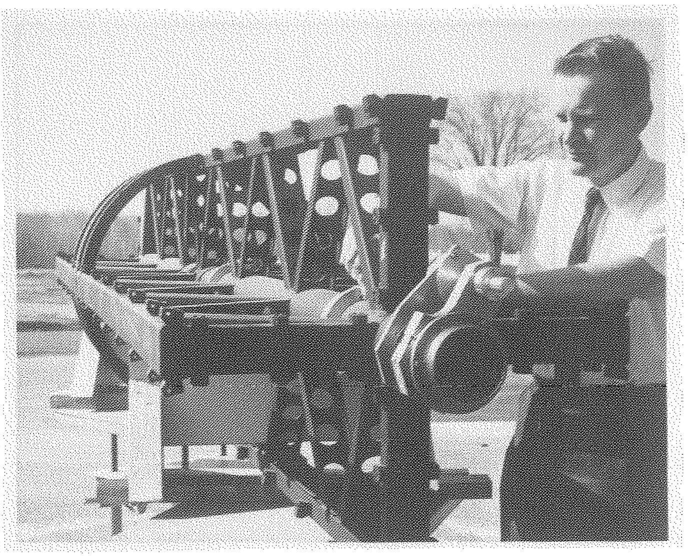
Gaizauskas was assisted in the site testing by Anton Kryworuchko[111]. It occupied the summers of 1966 through 1969. Among the obvious criteria in the selection of a site were accessibility, the number of hours per year of clear skies, the transparency of the sky, and the fraction of clear hours that provided good "seeing". For the purpose of the proposed research, high spatial resolution, of the order of one arc-second or less, was required.

Two semi-portable refractors which, through two doublets, produced solar images 100 mm in diameter, were built for the testing. They were equipped with photoelectric guiders and with a mirror that deflected any 6"x6" region of the solar surface into the aperture of a high-speed ciné camera. This camera could produce photographs at the rate of 20 per second with exposures in the millisecond range.

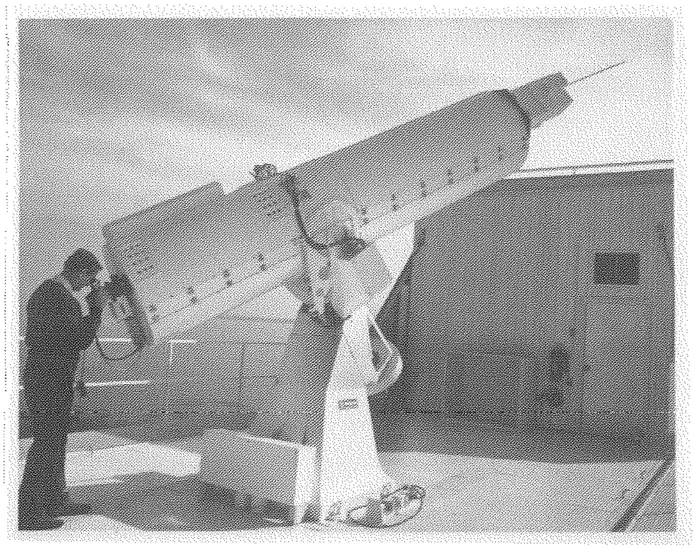
This was the period during which Mount Kobau was being developed as a national astronomical facility, so attention naturally turned to it. Two sites on the mountain were selected for testing, and observing platforms were built at both locations by Mel Lytle, the Observatory carpenter. These raised the objective lens of the telescope six metres above the ground. Testing at Mount Kobau continued through three summers and while there were many successive cloud-free days, the seeing was seldom of the highest quality. There seemed to be no point in having a new observatory with inferior seeing at the same longitude as the superb observatories in the southwestern United States and the search switched to the Ottawa area.

Here testing was conducted at three sites: at Lake Traverse, in Algonquin Park, the site of the NRC Radio Observatory; at the CHU transmitter station on Greenbank Road; and at a point of land, projecting into the Ottawa River, on property of the Department of National Defence at Shirley Bay. The latter site, which is very nearly surrounded by water, quickly proved superior to all others tested, and arrangements were made to acquire the necessary land, and access, from the Department of National Defence.

Attention then turned to the design of the instrument[112], or perhaps we should say instruments, for four different pieces of equipment were attached to a central "spar" which continuously tracked the sun. This spar is shown in the figure below, in which will be seen the central, guiding, telescope, and eight flat mounting surfaces on which other instruments could be mounted. This central spar, with its attached instruments, is enclosed within eight hinged, insulated, aluminum panels, lined with flexible heating panels for warming the interior during winter nights. Fans remove this warm air before observing begins. The spar is fork mounted on a polar axis; its declination travel is limited to the annual solar range in order to keep the fork arms short and rigid.



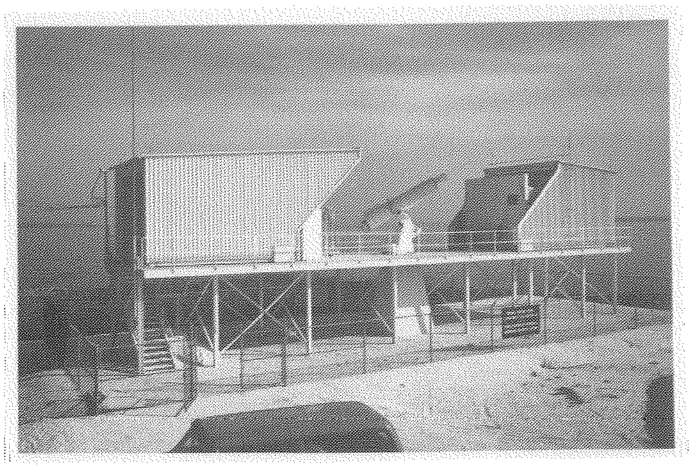
Dr. V. Gaizauskas adjusting an element of the spar to which the various cameras are attached. Note the central guiding telescope. NRC photograph.



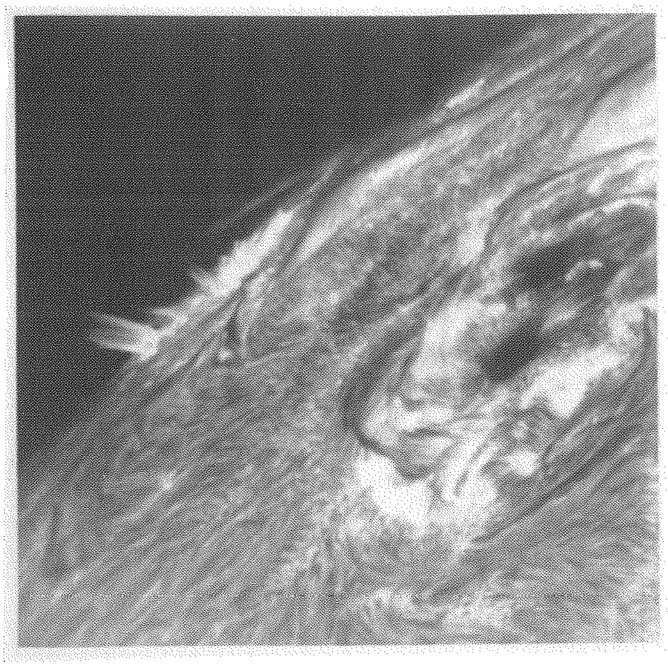
The telescope with its instruments enclosed within the insulated aluminum panels. NRC photograph.

This complex instrument is known as a Photoheliograph. Its principal component, which is mounted on the upper two quadrants of the spar, is designed to take large scale ciné photographs of selected areas of the chromosphere and of the photosphere. A telescope with a 25 cm objective lens channels the solar image through a system of filters to two ciné cameras. One of these can be used to sweep a bandwidth of 32 Å centred on the Hα line, and so provide information on the relative heights of the features in the chromosphere. The other photographs the sun in a band, 400 Å wide, centred on the same line; this permits a study of the photosphere.

The two lower quadrants house other instruments, a refractor with a 15 cm aperture designed for the wide-band photography of the photosphere, and another refractor, with an 11 cm aperture, for photographing the entire solar disk in Hα light.



The Ottawa River Solar Observatory. NRC photograph.



A small flare photographed at the Ottawa River Solar Observatory, April 26, 1978. Comparison with the photograph on page 70 gives some idea of the improvement in the new equipment and location over the old.

The telescope was the first in Canada to be controlled by a computer. The central guiding telescope, which controls the position of the spar with great precision, is completely automated; the sharpness and brightness of the solar image is monitored by photoelectric means and the camera rates are selected automatically on the basis of these observations.

At the time of the transfer of astronomy to the National Research Council, the design of the Photoheliograph had been completed, the component parts had been ordered, the property had been acquired and construction of the building had commenced. While the Earth Physics Branch cannot claim kudos for any work done with the new instrument, we have the satisfaction of having sent solar research into its new world suitably equipped.

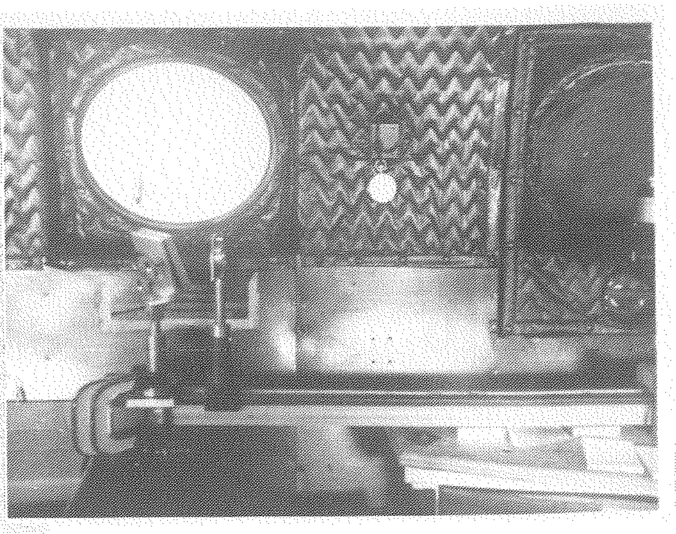
Solar Eclipses

The observation of eclipses is important for a number of reasons. For the Ottawa group the chief interest was to observe the spectrum of the chromosphere which, in the days before space telescopes, could only be observed in that fleeting moment just before totality.

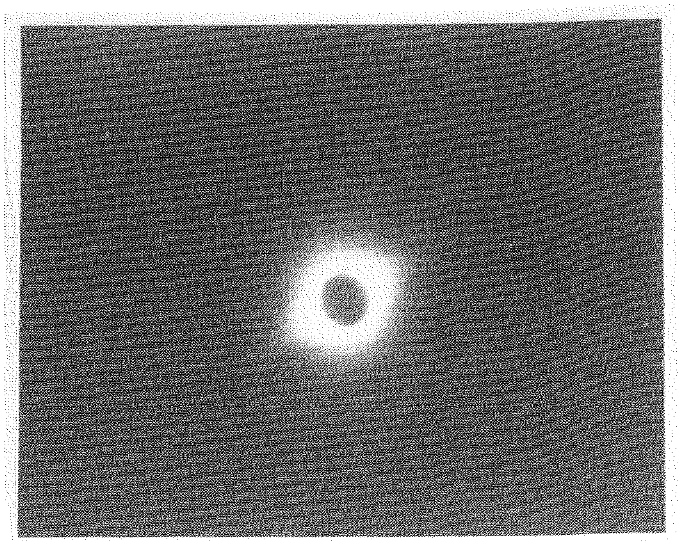
The first eclipse to be discussed, on July 9, 1945, should properly have been considered in Part I. The path of totality entered Canada south of Moose Jaw, passed south of Regina, over Wolseley, south of Yorkton, over Lake Winnipeg and across Hudson Bay. A multi-organizational party, under the direction of D.H. Menzel of Harvard College Observatory, was established at Bredenbury, Saskatchewan[113]. It included among its members Beals of the Dominion Astrophysical Observatory, and G. Herzberg, of the Department of Physics of the University of Saskatchewan. Unfortunately the sky was overcast at Bredenbury, but parties at Pine River, Manitoba, and Wolseley, Saskatchewan[114] were able to observe it. The RCAF, urged on by astronomers Squadron Leaders Heard and Millman, mounted "Operation Eclipse" which, flying in an Anson aircraft at 17,000 ft, took photo- graphs and spectrograms of the solar corona.

An eclipse on June 30, 1954, was total over a path in northern Ontario and Quebec[115]. Three expeditions were mounted by Canadian organizations: at Mattice, Ontario, by the University of Toronto[116]; at Hansen Ontario, by the Dominion Astrophysical Observatory[117]; and at Smokey Falls, about 40 miles north of Kapuskasing, by the Dominion

J.E. Kennedy with the Hilger ultraviolet prism spectrograph used in the airborne expedition to study the eclipse of June 30, 1954.



The mirror and lens which reflected the image of the sun into the spectrograph.



The eclipse at totality.

Observatory[118]. The sky was overcast at all three positions. At Mattice a plane was standing by to take observers above the clouds, but the ceiling was so low that it could not take off.

Fortunately this bad weather had been forecast and an airborne expedition had been planned, with the cooperation of the Division of Physics, NRC, and the Central Experimental and Proving Establishment, RCAF. An aircraft carrying two quartz spectrographs flew in the path of totality at a height of 27,500 feet over the North Atlantic off the coast of Labrador. One of the spectrographs was used to obtain spectra of the chromosphere, the other of the corona. Observations of the general form of the corona and the positions of the visible prominences were forwarded, via the CBC and the Swedish Broadcasting System, to observing expeditions in Europe, who were thus able to fine-tune their observing programs.

The next eclipse occurred on July 20, 1963. The path of totality, in Canada, began in the Yukon, moved south-eastward, passing between Montreal and Quebec, into Maine, and thence over the Atlantic[119]. Not many places were readily accessible along this path, but public interest was so high that both the Canadian National and the Canadian Pacific Railroads ran Eclipse Tours, the Royal Astronomical Society of Canada appointed a National Coordinator[120] and most of the Centres mounted expeditions. Fortunes varied; some parties had good weather, others did not.

The Observatory did not plan a ground expedition, but took part in a multi-organizational effort which observed the eclipse from an RCAF Yukon aircraft flying at some 35,000 feet over Great Slave Lake[121]. The Yukon is a large aircraft and many different experiments could be mounted by the participants – the Dominion Observatory, Oxford University, the National Research Council and the University of Saskatchewan. In addition, a number of observers from other scientific organizations in Canada could be accommodated. J.C. Arnell, who coordinated the arrangements on behalf of the RCAF, credits Beals with having proposed the expedition, and Thomson and Hollinsworth with supplying the necessary accurate timekeeping during the flight.

Due to the problems of navigation with limited ground support the plane was considerably to the west of its intended location. As a result, the eclipse started well before the anticipated time, catching some experiments a bit off guard. Halliday, who was a visual observer on the flight, noted that the plane had just passed over Fort Simpson seconds before the eclipse began. The error was a fortunate one. Had the plane been over Great Slave Lake the sun would have been obscured by high haze[122].

The Observatory's experiment, which was staffed by Locke, Gaizauskas, J.E. Kennedy and G.E. Sanders, aimed at studying the intensity of both the inner and outer corona, by measuring the intensity of the emission line at 5303 Å and its relationship to the continuum. Because the intensity of the coronal light varies by several decades with distance from the solar limb, a photometer with a logarithmic output had to be designed. This was accomplished successfully. The photometer had a circular aperture with a diameter of 3 min of arc, an automatic scanning device that allowed it to scan an area 4°x5° in 3 seconds, and an aiming system that allowed it to observe the corona on any part of the solar limb, or to any distance from the limb.

The shape of the white light corona was, as expected, uniformly distributed about the sun, but the green line corona showed two very active regions, on opposite limbs of the solar equator. One of these maxima extended to two solar radii the other to only one-third of a solar radii, but the two regions were well defined; in contrast, the green corona was entirely absent near the poles[123].

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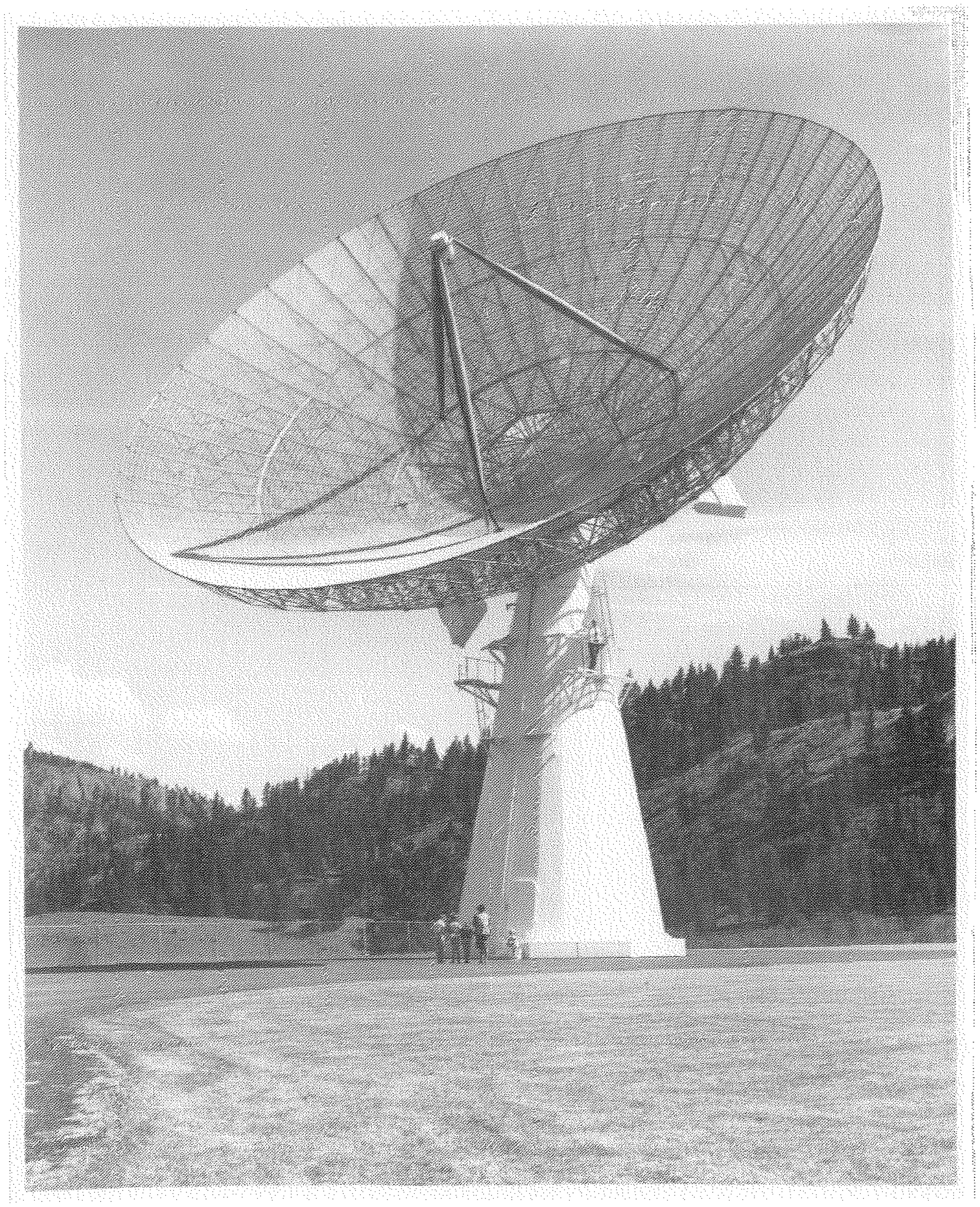


The 1420 Mhz Telescope.

IX - THE DOMINION RADIO ASTROPHYSICAL OBSERVATORY

INTRODUCTION[1]

Radio astronomy had its beginning in 1931. Karl Jansky of the Bell Telephone Laboratories had set up a radio antenna, seeking to understand the sources of noise that might interfere with the operation of short-wave radio telephones. He found that static appeared at about the same time every day. To be more precise, it arrived four minutes earlier each day, but this was by solar mean time; measured in sidereal time the return periods were exactly one day. The static was coming from outside the solar system. Jansky showed that it originated in the Milky Way, and was a maximum in the direction of Sagittarius; this was the direction in which optical astronomers, including Plaskett, had located the centre of the galaxy.

Astronomers paid little attention to this discovery; only an amateur, Grote Reber, followed up the work by building a radio telescope in his back yard. During the late 1930s and early 1940s, Reber confirmed Jansky's conclusion, but he showed that there were also a number of discrete sources; one of these was later proved to be associated with the Crab Nebula. Reber is revered by modern radio astronomers as their founding father.

World War II gave the science a big boost: the British found that their radar was being jammed by radiation from the sun, and sought ways to overcome this problem. Their findings had a security restriction and it was Reber who published the first evidence.

After the war, the receivers developed for radar were adapted for use in radio astronomy. We have seen in the section on solar physics how Covington and others at NRC began the study of solar radio noise at this time and in this way.

Radio astronomy studies that section of the electromagnetic spectrum in the range from 1 mm to 30 m (300,000 MHz to 10 MHz). These wave-lengths are very long in comparison with those used by the optical astronomers, and this imposes special problems in the design of radio telescopes. It is important that a telescope should have a narrow beam, otherwise it will receive radiation from a broad area of the sky and will be incapable of resolving discrete sources. This requires that its aperture be large in comparison with the wave-length of the radiation to be studied. For an optical telescope, where the wave-lengths involved are of the order of 5×10^{-5} cm, a mirror one metre in diameter is 2 million times the size of the waves to be measured; the largest steerable radio telescope in the world, 100 m in diameter, is less than 500 wave-lengths of the 21 cm radiation it may be used to study. The drive to improved resolving power has been a constant factor in the design of radio telescopes.

Early measurements in radio astronomy studied the variation with frequency of radiation from a particular object. The early radio telescopes could observe only a single narrow band of frequencies at one time. By combining observations from different telescopes it was possible to build up a profile of the continuum; the spectral lines, which are so important for optical astronomy, were not observed. In the early 1950s theoretical work suggested that neutral hydrogen atoms should emit a strong line at 1420 MHz (21 cm). Since hydrogen is the most abundant element in the universe the existence of this line would provide a powerful tool, and a search was mounted to observe it. Dutch, American and Australian astronomers joined in the search. The Americans, Ewen and Purcell of Harvard University, were the first to find it; the other teams soon provided confirmation.

The 21 cm hydrogen radiation proved indeed to be a powerful tool since it passes virtually unimpeded through the dust of interstellar space, which so limits the range of optical astronomy, and is observed both as an emission and an absorption line. By observing its Doppler shifts, radio astronomers could measure the radial velocities of the radiating hydrogen clouds. On the basis of these observations, and by making plausible assumptions about the form of the galaxy, and adopting some initial values from optical astronomy, they have built up a reasonable picture of our galaxy and of the motions within it. To a first approximation, material moves in circular orbits about the centre of the galaxy, with that nearer the centre rotating more quickly than that more remote. This suggests a model comparable to the Kepler model of planetary orbits, in which the period of the planets increases with distance from the centre. However the motion is more rigid than the Kepler model would suggest, perhaps held together by magnetic fields in the inner region, changing to Keplerian motion further out.

Astronomers[2] distinguish regions in which hydrogen is completely un-ionized (H I) from those in which it is completely ionized (H II). Hydrogen becomes ionized by the absorption of ultra-violet light. When ionized it is luminous, and can thus be observed by optical telescopes using appropriate filters. High luminosity is more likely to occur in the vicinity of hot stars than of cool ones. In this case the H II regions appear as brightly luminous *gas* or *emission* nebulae. As we have seen, H I gas radiates at 21 cm and this property is used to map the H I clouds. H II gas emits radio-frequency radiation with a continuous spectrum.

Astronomers also distinguish between *thermal* and *non-thermal* radiation. What do these terms mean? Every body emits electromagnetic radiation according to its temperature, and this is known as thermal radiation. There are other sorts of radiation, not related to the body's temperature. One example is *synchroton* radiation which originates in electrons spiralling around magnetic lines of force. It can produce radiation from a relatively cool astronomical body equal to thermal radiation that would require a temperature of a few million degrees. The continuous emission from H II regions is thermal, as is that from supernova remnants.

THE OBSERVATORY AND ITS EQUIPMENT

The Establishment of the Observatory[3]

As may be imagined, there was great interest within the Observatory in the developments in radio astronomy. Locke arranged a series of colloquia during the winter of 1955-1956 to review the literature; Beals gave one of the talks, on the optical identification of radio sources. By the time the series was finished both Locke and Beals were convinced that the Observatory should establish a radio observatory. In April 1956 Beals wrote to the Deputy Minister recommending that the Dominion Observatory be allowed to become involved in the new science of radio astronomy.

The suggestion was so well received that the preparation of a concrete proposal began immediately. To obtain data for the submission Locke visited a number of establishments in the United States that were working in the field, and Beals obtained the advice of a number of astronomers, including his brother-in-law R.O. Redman, who was visiting from Cambridge. The submission was based on the desirability of extending into the radio region the study of the Galaxy, for which the astronomers at the Dominion Astrophysical Observatory were so well known. Although it was always intended that the Observatory would undertake research in other areas, this approach was perceived as a proposal to build on existing strength, a fact that aided its acceptance.

The proposal called for the establishment of an observatory incorporating, as its initial instrumentation, an 84-foot paraboloid and a hydrogen-line receiver. It was submitted to the Acting Deputy Minister, George Hume, on July 25, 1956, forwarded to the Minister, George Prudham, with a strong recommendation from the Deputy Minister, Marc Boyer, on August 20, and transmitted to Treasury Board on September 19.

These names and dates are interesting. Boyer had been ill for several months and Hume had been acting Deputy Minister. When Boyer returned, Hume left the government service and was replaced as Director General of Scientific Services by W.E. van Steenburgh. It would have been van Steenburgh who was responsible for the "strong recommendation", the first of his innumerable services to the Observatories.

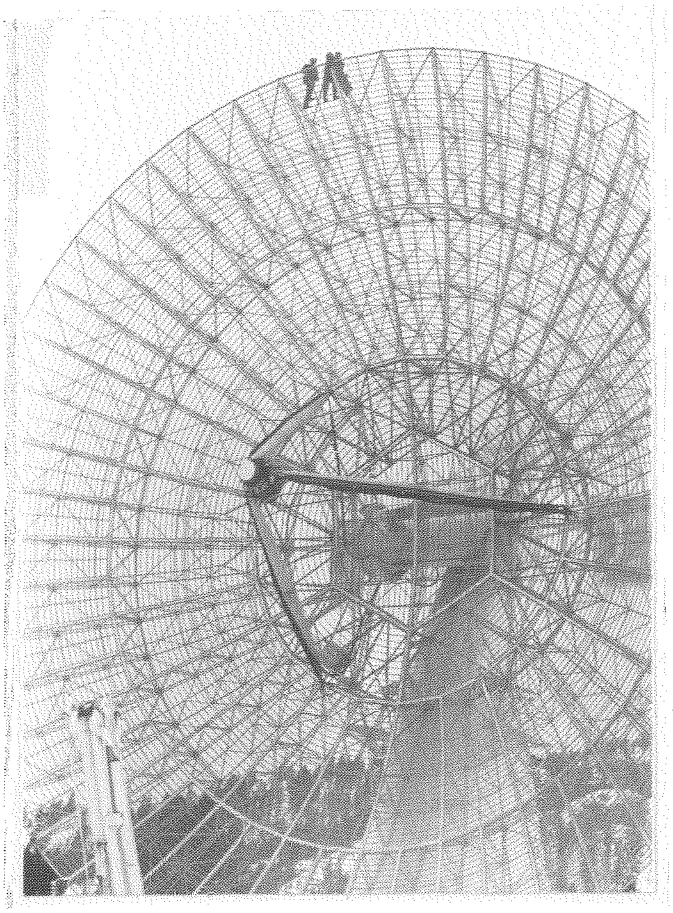
The submission reached Treasury Board at about the same time as one from the National Research Council, in which it proposed to expand its work in Radio Astronomy. Treasury Board staff considered the two proposals to be in conflict and felt that only one could be supported. Beals met with E.W.R. Steacie, the President of NRC, who agreed to withdraw their submission; they would submit a new proposal some time later, when their goals were better defined. With the way thus cleared, the Observatory proposal was approved on November 30, 1956.

One of the first tasks was to select a site for the new observatory. The search was limited to British Columbia for two reasons: first to place it close to Victoria and so facilitate close cooperation between the optical and radio astronomers; second since a valley surrounded by mountains would provide the best hope of avoiding man-made interference. Locke selected a number of potential sites from topographical maps and air photographs and "borrowed" E. Argyle from Victoria to assist in the site search. They gathered sight-testing equipment – radio antennas and receiving equipment with which to measure interference levels – mounted it in a van, and set off on their expedition in March 1957. To provide an upper limit for an acceptable Canadian site they visited the US National Radio Astronomy Observatory at Greenbank, West Virginia, and the radio astronomy observatory of the California Institute of Technology in the Owens Valley, California, measuring the interference levels at both sites. They visited many sites in British Columbia, both on Vancouver Island and in the interior and quickly decided on a valley known as White Lake, about 20 miles south of Penticton. It provided a flat area large enough to accommodate any antenna arrays that might be required in the future, and had the lowest interference levels of any of the sites visited. It is far enough south that observations down to declinations of -30° are possible. An additional point in its favour was the proximity of Penticton, a pleasant community where the staff would enjoy living and from which they could commute easily. Beals visited the site in mid-July, and confirmed the decision to locate in the White Lake valley.

It was not intended that the new observatory would always remain a part of the Stellar Physics Division. Shortly after Treasury Board approval was received J.A. Galt, a physicist with considerable experience in radio techniques, was taken on staff with the understanding that he would become the first Officer-in-Charge of the observatory. Galt was sent immediately to Jodrell Bank, one of the major United Kingdom radio astronomy facilities, to gain experience, and Locke continued to be responsible for the detailed planning of the observatory and its equipment.

As the project proceeded van Steenburgh became concerned about the wisdom of putting the observatory in charge of a person with no experience in government administration, particularly at a location so far from Ottawa. He suggested that Locke should remain in charge, and should be stationed in Penticton for a couple of years. Galt went to Penticton in February, 1959, upon delivery of the telescope components, and Locke moved there in August, by which time erection of the telescope and construction of the buildings were nearing completion.

Treasury Board approval of the Observatory had not included any provision for staff, and new positions became difficult to obtain when the Diefenbaker Government took office in June 1957; they became impossible to obtain in mid-1962 when a financial crisis forced the Government to establish the most rigid austerity program in Canada's history. By the time of the official opening, the staff consisted of four scientists – Locke, Galt, Argyle (who had by now been permanently transferred from the Victoria staff), and C.H. Costain, fresh from his graduate work at Cambridge; one technician, Roy Hamilton; a machinist, Bud Orge; a caretaker and jack of all trades, Ray Stewart; and a clerk-secretary Kay Adams. Locke returned to Ottawa in 1962, leaving Galt in charge.



A close-up of the paraboloid. I asked Dr. Galt to give me a photograph showing him at work; this was his contribution.

The official opening took place on June 20, 1960. There was a distinguished gathering. The Department was represented by the Minister, the Hon. Paul Comptois, and by the Deputy Minister, Marc Boyer; E.W.R. Steacie, President of the NRC, Beals and Petrie led substantial delegations from their respective institutions; the local Member of Parliament, David Pugh, and the Mayor of Penticton, Charles Oliver, represented the local input.

The Observatory was declared officially open by the Minister, Mr. Comptois, who pushed a button that caused the telescope to begin a scan across a strong radio source. As the scan went on, the rise and fall of the radio signal was broadcast over the loudspeaker. Because the staff did not have complete confidence in the reliability of the receiver, the expected signal had been recorded on tape a few days before. As the Minister pressed the button, Roy Hamilton was in the laboratory, ready to switch from the telescope to the tape, but it proved unnecessary.

June 20 was a beautiful day but rather windy. The area around the telescope was unpaved and dust was blown into the Ministerial eye, as well as into a lot of others. At the earliest opportunity a requisition was submitted to Ottawa, asking that the area be paved. The Minister approved the allocation of the necessary funds immediately!

A symposium on the objectives of radio astronomy was arranged after the opening by the Canadian committees for the IAU and the International Union of Radio Sciences[4].

The 1420 MHz Telescope

Locke and Galt were joined by Costain in a description of the Observatory and its equipment[5]. The antenna is a parabolic reflector 25.6 m in diameter, with a focal length of 7.6 m, conforming to a true paraboloid to within one cm. It is equatorially mounted, can track in hour angle at the sidereal rate, and can move in hour angle or declination at scanning rates of from 0° to 1° per minute or at a slewing rate 15° per minute. The antenna is supported on a steel tower bolted to a massive reinforced concrete foundation.

Three fibreglass spars hold the radio receiver at the focus of the reflector. It was maintained at constant temperature by water, adjusted in temperature by a refrigerator and heater housed in the tower, circulating in coils around it. The tuning of the receiver could be adjusted to any frequency in the vicinity of 1420 MHz in order to examine the wings of the spectral line and to monitor its Doppler shifts. The motions of the earth within the solar system, and of the sun within the galaxy, contribute to this Doppler shift, and must be allowed for. This was done in the electronic circuitry, so that any shift recorded is that of the distant gas cloud.

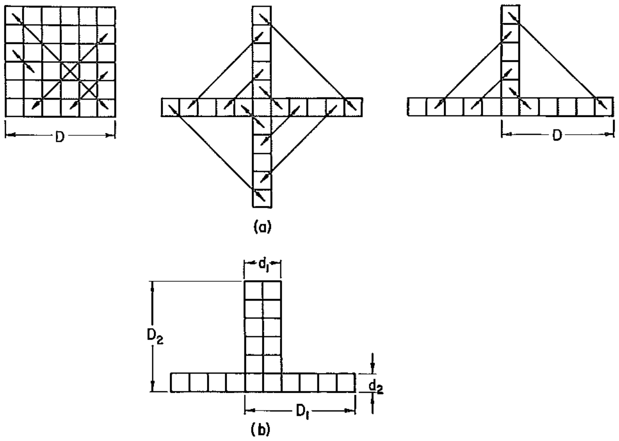
While the telescope was normally operated with a 1420 MHz receiver, it was relatively easy to change the apparatus at the focus to observe other frequencies as required. Several different receivers were available; one in particular, at 110 MHz, was used in the study of pulsars to be discussed later.

The signals the telescope was intended to record are weak, and with the initial electronics it required about an hour to produce a single line profile. There was a continuing effort to improve the sensitivity and speed of operation of the telescope. Part of this effort went into reducing the noise level of the receiver and associated electronics, and part went into developing the ability to record at a number of adjacent frequencies simultaneously. This was accomplished by "autocorrelation". The signal path is split in two, one half going to a delay line. Then many correlation products can be formed between the undelayed signal and signals from multiple taps on the delay line. These correlation products are Fourier transformed, within a computer, to form a spectrum. Initially this method was used by David Sloan, a graduate student from the University of British Columbia, to provide five channels; then Argyle built a twenty-channel system; finally Roger and Peter Dewdney produced systems with 128 and 256 channels.

The telescope has a nearly circular beam with a half-width of 36′. It is controlled by a console in the laboratory building. Its output was initially recorded on a chart recorder, and later on punch cards, ready for computer processing. For the first two or three years these cards were sent to the departmental computing centre in Ottawa, where charts were plotted and sent back to Penticton. When the observatory acquired its own computer the telescope output was fed directly to it.

In the beginning, the telescope was almost always used in what is known as the "drift scan" mode. Instead of moving the telescope continuously through the region to be studied, the antenna was set at a particular declination, and maintained there while the earth's rotation moved the area of interest through the antenna beam.

We must note the addition of second radio telescope at the DRAO facility, a 1.8 m parabola and associated electronics installed by the Division of Radio and Electrical Engineering of NRC in 1963-1964[6]. It operated at a frequency of 2700 MHz and was essentially a twin of a telescope operated at the Algonquin Radio Observatory. It was thus "part of a continuing program to provide a high quality basic index of solar activity." The telescope was in operation in time to document the lowest "low" of the sunspot minimum – July 26 and 27, 1964.



The Development of Long Wave-Length Arrays

While the 21 cm telescope permitted studies of the hydrogen spectral line emissions, it was less satisfactory for H II emissions which were better studied at lower frequencies. Shortly after the paraboloid was completed planning began for a telescope to record these lower frequencies. In selecting the specific frequency, thought had to be given to occupying a spectral region not being studied by other observatories and, more particularly, to select a frequency with a low man-made radio noise level; a frequency of 22.5 MHz (wave-length 13.5m) was believed to be the lowest practicable, and it was decided on. At this long wave-length ionospheric disturbances can be serious, and it was planned to have the new telescope in operation by the time of the sunspot minimum, expected to begin in 1964.

We saw earlier that in order to obtain a narrow beam width a radio telescope must be large in terms of the wave-length it is intended to record. The 25.6 m paraboloid has a diameter about 120 times the wave-length of the 21 cm hydrogen radiation. To maintain the same ratio for 13.5 m radiation would require a paraboloid about 1600 m in diameter, clearly an impossibility. For these long wave-lengths recourse is had to arrays, in which a large number of individual antennas are connected to form an antenna with a much larger aperture.

The array might consist of a large number of rows of antennas making a square pattern, as shown in the left-hand diagram in the figure below. However it may be shown that the cross, (a) in the figure, has the same beam width as the solid array; it does not of course have the same sensitivity. It may further be shown that the T-pattern with a double arm, (b) in the figure, has the same resolving power and sensitivity as the cross. It was this T-array that was adopted for the DRAO. For the moment we will neglect the question of how the array is "steered".

The proposal for the array, and its design concept, were due entirely to Costain, and he was assisted by J.D. Lacey and R.S. Roger in its construction. They have described the array[7]. The receiving elements of the array are full-wave dipoles mounted on wooden poles above a reflecting screen 65,000 m^2 in area. The EW arm of the array consists of 368 dipoles, aligned in an EW direction in four rows; the NS arm consists of 240 dipoles aligned in an EW direction in 64 rows; there are 16 dipoles in the overlap of the two arms. The signal from the dipoles is collected by a branching network of coaxial cables and led to the receivers. But not directly! By putting more or less delay into the leads from the dipoles of the different rows of the array, one can delay the arrival of the signal; this has the same effect as tilting the array. Remotely controlled "phasing" switches are used to direct the beam of the array to any desired zenith angle. To observe a particular radio source, the appropriate zenith angle is set and the rotation of the earth provides the necessary scan. To provide more rapid sky coverage a rapid-phasing network continuously adjusts the declination setting through five positions adjacent to the central one. The array is controlled, and the data recorded, in the main Observatory building. Full operation began in mid-1965.

Each arm of the array produces a fan-shaped beam, but by multiplying the output of the two beams electronically a pencil beam 1.1°x1.7° is produced.

Early results with the 22.5 Mhz array were promising and Cambridge University proposed the joint construction of an array for even lower frequencies. A central frequency of 10 MHz (wave-length 30m) was selected. While the design of the array differs in detail from the earlier one, the principle is the same. One important improvement was that the new array could produce a number of beams at the same time, a feature that enabled the astronomers to take full advantage of those rare periods when observing conditions were good. These beams had a width of 2.6°x2.4° at the zenith.

Galt and Costain, in a review[8] of the telescope building program at the DRAO, illustrated the properties of the three telescopes by showing scans of various radio objects. The figure on the following page shows a scan of the galactic plane at the three frequencies. At 1420 MHz the plane of the galaxy shows strong thermal emission; at 22 Mhz there is a partial absorption of this background emission and at 10 MHz the absorption is almost complete.

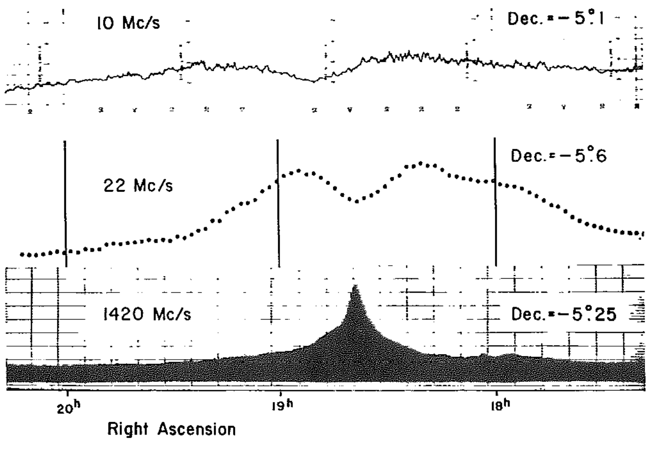
RESEARCH PROJECTS

It is difficult to exaggerate the excitement which pervaded the radio astronomy community in the period when the DRAO was being developed. It was as if optical astronomy had arrived at a position of sophisticated instrumentation, including computer data processing, and had only then discovered the existence of spectral lines. The entire universe was suddenly opened up to a completely new, but technically sophisticated, discipline. Large radio telescopes, with a variety of frequency ranges, were built in many countries, particularly in Britain, Germany, the Soviet Union, the United States and Australia. Systematic surveys of the sky were carried out, catalogues of radio sources were published, and observatories cooperated in studying these sources at different frequencies. The principal catalogue, or catalogues, for there was a series of them, were issued by the Radio Astronomy Observatory at Cambridge.

Some of the radio sources could be identified with visual objects, but the low resolving power of the early radio telescopes made such identifications difficult.The development of instruments with narrow beams had a high priority. These involved interferometry, a subject to which we shall return; with these new instruments it became possible to identify a high percentage of the radio sources.

A number of high-energy radio sources were eventually identified with what appeared to be very faint stars that could only be observed optically by the largest telescopes; these were called *quasars*, for "quasi-stellar radio sources". The spectra of these objects showed emission lines which, at first, did not appear to arise in any known element. It was discovered in 1963 that they were, indeed, well known lines, displaced from their rest position by a very large Doppler shift; the objects were apparently receding at a large fraction of the speed of light.

This was by far the largest "red shift" ever discovered, and it presented astronomers with a major problem. According to standard astronomical reasoning, the large shift indicated that the objects were very distant, beyond the furthest known galaxies. If this were not so, then the standard reasoning was wrong, and the accepted model of the universe would be in serious difficulties. If it were so, how could one explain the tremendous energy that they radiate? Quasars presented the problem of the decade!



Scan of the galactic plane at three frequencies. From Transactions, Royal Society of Canada, Fourth Series, Volume III, Section III, 427.

The DRAO was part of this exciting period. We shall now examine its research output.

Studies with the 25.6 m Paraboloid

One of the first studies made with the new telescope, in which Locke, Galt and Costain collaborated, fulfilled the commitment to Treasury Board that the instrument would be used to study the galaxy. It involved the survey of an area 20°x30° in the direction of the anticentre of the galaxy[9]. We would expect that in this direction the velocity component due to galactic rotation would be small. The survey was made in the drift scan mode, with 30′intervals between declination settings. Observations were repeated at five different frequencies, covering a range of 0.08 MHz, so that five radial velocities, varying from +1.2 to -15.7 km/sec, could be mapped.

The maps are reproduced in the paper. The distance to the main emission was determined, and an earlier suggestion that an outward velocity was imposed on the rotational velocity of the galaxy was supported. There was some evidence for the existence of cool dense clouds observed against a background of hotter gas.

The same authors next made a study of the neutral hydrogen radiation in the region of an optical nebula, IC443[10]. The area of the nebula was scanned at three different frequencies, corresponding to a velocity range from -3 to -11.4 km/sec, using drift scans at 15′ intervals; maps were produced for each frequency. This nebula, which is generally regarded as the remnant of a super nova, is characterised by emission lines in the visual range, which, as we have seen in earlier chapters, were postulated to originate by collision with some shell or shells surrounding the nova. The radio-astronomy results demonstrated the existence of an interstellar cloud surrounding the nebula, thus supporting the position of the optical astronomers.

It had been intended from the beginning that the 25.6 m telescope would be used to provide a survey of the sky. This survey, by Galt and J.E.D. Kennedy occupied a major part of the observing time between July 1963 and July 1967, at which time the results were published[11]. The survey was made by the drift scan method, declination intervals being 15′, and it covered the sky from declinations -5° to +70°. The survey documented 615 radio sources, 81 here observed for the first time. The positions, size and intensity of all sources were tabulated.

A number of the sources were also observed at the Algonquin Radio Observatory at a frequency centred on 3200 MHz. Analysis was in terms of *spectral index*, which is determined in the following way. If for any source the log(flux) is plotted against log(frequency), the slope of the line is the spectral index of the source. The study measured differences in the index over different regions of the sky.

Kennedy used the data to produce maps showing the structure of different regions of the sky; this was submitted as a PhD thesis at York University.

Another study concerned the Andromeda Nebula[12]. Using the 20-channel autocorrelation spectrometer he had developed, Argyle surveyed an area several times that of the visible nebula, obtaining at each of 368 points a spectrum covering 1.7 MHz, centred on the 1420 MHz Hydrogen line; this spectrum corresponded to a velocity range of 360 km/sec. The extent of the nebula in the radio picture was substantially greater than the visual limits and Argyle was able to contour both the distribution of the neutral hydrogen and its radial velocity. Accepting the distance of the nebula as determined from optical astronomy he estimated its total mass of hydrogen as 1.93×10^9 solar masses.

There was close cooperation with radio astronomers at the Universities of British Columbia and Calgary[13]. One important study was an attempt by V.R. Venugopal and W.L.H. Shuter to define how the random velocities of neutral hydrogen were distributed in the solar neighbourhood. Early in the research they realized that they would need to know how the solar motion related to the neutral hydrogen[14]. They observed 21 cm profiles at 34 widely dispersed positions and obtained the average value of the relative motion between the sun and these gas clouds. Their value was comparable with that obtained by optical astronomers of the sun's motion with respect to other stars; they conclude that the clouds of neutral hydrogen in the vicinity of the sun are not moving with respect to those stars.

Returning to their main problem[15] Venugopal and Shuter obtained profiles of 21-cm emission at 1200 points equally spaced over the entire sky visible from Penticton and determined the radial velocities of the gas in each of these directions. They described these random velocities in terms of a velocity ellipsoid; the velocities were found to be closely aligned with the local magnetic field, and apparently influenced by it.

Studies with the Arrays

While the frequencies of all the telescopes could be extended somewhat, they still covered a limited section of the spectrum. This was equally true of the telescopes at other observatories and much of the work depended on the pooling of data. For example, the first publication to come from the low frequency arrays describes observations using parts of both the 22 and 10 MHz telescopes before the complete arrays were available[16]. It added points at these two specific frequencies to the spectrum of a distant galaxy, NGC 1275, and confirmed the existence of a low-frequency spectral component.

In this paper, as elsewhere, radiation is measured in flux units (f.u.), which express the power incident on a square metre of the receiver surface in a band-width of one Hz. It is also expressed as antenna temperature in °K; this is the temperature that an equivalent black body would have if radiating at the same intensity in the same band-width.

We saw earlier that, at low frequencies, ionized hydrogen clouds may obscure bright radio emission beyond them. Roger[17] used the 22.5 MHz array to scan an area in the sky in which this could be observed. To minimize ionospheric effects, the scan was repeated four times, and averaged. A similar scan at 38MHz was available in the literature, and Venugopal had included the region in his 21 cm scan[14]. Combining the data at these several frequencies, Roger was able to separate the background and foreground emissions, and to determine the electron temperature of the gas clouds.

The principal surveys made with the arrays during the sunspot minimum are listed in three papers. A.H. Bridle and C.R. Purton[18] studied 124 known radio sources with the 10 MHz array, Roger, Costain and Lacey[19] and Roger, Costain and Stewart[20] 400 sources with the 22 MHz array. Even though the measurements were made during the sunspot minimum, ionospheric absorption and scintillation were severe at these frequencies, and elaborate precautions had to be taken to allow for their effects, as well as in the calibration of the array sensitivities. In each case the corrected flux densities, and their probable errors, are tabulated and combined with similar measurements to define the spectra of the radio sources[21].

These surveys mark the end of the low frequency studies. It is gratifying that, despite the short lead time available, the arrays had been completed in time to take advantage of the sunspot minimum. Now that the data had been obtained, and the minimum had passed, the observations with both the 10 and the 22 MHz arrays were terminated.

Pulsars

In 1968 radio astronomers at Cambridge University announced the discovery of four radio sources that were emitting regular pulses of energy with periods of about one second; they called these sources *pulsars*. This discovery engendered much research throughout the radio-astronomy community. What were these strange emitters; were there more of them; were their emissions constant?

At the DRAO Galt and A.C. Gower studied the variation in intensity of pulsations from three such sources[22], searching for a period to such variation. If such periodic fluctuations existed, they might provide a clue to the emission mechanism. Using the 25.6m paraboloid, with autocorrelation techniques, at a frequency of 113 MHz they sought, unsuccessfully, to find periodic variations in the range from several minutes to several hours. Three other papers by DRAO personnel were critical of work done elsewhere, pointing out the difficulties of drawing reliable conclusions from data contaminated by serious ionospheric disturbances. The first[23] concerned a study that had placed an upper limit on the interstellar magnetic field in the direction of a particular pulsar; the second[24] a paper that discussed the short-period variability of two radio sources; the third[25] questioned the existence of high frequency variations found elsewhere in NGC 1275.

The Development of Interferometers

As we have seen repeatedly, the resolving power of a radio telescope depends on its diameter and the wave-length of the radiation being measured. The formula which illustrates the relationship is

$$\text{Resolving power} = 70\text{x}\ \frac{\text{Wave–Length}}{\text{Diameter}}$$

where the wave-length and diameter are measured in the same units and the resolving power is measured in degrees. If we apply this formula to the DRAO paraboloid, where the wave-length is .21 m and the diameter is 25.6 m, we obtain a resolving power of 0.57°; this compares to the actual half-width of the circular beam of 36′ or 0.6°. To improve the resolving power one would have either to go to much shorter wave-lengths or to a much larger telescope. If, for example, a resolving power of 1′ were desired with the 25.6 m telescope one would have to measure waves at 0.6 mm; if one wanted to build a telescope to provide a resolving power of 1′ at 21 cm, its diameter would have to be 880 m! These are crude values, but they illustrate the problem.

How are we to obtain a telescope of these dimensions?The solution lies in the use of interferometry. We recall from elementary optics that when light waves from a coherent source, which have traversed different paths, are combined so that wave crests coincide there is an increase in intensity, and that when they are combined so that the crests coincide with troughs there is a decrease in intensity. If we can arrange some device in which two wave paths differ progressively in phase, fringes, alternately light and dark, result. These fringes can be used to measure the properties of the emitting source. The phenomenon of interference is not limited to light waves, but is observed throughout the electromagnetic spectrum.

A panoramic view of the Dominion Radio Astrophysical Observatory; in the middle foreground the long wave- length arrays, beyond them the synthesis telescope track with its two paraboloids. The 1420 Mhz telescope and office complex are at right centre. DRAO photograph.

If the output from two radio telescopes pointing at the same object can be superposed in such a way that the coherence of the radiation received by the two dishes is maintained, then the difference of arrival times at the two telescopes can be measured. This difference depends on the direction of the radiating object with respect to the line connecting the two telescopes, and by measuring the time difference, information can be obtained about the angular dimensions of the radiating object. The technique of establishing the coherence of the two signals is analogous to that used in the optical case, except that the combining of the two signals is done electronically. The resolving power of two telescopes used as an interferometer is equal to that of a single telescope having a diameter equal to their separation, or baseline.

There are two ways in which this principle has been used at the DRAO. The first involves a "synthesis" telescope. In this telescope two paraboloids are set up on moving trucks mounted on railway tracks in such a way that the distance between them can be varied. The trucks, and the tracks along which they move, must be rigidly constructed so that the distance between the telescopes can be established and maintained to high accuracy.

Why have a moving telescope; why not set the telescopes at a fixed distance selected to give the desired resolving power? Most areas of the sky have broad, diffuse structures extending over several degrees, intermediate structures of various degrees, and "points", which are unresolved even at the maximum spacing. The variety of spacings provided by the moving telescope permit the recognition of all these structures; if any spacings were omitted the picture of the sky would be distorted.

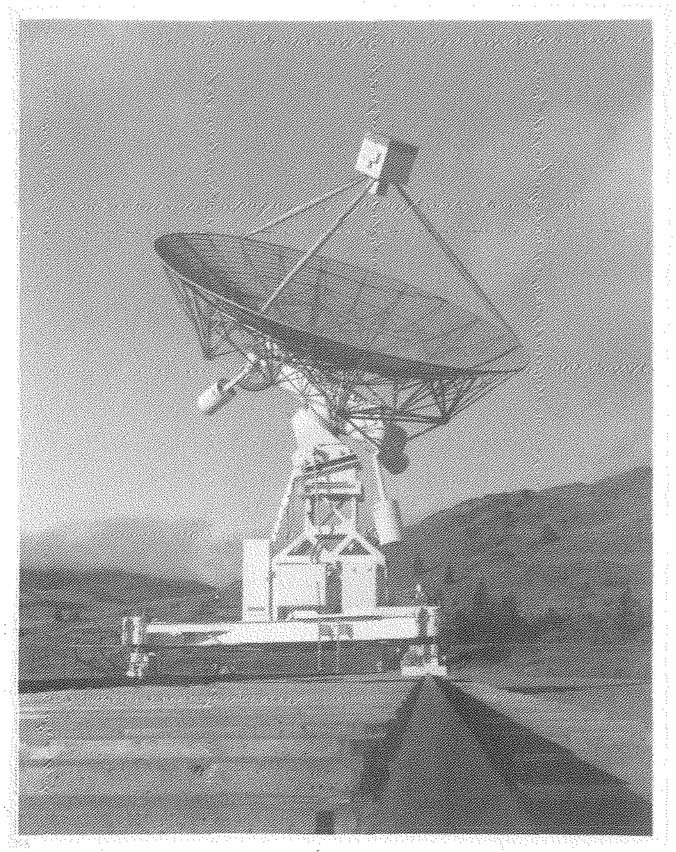
Construction of a synthesis telescope began in 1968[26]. It initially involved two 8.6 m paraboloids mounted on an east-west track 300 m long. It could survey an area of the sky of 2° diameter with a resolving power of two arc minutes. Electronic combination of the antenna outputs gave 80 different frequency channels. Later, two fixed paraboloids were added to extend the baseline to 600 m. This improved the resolution to one arc minute and permitted simultaneous observation at four separate spacings.

The instrument had not been completed by the time the Branch was broken up, but its development was supported strongly right to the end and we can take some satisfaction in its completion.

One section of the synthesis telescope is named after Grote Reber, the "father" of radio astronomy, who was a frequent visitor to the Observatory, and who contributed money directly to it. When the Observatory celebrated its 25th anniversary in 1985, Reber was guest of honour.

The Long Baseline Interferometer

As noted above, there are many radio sources of such small diameter that even the synthesis telescope is unable to resolve them. Quasars are one important example. To resolve them, very much larger separation of the two telescopes is necessary. How is this to be accomplished? In the case of the synthesis telescope the outputs of the two dishes were led to



One of the synthesis antennas. DRAO photograph.

the correlators by cables. For longer baselines, at distances up to about 100 km, the two signals may be brought together by radio links, although this was never done at the DRAO; beyond that distance, irregularity in the paths of the radio waves might introduce errors in the timing. Nevertheless there was a strong desire for longer baselines. Measurements made at Manchester University using radio links over a baseline of 127 km had established an upper limit for the diameter of some quasars of 0.025 sec of arc. We have seen that quasars emit high amounts of energy. If they are so small, how can this energy be developed? Was this a realistic value of their diameters, and would the values be the same at other wavelengths?

The desire to investigate this question led to the establishment of a cooperative study involving radio astronomers from Queen's University, the University of Toronto, NRC, and the DRAO. They hoped to use the telescopes at Penticton and Algonquin Park as an interferometer with a baseline of 3074 km, by recording the signals at each on magnetic tape, with time marks from accurate, synchronized, clocks, and bringing the records to a correlator. There were two problems to be overcome. To obtain the required resolution the signals would have to be recorded on wide-band recorders that could be precisely aligned and would remain in alignment throughout the experiment. To measure the difference in arrival times of the wave-front at the two stations it would be necessary to press to the frontiers of timing precision. Both problems were solved. The CBC, which was in the process of converting from black-and-white to colour television, had a number of black-and-white recorders that were converted, at Queen's University, to record the expected signals. On the advice of the Time and Frequency laboratories of NRC, rubidium frequency standards were acquired, which provided not only the required timing accuracy but also controlled the alignment of the recorders. The experiments could begin.

The first test[27], on February 5, 1967, involved the large telescope at the Algonquin Observatory and a second, 10 m, instrument located 200 m away. At this short distance the measurements could be made both by linking the two telescopes directly to the analyzer, and by the use of the recorders. The experiment was a complete success.

Finally the main experiment[28]. A recorder and rubidium frequency standard were sent to the DRAO in mid March. As they were being installed the clock stopped! Must the experiment be postponed? No! It was rumoured that a team of American astronomers were on the verge of success in a similar experiment. The Penticton clock was driven to Vancouver, and placed on an Air Canada flight to Toronto where it met the Algonquin clock. The two were synchronized at the Toronto Airport, returned to their respective bases, and the experiment was completed. However, when the two records were brought together at the Algonquin Observatory, no fringes were found. Could the clocks be wrong? The Penticton clock was again shipped east and the two clocks compared. They differed by six seconds! The clock was corrected, returned to Penticton, and the experiment repeated. Again no fringes.

What was the problem? Was the jump from 200m Algonquin link to the Algonquin-DRAO link too great? It was decided to attempt the experiment at an intermediate distance. The equipment was shipped from Penticton to Ottawa and the experiment was repeated, between a Defence Research Board 60-foot dish at Shirley Bay and the Algonquin telescope, a baseline of 250 km. Good fringes were obtained.

It was concluded that the fringes *must* exist on the Penticton-Algonquin baseline. The tapes were run again, and again and again. Finally, at 8 o'clock on the morning of May 21, the coherence was established. Two days later the success was reported to the International Scientific Radio Union, meeting in Ottawa. It was a triumph!

The experiments continued; more than 50 quasars were observed over the Algonquin-DRAO baseline over the next few months, and coherence was established on eleven of them; these showed apparent diameters ranging from 0.02 to 0.06 seconds of arc, but there were unresolved quasars for which the diameter must be less that 0.01 seconds of arc!

The Geodetic Survey of Canada had contributed to the project by establishing the position of each of the telescopes and so defining the baselines precisely. Surveying may well become one of the principal beneficiaries of long baseline interferometry. If the position of a small quasar could be established, then the experiment could be reversed; the source position would become the known factor, the length of the baseline the unknown.

H.E. Jones[29] has reviewed the possibilities of such an experiment and suggests that an accuracy of one part per million in intercontinental distances may be anticipated. Interferometry is likely to settle the question of continental drift before the Photographic Zenith Tube network does.

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X – METEORITE CRATERS

INTRODUCTION

In 1951 an event occurred that had a profound effect on the direction of research within the Observatory. It opened up an entirely new field for both Beals and his staff; it called on the resources of astronomers and of geophysicists in a variety of disciplines and so helped to break down those interdivisional barriers that had existed for so long; it established the Branch's position, independent of the Geological Survey, as an authority on the forces shaping the earth.

On that day in 1951 John A. Roberts showed Beals air photographs that his company had taken of a large circular feature in Algonquin Park, near the village of Brent, Ontario. Examined with a stereoscope, the feature showed a relatively shallow depression, largely occupied by two kidney-shaped lakes, Gilmour and Tecumseh. The circular shape and the depression suggested that the feature might be a meteorite crater, and this possibility interested Beals very much. His interest was, initially, that of an astronomer.

There was at that time a good deal of controversy about the nature of the craters on the moon. Many felt that they were the result of volcanism or of some related type of internal magmatic activity, but Dietz[1] had recently made a good case for a meteoritic impact origin. He suggested that, given the moon's small mass and low internal pressure, volcanism was unlikely. Furthermore, the moon's craters are not at all similar to terrestrial caldera caused by volcanic explosions or by the collapse of overlying strata into voids caused by the withdrawal of magma; they do however resemble closely, except for size, proven terrestrial impact craters.

Baldwin[2] carried the argument a step further. He measured the diameters and depths of a large number of the lunar features and of all the proven terrestrial meteorite craters. The bombing and explosive testing of the recently ended war had provided a large number of explosive craters; these too were measured. When the diameter of these craters was plotted against their depth on a logarithmic scale a smooth curve resulted, as shown in the adjacent figure.

Baldwin formulated this empirical relationship in the following equations:

$$D = 0.1083d^2 + 0.6917d + 0.75,$$

where

$$D = \log \text{diameter (feet)}$$

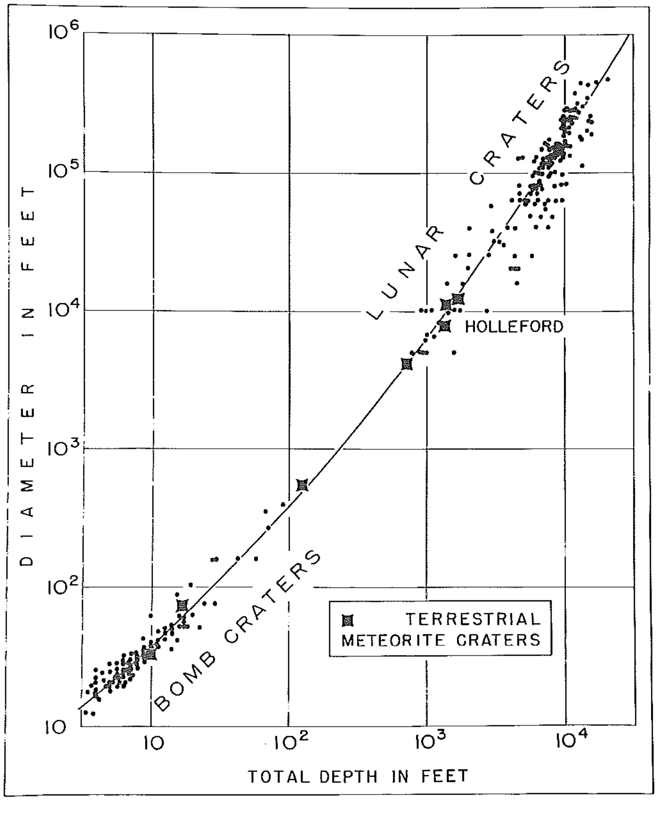
$$d = \log \text{depth (feet)}$$

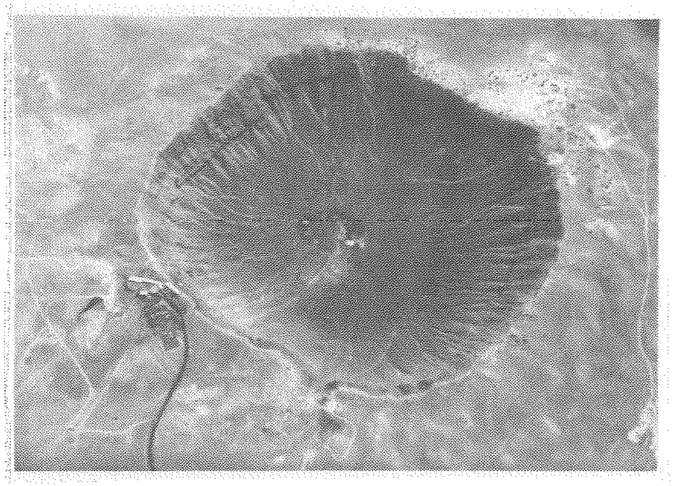
In addition, where H is the log of the rim height (feet), he found

$$H = - 0.097D^2 + 1.542\,D - 1.841.$$

If the Brent feature was due to a meteorite impact, the event had happened a long time ago; subsequent geological processes had filled it with sediments and badly eroded it. In its present form it was at best a crater remnant, a "fossil" crater, and it could not be expected to fit the Baldwin curve. Could ways be found to determine the original dimensions and to establish reliable identification criteria?

There were at this time, world-wide, some ten craters, or closely associated groups of craters, recognized as having a meteoritic origin. The identification was based on the presence of meteoritic material. The best preserved of these was the Meteor Crater in Arizona[3] where large amounts of meteoritic nickel-iron have been found in the debris surrounding the crater. It has a diameter of 4000 feet (1.2 km), its rim stands 150 feet (45 m) above, and its bottom 570 feet (175 m) below the surrounding countryside. The impact has formed a rim around the crater, some 800 feet wide, and from 130 to 200 feet in height. The impact has also lifted the surrounding strata, so that they slope away from the actual crater rim at an angle of about 13°. The rim owes its height in part to compression from below, in part to strata that have been overturned onto it, and in part to debris from the impact. Detailed studies show that the debris thrown from the crater by the impact consisted of angular rock fragments of all sizes, from dust to blocks weighing as much as 4000 tons, and that it was





The Arizona Meteor Crater.

dispersed to considerable distances. However a large proportion of the debris fell back, on to the rim and into the crater itself. Drilling has shown that this debris exists to a depth of some 600 feet, and that the rock beneath it is shattered; the shattering decreases with increasing depth. Much of the debris shows evidence of intense metamorphism. The large amounts of meteoritic nickel-iron found on the site indicate that the meteorite was an iron one, but there is no evidence of a main meteoritic body. It was destroyed in the impact process.

To anyone who has seen the Meteor Crater, its impact origin must seem obvious, but controversies, excellently documented by Hoyt[4], have raged over the question of the crater's origin for more than eighty years. Some have suggested that the surface strata have collapsed into an underground cave or that it is the result of erosion; others have proposed a variety of causes related to volcanism, for example a giant explosion of steam. Some distinguished American geologists remained unconvinced of its meteoritic origin for many years. It is a process that geologists have been reluctant to accept.

In addition to recognized craters, there are many structures around the world that have crater-like properties but in which no meteoritic remains have been found. Many of these consist of a central uplift of intensely disturbed and fractured rocks, surrounded by a ring-shaped depression. Despite the fact that most of these structures are remote from volcanic areas, most geological opinion was that they were the result of volcanic action. Perhaps volcanic gasses, associated with a rising column of lava, had exploded, causing fracturing of the overlying rocks and causing them to dome. Geologists called the structures cryptovolcanic, the "crypto" admitting a lack of understanding of the mechanism, the "volcanic" insisting that it was related in some way to the movement of magma. Dietz, the champion of the meteoritic origin of the moon's craters, believed that the cryptovolcanic features had the same source. He christened them "astroblemes"[5].

If the Brent feature is a fossil crater, or astrobleme, what remnants of the original crater might one expect to find? The rim is largely gone. The sediments that have filled the crater will be underlain by the fall-back debris, broken fragments of the crushed country rock, cemented into a *breccia*, by the fine rock flour produced by the impact. The inclusions will exhibit a great variety of sizes, and should show evidence of alteration due to the pressure and heat of the impact. Beneath the breccia the rock should be fractured, the amount of fracturing decreasing with depth. We shall see that these expectations were met.

The craters on the moon are almost numberless and range greatly in size. If they are due to meteorite impact, should the earth not have undergone a similar bombardment? If so, what better place to look for them than in Canada's ancient Precambrian Shield. Beals instituted a program[6] to search all the air photographs of the Shield for circular features. Each summer for a number of years two students pursued this task. When they discovered anything even approximately circular they would bring the photographs to Beals. If these seemed interesting to him he would arrange for further studies. This imaginative program led to the discovery of only one crater, that a major one.

Numerous other circular features were discovered in routine field work. These were investigated in more or less detail, depending on their interest and availability, by a number of people, with the result that several craters were usually under investigation at the same time. New findings in one study often modified proposed work in another, or sent researchers back to look for evidence of a sort not known when the initial work was done. The description of the work is necessarily somewhat confused. For the sake of clarity, the story will be divided into three periods; the craters studied in each period will be discussed as nearly as possible in the order in which the work began, and each section will be concluded with an analysis of the understanding gained.

Considering the reluctance with which geologists accepted the evidence for even well-preserved craters it is not remarkable that not all of them accept the story that I am about to tell. In recognition of their position, the word "crater" should everywhere be preceded by the adjective "possible", or "suspected". This would make for dull reading; I content myself with this acknowledgment of their disbelief[1].

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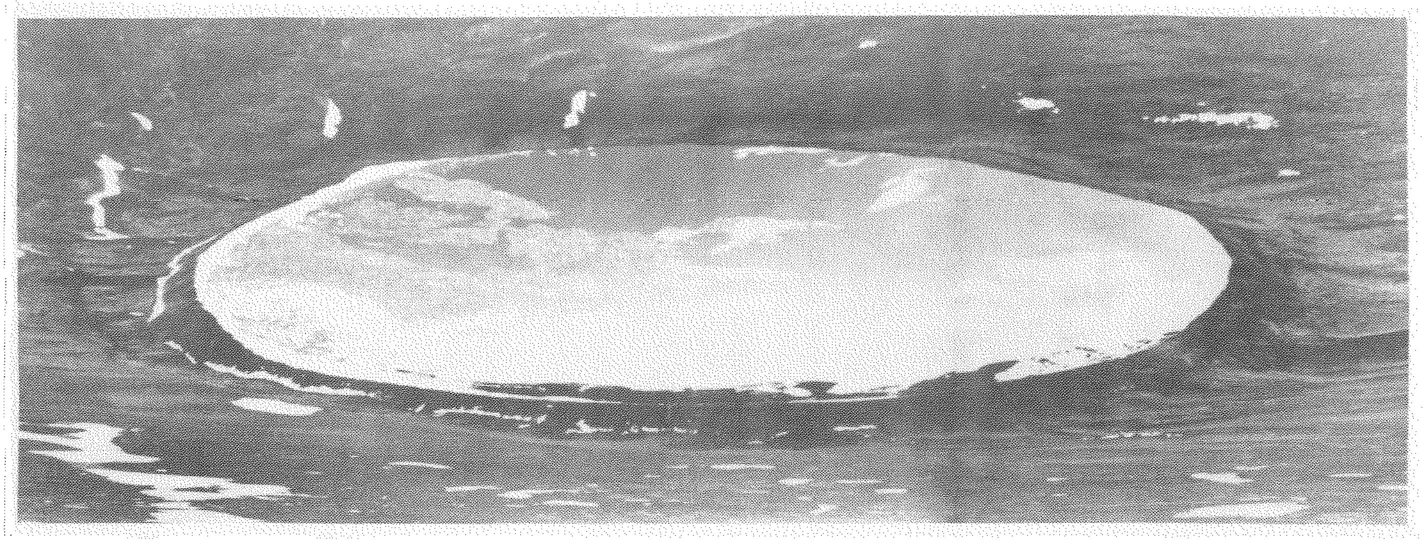
[1] Dr. Ian Halliday, who has been most helpful in the preparation of this chapter, is a little impatient with even this concession. "There is now no greater need to preface an impact crater with the word 'probable' than there is to describe Mauna Kea as a 'probable' volcanic feature. Humans have never observed volcanic activity there, so the presence of cinder conces, etc., and the similarity to the active Mauna Loa, make it very 'probable' indeed, but really no better proven than is the case for a meteoritic origin of a feature with all the shock criteria in evidence."



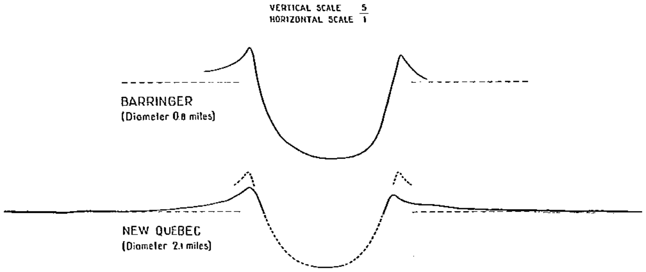
The New Quebec Crater.

THE INITIAL PERIOD – 1951-1962

The New Quebec Crater (61°17.0′N, 73°40.2′W)

The Observatory was not the first scientific organization in Canada to interest itself in the study of meteorite craters. That honour falls to the Royal Ontario Museum and to V.B. Meen. In 1950 a prospector, F.W. Chubb, drew Meen's attention to a crater-like lake shown in aerial photographs taken by the RCAF in Ungava, Northern Quebec. Chubb, assuming that the crater was volcanic in origin, was interested in the possibility of finding diamonds associated with it. A private expedition was arranged to explore the crater. On his return Meen[7] published a general description of the crater, concluded that it was of meteoritic origin, but noted a complete absence of meteorite debris despite the fact that, in his opinion, there was no evidence of glacial action. In 1951 a much more detailed study was made in a visit sponsored by the National Geographic Society and featured in their Magazine[8]. The width of the crater from rim to rim was measured as 11,500 feet, the diameter of the lake as 9100 feet, and the depth at the centre of the lake was 825 feet.

P.M. Millman, of the Observatory staff, felt that it was important to obtain as precise measurements of the crater as possible in order to test its impact origin against the Baldwin curve, and he requested the RCAF to make the necessary air photographs. This was done in 1953, and a topographic map of the area to a scale of approximately 1:25,000 was produced by the Surveys and Mapping Branch of the Department. The scale was approximate because there was no ground control in the area at the time the map was produced.



In the meantime, J.M. Harrison, shortly to become Director of the Geological Survey, visited the crater and from his work at the site as well as from the air photographs, concluded that it undoubtedly had an impact origin[9]. However, in contrast to Meen, he found evidence of heavy glacial action. Why then was the crater not filled with glacial debris? Harrison suggested that, as conditions suitable for continental glaciation developed, the lake froze and the crater filled with snow, thus providing a surface over which the glacier could slide.

Once the topographic map was available Millman[10] made a detailed study of it. He first had to establish its scale as accurately as possible, despite the lack of ground control.

The Observatory party, camped within the New Quebec Crater, is visited by some mining geologists. The rim of the crater is seen in the background.

This done, he drew 16 uniformly spaced diameters of the crater and plotted the profile along each of these. From these he was able to determine the mean diameter of the crater, rim to rim – 11,290 feet, and of the lake – 9430 feet, and to study the variation in the rim height. The height was greatest to the northwest where it reached a height of 455 feet above the lake level, least to the southeast where it reached a minimum height of 220 feet. He attributed the difference to the action of the glacier which, according to Harrison's findings, would have moved debris from the southeast toward the northwest.

How does the crater agree with the Baldwin curve? The depth agrees reasonably well, but the mean height of the rim is only about half that forecast. Glaciation has reduced the rim in height and softened its profile. The effect is demonstrated in the figure on the preceding page in which the profile is compared with that of the Barringer crater and, by the dashed lines, with Baldwin's theoretical values.

The Observatory mounted two expeditions to the crater, one in the winter of 1962-1963, one in the following summer[11]. A detailed gravity survey, supported by horizontal and vertical control measurements, was made over and around the crater, and numerous soundings were made of the lake. The gravity survey showed a negative anomaly centered symmetrically over the lake; by this time experience with other craters had shown this to be diagnostic of meteor craters. The control measurements permitted the Topographic Survey to prepare a new map of the crater area, contoured at ten foot intervals. E.M. Shoemaker, an authority on the Arizona Meteor Crater, was a member of the summer party; he made preliminary geological studies.

Airphoto of the Brent Crater.

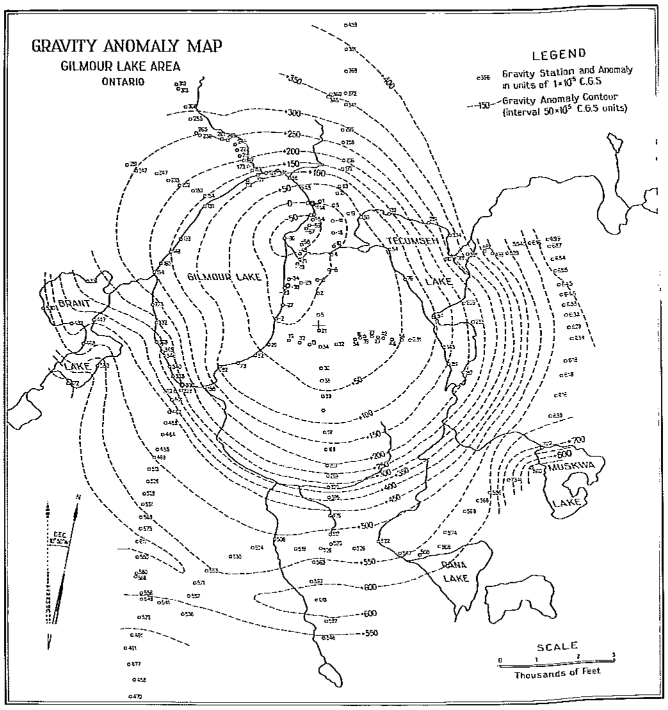
Brent, Ontario (46°04′31′N, 78°29′00″W)

This is the most thoroughly studied of all Canadian craters, and the sequence of investigations established here became a model for that on others. Studies for the period 1951-1958 were described in a joint paper by all the scientists involved[12].

The first step was a small exploratory expedition to study the area on the ground. The party consisted of Millman, two magneticians, W.E.T. Smith and A.A. Onhauser, and H.M.A. Rice of the Geological Survey. The expedition covered a six-day period in July, 1951.

Rice made a preliminary geological reconnaissance. The Precambrian country rock was cut off sharply at the crater boundary, and none was found within the crater. Outcrops of limestone and shale of Ordovician age were found at a number of points within the circle. Breccia outcropped at several points near the crater boundary and a number of chunks of cemented breccia, some containing large blocks of gneiss, were found within the circle.

While Rice was examining the geology, soundings were taken along three traverses across Gilmour Lake, the larger of the two lakes lying within the crater, and some magnetic observations were made. There was nothing unusual about the water depths, and the magnetic map was remarkable only in the flatness of the contours over the crater as compared with



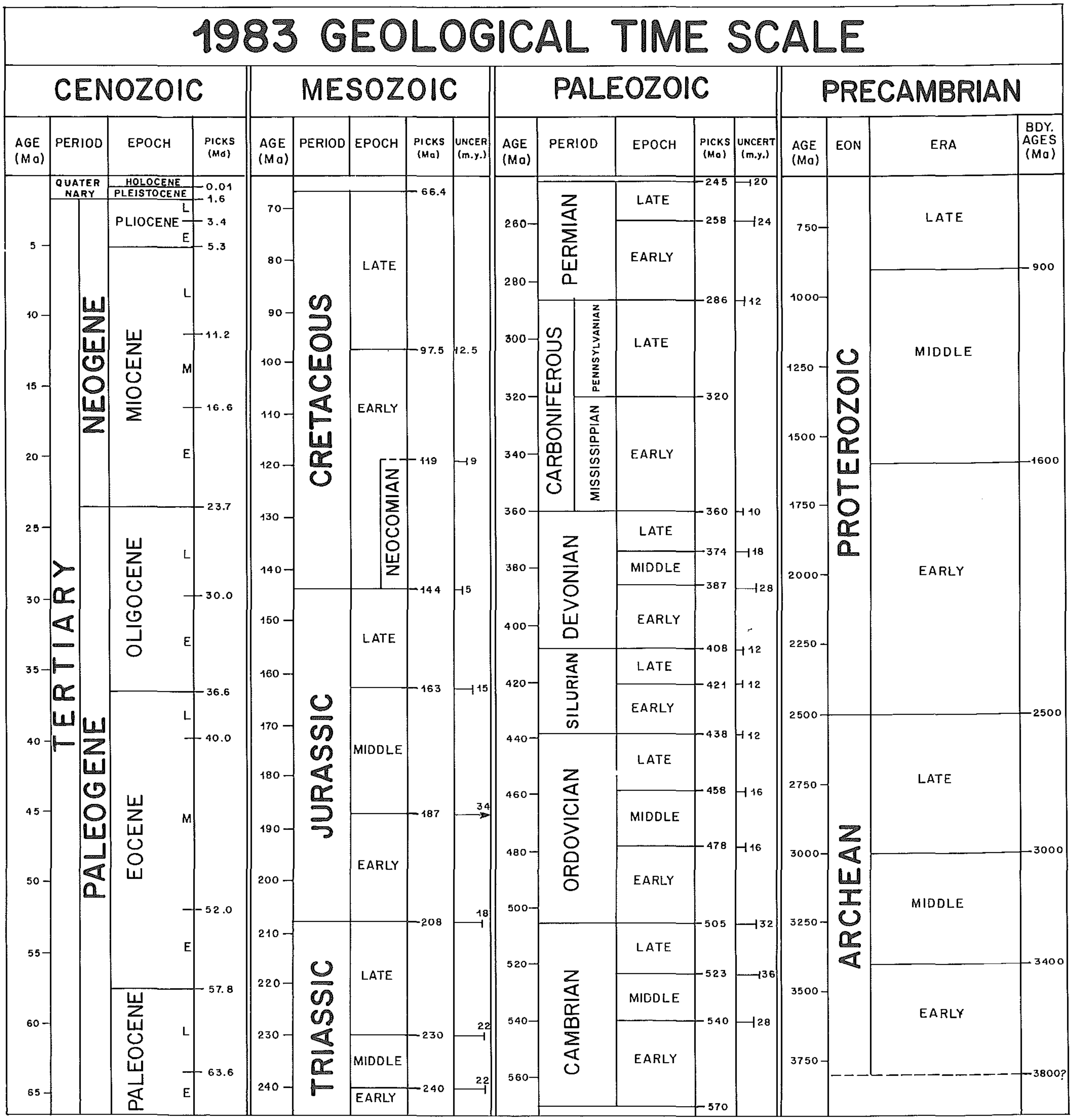
Gravity map of the crater area.

their convolutions outside it. This was confirmed shortly afterwards when the Geological Survey completed a total-force air-borne magnetometer survey of the area.

There were three conclusions; Precambrian country rock was absent within the crater; there was no evidence of a volcanic plug which would have been present if the crater had a volcanic origin; there was no magnetic debris from a meteor.

End of step one; a meteoritic origin was possible, although no meteoric material had been found. Beals approached J.M. Harrison, now Director of the Geological Survey, proposing a joint program to investigate Canadian meteorite craters. The invitation was declined, and the Observatory was on its own. This is not to suggest that the Survey was unwilling to help. B.A. Liberty was assigned to advise Beals on geological aspects of the work at Brent and over the years

Table 1. The Geological Time Scale. Under each of the Eras – Cenozoic, Mesozoic, Paleozoic and Precambrian – the left-hand column gives the age in millions of years. This table will be useful in understanding many of the details in this and the following chapters.



1983 GEOLOGICAL TIME SCALE

CENOZOIC

AGE (Ma)	PERIOD		EPOCH		PICKS (Md)
	QUATER NARY		HOLOCENE		0.01
			PLEISTOCENE		1.6
	TERTIARY	NEOGENE	PLIOCENE	L	3.4
5				E	5.3
10			MIOCENE	L	11.2
15				M	16.6
20				E	23.7
25		PALEOGENE	OLIGOCENE	L	30.0
30 35				E	36.6
			EOCENE	L	40.0
40 45 50				M	52.0
55				E	57.8
60			PALEOCENE	L	63.6
65				E	

MESOZOIC

AGE (Ma)	PERIOD	EPOCH		PICKS (Ma)	UNCER (m.y.)
				66.4	
70 80 90	CRETACEOUS	LATE		97.5	2.5
100 110		EARLY		119	9
120 130 140			NEOCOMIAN	144	5
150 160	JURASSIC	LATE		163	15
170 180		MIDDLE		187	34
190 200		EARLY		208	18
210 220	TRIASSIC	LATE		230	22
240		MIDDLE		240	22
		EARLY			

PALEOZOIC

AGE (Ma)	PERIOD		EPOCH	PICKS (Ma)	UNCERT (m.y.)
				245	20
	PERMIAN		LATE	258	24
260 280			EARLY	286	12
300	CARBONIFEROUS	PENNSYLVANIAN	LATE	320	
320 340		MISSISSIPPIAN	EARLY	360	10
360	DEVONIAN		LATE	374	18
380			MIDDLE	387	28
400			EARLY	408	12
	SILURIAN		LATE	421	12
420			EARLY	438	12
440	ORDOVICIAN		LATE	458	16
460			MIDDLE	478	16
480 500			EARLY	505	32
520	CAMBRIAN		LATE	523	36
540			MIDDLE	540	28
560			EARLY	570	

PRECAMBRIAN

AGE (Ma)	EON	ERA	BDY. AGES (Ma)
750	PROTEROZOIC	LATE	900
1000 1250 1500		MIDDLE	1600
1750 2000 2250		EARLY	2500
2500 2750	ARCHEAN	LATE	3000
3000 3250		MIDDLE	3400
3500 3750		EARLY	3800?

there were numerous instances of Survey assistance in geological aspects of the project. But the program was an Observatory one, regarded with various degrees of skepticism by Survey members.

Next came a gravity survey of the crater and the surrounding area. This was made by M.J.S. Innes in 1953. The anomaly map is shown on page 92. The pattern of circular contours, defining a gravity low centered over the crater, proved to be typical, and almost diagnostic, of impact craters. It is what one would have expected if sediments and breccia, less dense than the surrounding country rock, filled the crater. The density of the supposed sediments and breccia was not known, but by assuming reasonable values Innes estimated that the depth to undisturbed rock was 3640 feet below the present surface.

During the gravity work Innes carefully surveyed the position and elevation of his stations, thereby providing ground control for the interpretation of the air photographs. The Topographical Survey was then able to produce a topographic map of the area. Millman studied this map, as he had studied that for the New Quebec Crater. The diameter of the circle proved to be 9500 feet but, since erosion has planed off much of the surface, this is not the original diameter of the crater. It was necessary to project the slope of the crater sides upwards by the amount of erosion to obtain the original dimension. Millman suggested that this was about 11,500 feet. Using this figure in the Baldwin formulae he estimated that the original height of the rim was 662 feet, the depth of the crater 900 feet, both measured from the original ground level. These were tentative values useful in planning the drilling program.

The next step in the program involved refraction seismology, a discipline which is difficult to apply to crater studies for reasons well described by P.L. Willmore[13]. "In the refraction method charges are set off in the ground, and the times of arrivals of the seismic waves are observed at various distances from the shots. If the structure consists of a succession of plane layers, and if the velocity of propagation in each layer exceeds that in the layer above, the 'travel-time curve' will consist of a series of straight lines, each of which will represent waves propagated through one of the layers of the structure. With suitable arrays of shots and seismometers it is possible to determine the propagation velocity, thickness and dip for each layer, but the geometry of the ray paths usually requires the observations to be conducted over horizontal distances about six times as great as the thickness of the structure being investigated.

A crater-like structure presents special difficulties ... for its depth is comparable to its horizontal extent and it may be impossible to set out a refraction spread long enough to determine velocities in the various formations."

Willmore overcame these difficulties in an ingenious way. A 600 foot "spread" of six equally spaced seismographs was set out in a succession of four different positions with the crater. Shots were fired at increasing distances to as much as 3000 feet off each end. These distances were not sufficient to ensure penetration beneath the crater, but two flat-lying marker layers within the crater, with velocities of 10,500 ft/sec and 14,150 ft/sec were indicated.

Two additional seismograph stations had been set up well beyond the boundary of the crater, one off each end of the line of shots, and the shots used in the short-range work were recorded at these distant stations. Arrival times at these stations were delayed by various amounts with respect to the time that would have been taken if the wave had traversed undisturbed rock; the delay could be interpreted in terms of the thickness of the disturbed rock within the crater. When these thicknesses were plotted they outlined a rough crater with a central depth of about 1000 feet.

One of the shots had been recorded at the village of Brent, some three miles away. Its delay time in comparison to a normal Shield velocity was much larger than would be expected given a crater depth of 1000 feet. Willmore interpreted this additional delay as being due to a fractured zone, as much as 4000 feet in extent, surrounding the actual crater. This value agrees well with the gravity-based value of 3600 feet.

The stage was now set for diamond drilling. The first drilling was done in 1955. Two holes were proposed, one near the centre of the crater, the other close to its edge. It was hoped that each of these would reach the expected breccia, and perhaps penetrate it to reach the crater floor. Unfortunately, great difficulty was encountered in the drilling and the central hole, although it penetrated to a depth of 554 feet, did not reach breccia. The rock sequence was a normal Paleozoic one, of early Ordovician and, possibly, late Cambrian sediments.

The hole near the edge however, though its total depth was only 202 feet, did enter a zone of gneiss breccia at 150 feet and continued in it to the bottom of the hole. This was encouraging, but was not regarded as proof of an impact origin, since outcrops of breccia, in situ, had been found in the rim.

Difficulties with the drilling were due to two factors, the poorly consolidated sediments and the use of too-light drilling equipment. The sediments were eroded latterly by the drilling fluid to provide a hole much larger than the drill bit, and the too-flexible drill rods could whip about in this enlarged hole, which resulted in diminished efficiency and sometimes in the loss of rods and bit.

By this time a second major feature, at Holleford, Ontario, was being studied, and the next drilling was at that site. Heavier equipment was used and successful drilling techniques were developed. The drilling program at Brent was resumed in 1959, making use of this improved technology[14]. In that year a hole was successfully drilled to a depth of 3500 feet at the centre of the crater; core recovery was almost complete. The hole reached the bottom of the sedimentary layer at 851 feet and encountered fragmented material and gneiss breccia. This consisted of fragments of all sizes, from small particles to blocks several feet in thickness, and was similar to that found on the crater rim and in the earlier, rim, drill hole. No definite level could be established for the base of the breccia; it graded into fractured country rock at about 3000 feet, at which point a 150 foot sequence of rocks, interpreted as coming from a lava sill, were encountered.

The breccia lens had been encountered about 150 feet deeper than forecast on the basis of Millman's estimate of the original crater diameter. It was necessary to revise this estimate upwards to 14,700 feet.

The existence of the breccia and of the fractured rock beneath it was exciting support for the impact origin of the crater, but in order to define the breccia lens more precisely seven additional holes were drilled in 1960-1961; they ranged in depth from 400 to 1200 feet. Finally, in 1967, two deep holes were completed, one to 3500 feet near the centre of the crater, the other, halfway to the rim, to 2400 feet. These holes refined the picture of the crater profile.

The fragmented material in all the cores taken close to the centre of the crater showed evidence of having been subjected to extreme heat, all appearing to have been melted to varying degrees[15]. This further sustained belief in the impact origin of the crater. What was its age? Since the crater was in the Precambrian basement, and was filled with middle Ordovician sediments an upper and lower limit could be set – 600 to 470 million years.

Holleford, Ontario (44°28′N, 76°38″W)

The Holleford crater was discovered in the routine search of air photographs$_6$. It lies about 17 miles northwest of Kingston, about 2 miles off highway 38. The crater includes woodland, pasture and arable land and a number of roads run through it. Its accessability made its investigation logistically simple.

It has a diameter of about 1½ miles, but it has been so badly eroded that the perimeter is not recognizable everywhere. It is covered by Ordovician sediments which, where they outcrop within the crater area, dip toward its centre. Subsequent drilling showed that the Paleozoic column extended to the middle Cambrian, and this is usually taken as the time of the impact, indicating a crater age of 550 million years.

Beals took a personal interest in the study of the Holleford crater and published the principal paper on the investigation[16]. The steps taken in the study followed closely the pattern set at Brent. The Geological Survey had already surveyed the area with its total-force airborne magnetometer; as at Brent the crater was marked by simple uncontorted lines in an otherwise complex area. As we have seen, this indicates, among other things, an absence of residual meteorite material, a matter to which Beals gives a good deal of attention.

Willmore attempted a seismic survey of the crater. The difficulties of working within the confining limits of a crater have been explained earlier. Experiments inside the crater defined the velocity of the limestone cover, but no underlying beds of higher velocity could be reached. This was taken to indicate that the material in the crater beneath the limestone was of lower velocity. To check this, recording stations were set up well outside the crater, and shots were fired inside and outside the crater, at the same distance from the stations. The crater shots were indeed later in arriving, indicating the existence of a delaying low velocity material under the crater.

As at Brent, the crater produced a negative gravity anomaly of almost circular contours. These were analyzed by Bancroft[17]; they suggested a mass of low-density material, presumably breccia, beginning at about 300 feet and extending to somewhere between 700 and 1600 feet, depending on the density assumed for the breccia. These estimates provided a guide to the drilling program.

The first hole was located at a distance of 1400 feet from the estimated centre of the crater. Guided by the experience at Brent, a heavier drilling rig was used and the hole started with much larger drill rods and bit, permitting a succession of casings to be set when difficulties were encountered. Despite this care, serious difficulties were encountered, including a gas blow-out that caught fire and did much damage to the drilling equipment. The hole reached 1128 feet, at which point the rods became stuck and the hole was abandoned.

The gas blowout had a surprising consequence. H.D. Babcock, the farmer on whose property the hole was being drilled, had a well inside his barn which had supplied water for all his needs for many years. After the blowout the well went completely dry for several days; eventually some water returned but it was never sufficient for the farm needs. The main drilling program had to be deferred while a new well was drilled for farm use. A good supply of water was eventually found, but the well was 700 feet away from the barn, rather than in it.

Two additional holes were drilled; it is interesting that the Observatory drilling budget had been exhausted and that these two holes were paid for by the Geographical Branch of the Department. The second hole was at a distance of 2500 feet from the centre of the crater, and had a depth of 1486 feet; the third hole, 3750 feet from the centre, close to the rim of the crater, reached a depth of 443 feet.

A large amount of core was recovered from all three holes. The first hole penetrated a sequence of Paleozoic sediments to a depth of 750 feet and then found the expected breccia. It did not get through them. The second hole passed through a thinner layer of the sediments, 440 feet, and through 160 feet of breccia. Below this, numerous cracks were suggested by fluid loss in the drilling and by examination of the cores. The fragmented material which made up the breccia was all Precambrian rock of the type found in the immediate vicinity. Hole three, at the crater rim, contacted a thin layer of the sedimentary cover and a thin layer of the breccia. The contour of the crater floor defined by the drilling, comes close to the mean position of the New Quebec and Brent craters. This, and the presence of the breccia and underlying fractured rocks, are strong evidence for an impact origin.

Beals was eager to find any residual material from the meteorite, and arranged to have J.L. Roy measure the magnetic moment of a selected suite of samples on the recently-developed astatic magnetometer. No unusual material was found. Beals points out however that the diameter of the drill holes represents 2.2 x 10^{-10} of the area of the crater; it would be possible for substantial amounts of magnetic material to be missed completely. He also speculates that the meteorite might have been a stone one, rather than an iron. The densities of the cores were measured.

How large a meteorite would have been required to produce the crater? Prof. M.S. Macphail, of Carleton University, considered this problem over a period of two summers and suggested that a stone meteorite would have had to have a diameter of 100 metres.

Deep Bay, Saskatchewan (56°24.4′N, 102°59.4′W)

Reindeer Lake, which lies in northern Saskatchewan, about 250 miles north of Prince Albert, is a typical Canadian Shield lake, shallow and surrounded by flat-topped rock exposures rising to moderate elevations. Typical except for one thing; at its southeastern end there is a deep, perfectly circular, bay, more than six miles in diameter. This is Deep Bay, an object of superstitious fear to local Indians.

Innes first noticed the feature in 1947 when he was making regional gravity measurements in the area[18]. As the program of crater studies developed, he remembered the feature and in 1956, with the cooperation of scientists from the Saskatchewan Division of Mineral Resources, he made a preliminary visit to it. The general dimensions, including the depth of the lake, were measured, and gravity observations were made around the rim and along several radiating lines to distances of several miles. Corrected for irregularities of terrain, the results showed an increase of gravity in all directions from the margin of the bay. In the course of this work extensive fracturing of the country rock was observed.

During this preliminary visit, sufficient topographic traverses were made to permit the preparation of a photogrametric map by the Topographical Survey, to a scale of 1 inch to a mile. The map confirmed that the lake had a diameter of nearly six miles, and was surrounded by a rim, marked by a height of land, concentric with the lake, 8 1/2 miles in diameter. The ridge has been deeply eroded but has a maximum height of 400 feet above the lake, with an average value of 270 feet. Close examination of air photographs confirmed the rock fracturing observed on the ground; much of it was concentric with the crater and these concentric rings, as well as the rim itself, were cut radially and obliquely by fault zones of various widths.

The Geological Survey made a total-force airborne magnetometer survey, which showed, as usual, a less convoluted pattern within the lake than in the surrounding region, although there were some details, two lows and a high, both minor, within the lake area. The evidence was thus all in favour of an impact origin.

A more complete gravity survey was carried out during the winter of 1959. Observations were made on the ice surface, using a gravity meter recently adapted for use in the Arctic. During the subsequent summer season the density of observations out to a distance of 30 miles was improved. The Bouguer anomaly map showed the characteristic circular negative anomaly but the anomaly was much larger, -15 mGals, than had been found at Brent or Holleford. This was not surprising, considering the much larger diameter of the Deep Bay crater.

In the winter of 1961 G.W. Sander and A. Overton[19] made a seismic survey of the crater. The dimensions of the crater obviated many of the difficulties that Willmore had faced earlier, since penetration to basement rock could be expected within the available 6 mile range. A line was laid out in a SE-NW direction, crossing Deep Bay and extending northwestward into Reindeer Lake. Twenty-four geophones were used to cover a uniform "spread" of one-half mile. This spread was moved across Deep Bay, occupying a succession of positions; at each position shots were fired at the crater rim at both ends of the Deep Bay section.

It was immediately evident that penetration to sub-crater material was being obtained, but the velocities in the two directions were not the same, indicating that the refracting bed was dipping. An ingenious method was used to overcome this. While the velocity in normal Shield rocks should be about 20,000 feet per second, the best estimate from the "dipping bed" values, and a value found outside the crater, was 15,000 feet per second. This was taken to be the velocity in the fractured bedrock underlying the crater. Travel-time lines were drawn through the shot points at the appropriate slope. The actual arrival times were much later than this line. Part of the delay was due to transmission through the water; this could be allowed for since the water depths were known. The remaining delay was due to propagation in the brecciated zone below. By assuming a reasonable velocity for this they were able to profile the crater bottom. They estimated a maximum thickness of the sediments of 2100 feet.

Measurements were also made to the northwest, into Reindeer Lake. Beyond a distance of 12,000 feet, normal Precambrian velocities of 20,000 ft/sec were observed, but closer to the crater the measured velocity was only 15,000 ft/sec. This suggested that fracturing of the country rock extended to a distance of 12,000 feet from the rim, 39,000 feet or about seven miles from the centre.

R.D. Bataille assisted Sander and Overton in the seismic work and also produced a geomagnetic map of the crater. This map, taken at ground level, showed more detail than the airborne magnetometer map. Two small anomalies, one under the crater and the other at its rim, were estimated to be due to sources at depths of 2000 and 300 feet, in good agreement respectively with the seismic estimates of depth to bedrock and the known depth to the lake bottom.

The stage was now set for drilling. This was done during the winter of 1961-1962, and is described in a paper which summarizes all the work done on the Deep Bay crater[20], including a detailed account of the geology of the area. The drilling rig was mounted on the ice which meant that the drilling rods had to be lowered through several hundred feet of water before actual drilling commenced. Since drilling companies had little experience with this problem a site for the first hole was selected, not at the centre of the crater, but three quarters of a mile west, where the water depth was about 400 feet. Even this water depth made for severe difficulties. Casing could be hung through the water, but there was no way of supporting it. Much casing, and several hundred feet of drill rods were lost and the hole had to be terminated at a depth of

591 feet below the lake surface. At this point it had penetrated about 100 feet of silt and poorly compacted shale and some 90 feet of a more compacted shale.

A second hole was drilled much closer to shore where the water depth was 240 feet, and, although many difficulties were encountered, it was carried to a depth of 1420 feet. Contrary to expectations it did not encounter any sediments, but went immediately into a sequence of the country rocks more or less badly fractured and held in a matrix of unconsolidated rock powder. This was puzzling; the hole should have encountered breccia. Innes concluded that the crater must be smaller than originally thought. Making use of Baldwin's formulae, he arrived at the following dimensions:

Original rim diameter – 31,000 feet (5.9 miles)

Rim height – 890 feet

Crater depth – 3210 feet

Innes estimated the sediment thickness to be not less than 1600 feet, in good agreement with the seismic estimate of "not more than 2100 feet."

Sediments in the crater are of lower Cretaceous or upper Jurassic age, some 145 million years old. The crater itself was formed in granite gneiss believed to be 1,700 million years old; it has not been possible to narrow the age estimation beyond these limits.

Review of Research to 1962

With preliminary work on these three craters completed, two important review papers considered the findings and pointed the way to further work.

Although the first of these, by Beals, Innes and Rottenberg[14], did not appear until 1963 because of publication delays, it was written before the results of the drilling program at Deep Bay were available. It first considers the theory of crater formation, making use of data provided by nuclear explosions. Defining a "ruptured" zone as one in which the shattered rock is completely disassociated from its original position, and a "fractured" zone as one in which the rock, though fractured, is still essentially in situ, they showed that the zone of rupture should extend to a depth of one-third the crater diameter; the zone of fractured rock should extend to a depth of two-thirds the crater diameter.

The paper then goes on to enquire how various post-impact histories might modify the appearance of the crater, and defines eight hypothetical types of fossil craters. The known craters, Brent, Holleford and Deep Bay, as well as a number of circular features not yet studied in detail, are examined in relation to these types. One possibility is particularly interesting. Impact by a very large meteorite would cause a fracture zone extending well into the earth's mantle; this might give rise to secondary volcanism with a possible central volcanic peak. A number of the lunar craters seem to suggest this origin. In an effort to reconcile points of view of Survey geologists with the Observatory team, J.M. Harrison is quoted as suggesting that an impact of sufficient violence might release latent volcanism and so complicate the interpretation of fossil craters.

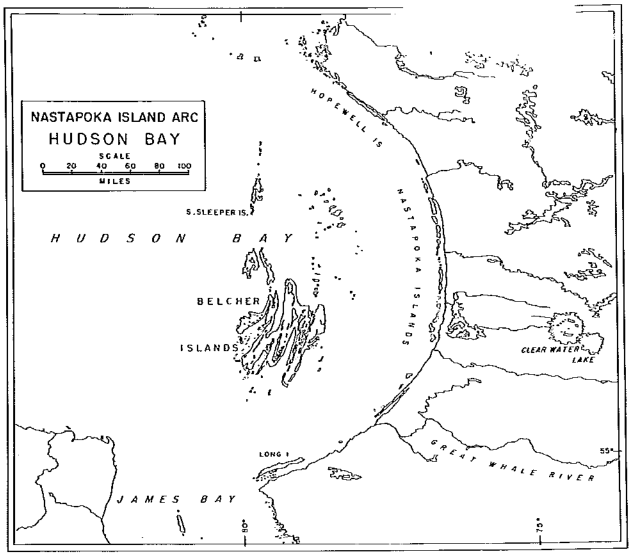
The second paper, by Innes[21], was written before drilling had been undertaken at Deep Bay, but with the cores from Brent and Holleford available for density measurements. He notes that the gravity anomalies for the three craters increase, negatively, with the crater diameter: -2.2 mGals for Holleford, -5.5 mGals for Brent and -20.1 mGals for Deep Bay. By a process of integration, he translates these anomalies into mass deficiencies and, assuming reasonable values for its density, determines the mass and volume of the ruptured rock. The results compare well with those determined on theoretical grounds by Rottenberg[14]. By considering the energy involved in crushing these volumes of materials he derives impact energy level in the range 10^{23} to 10^{25} ergs.

In the early 1960s interest began to grow in the effects of shock waves on rocks, and petrology became an indispensable tool in the study of the crater cores. Beals was well aware of this development and determined to be part of it. An Australian petrologist, M.R. Dence, was recruited in 1962, a petrological laboratory was set up and a technician, Rene Wirthlin, was hired to assist in the work. P.B. Robertson joined the group in 1965. It operated as a section in the Gravity Division under the general supervision of Innes.

THE SECOND PERIOD – 1962-1964

Clearwater Lakes, Quebec (56°N, 74°30′W)[22]

The east coast of Hudson Bay is one of largest and most conspicuously circular features in Canada. It has been called the Nastapoka Arc, after the islands that follow the feature for much of its length. One is less likely to notice a connected pair of circular lakes about 100 miles to the east of the larger feature. These are the Clearwater Lakes; Beals's attention was



Hudson Bay, the Nastapoka Arc and the Clearwater Lakes.

drawn to them as possible impact structures by our Deputy Minister, Dr. Marc Boyer, and by the Quebec Department of Mines. The West and East lakes have, respectively, diameters of 19 and 13 miles and depths of 98 and 525 feet. The larger lake has a ring of islands concentric with the crater. These are of breccia in a crystalline matrix and they overlie strongly shattered and altered country rocks.

The pattern of the lakes suggests that they were formed by a pair of meteorites. Such pairs of closely-bound asteroids are not common, although some of the lunar craters might have been formed in this way. Could such a pair exist and remain together as they travelled through the solar system? R.W. Tanner[23] investigated this question and concluded that they could.

Preliminary work was done in the area in 1958; the gravity field was investigated as part of the regional survey, and S.H. Kranck[24] made a geological examination of the feature. He attributed the craters to volcanic processes, but noted the presence of maskelynite, a glassy mineral that was shown to be a form of feldspar, altered by intense shock pressures; it was later to prove of importance in the identification of impact craters.

D.B. McIntyre, a Scottish-American expert on the Precambrian, was attached to the gravity party during the 1958 regional survey. He collected a suite of what he took to be volcanic rocks in Clearwater West. When these were examined in thin section he found that they were quite unlike anything previously described. Some were microbreccias in which progressive stages of brecciation and deformation were displayed. Others, which looked like slightly weathered granite, were completely reconstituted. McIntyre adopted the hypothesis of impact metamorphism and published a series of superb photomicrographs[25] that were the first explicit recognition of shock metamorphism in Canadian craters.

A more detailed gravity survey was made during the winter of 1961-1962 as part of the expedition to make similar measurements at the New Quebec crater[11]. The diagnostic negative anomaly defined by circular contours, was found over each lake although the anomalies were much less than would have been expected, given the large diameters of the features. Sanders made a preliminary seismic traverse across East lake, which gave some evidence of low velocity material underlying the crater, but he left the Department at about that time and the work was never completed.

In the late summer of 1962 a hole was drilled on Neilson Island of the Nastapoka Arc to test the Hudson Bay crater hypothesis; the rig was flown to West Clearwater in the winter of 1963 and a second rig was flown to East Clearwater shortly thereafter. Both rigs were busy until April when ice conditions deteriorated. Five holes were drilled in the larger, West lake, all of them relatively shallow. Holes near the ring of islands encountered breccia similar to that found on the surface, with shattered basement rocks beneath it as expected, but two holes near the centre of the lake met only the shattered basement rocks beneath a thin layer of unconsolidated material. There was no sign of the breccia!

One hole was drilled in the smaller, but much deeper, East lake where great difficulties were experienced because of the water depth. The hole reached a depth of only 1116 feet; 151 feet of unconsolidated material was underlain by 367 feet of sediments. The hole then entered breccia which contained small fragments of the local Precambrian rocks in a matrix which was almost igneous. The hole bottomed in this breccia layer.

The igneous-like matrix, similar to lava, was also found in the breccia forming the greater part of the ring of islands in the West Lake, indicating that a great quantity of rock has been melted and consolidated.

One of the rigs was left over the summer and in the winter of 1964 it was positioned near the centre of the East lake. The drilling company, Heath and Sherwood, developed a method that solved the problems of drilling through deep water. Casing was lowered through the water and anchored at the bottom by lead weights. The upper half of the casing was supported by cylindrical steel floats that provided buoyancy to the drilling pipe almost equal to its weight. A hole was successfully drilled through 404 feet of water to a total depth of 3320 feet[26]. After passing through some glacial till and a thin layer of post-crater sediments, the hole encountered almost 2900 feet of heavily shocked and fractured gneiss, showing various degrees of recrystalization. Instead of being a simple bowl, such as at Brent, the crater had a central mound of uplifted Precambrian basement rocks!

Manicouagan, Quebec (51.4°N, 68.7°W)

This feature has the form of a circular trench nearly 30 miles in diameter, defined principally by two lakes, Manicouagan and Muschalagan. The feature is quite unlike the craters we have been considering; instead of a central depression it has a hill, known as "Mont de Babel", which rises nearly 1000 feet above the surrounding plateau. This suggests a similarity to Clearwater West, and the feature was regarded as a probable crater from about 1964.

The initial expedition to study the feature took place in 1954; E.R. Rose, of the Geological Survey, made a geological study of the crater and surrounding area[27], Innes conducted a gravity survey, the results of which were included in the regional gravity survey of central Quebec[28], and Willmore carried out refraction seismic work[29].

The geological study showed that the suspected crater lies in an extensive area of Grenville gneiss, that the central boss is an intrusion of anorthosite and that the area between this central intrusive and the lakes is largely covered by extrusive rocks. The gravity picture was not that of a typical crater; the lakes lie at the southern margin of a trough of strongly negative southwest to northeast anomalies that coincide with the Grenville Front, the northern boundary of the Grenville Province of the Canadian Shield. Within the crater the gravity field is complex, with some indication of a modest negative anomaly over the arcuate lakes which outline the structure.

One might have hoped that the size of the feature would have made an in-crater refraction spread possible or, failing that, a line of shots inside the crater with receiving stations at the edge, as at Brent. These proved to be impossible because of the absence of outcrops within the circle on which to set up the seismometers and of the scarcity of lakes to be used as shot points. One short refraction spread was obtained making use of a fairly large lake toward the centre, but except for this, seismographs and explosions had to be set where logistics permitted and where outcrops, or suitable lakes, existed.

When the data from the random distribution of shots and recording stations were plotted on a conventional time-distance curve, they suggested a two-layer structure, but the deviations from the least-square lines were too great to inspire confidence. The solution of the problem, and the published paper, had to await the development of the time-term method[30], which will be described in the chapter on seismology. It will suffice to state here that the method yields, for each shot point and seismograph station, the time required for a seismic wave to travel vertically to the refracting layer.

When the time-terms had been calculated they showed a trend from low values to the north and west to higher values in the south and east, a trend that parallels the gravity anomaly and bears no relationship to the circular pattern of the feature. Furthermore the velocities obtained for the "marker layer" and the overlying material bore no relationship to velocities in observed rocks in or near the feature. The conclusion had to be drawn that, as the geologists had originally suggested, the feature was a result of differential erosion.

This conclusion was only a temporary one. New forces were moving in crater studies, which were to return Manicouagan to the list of probable craters.

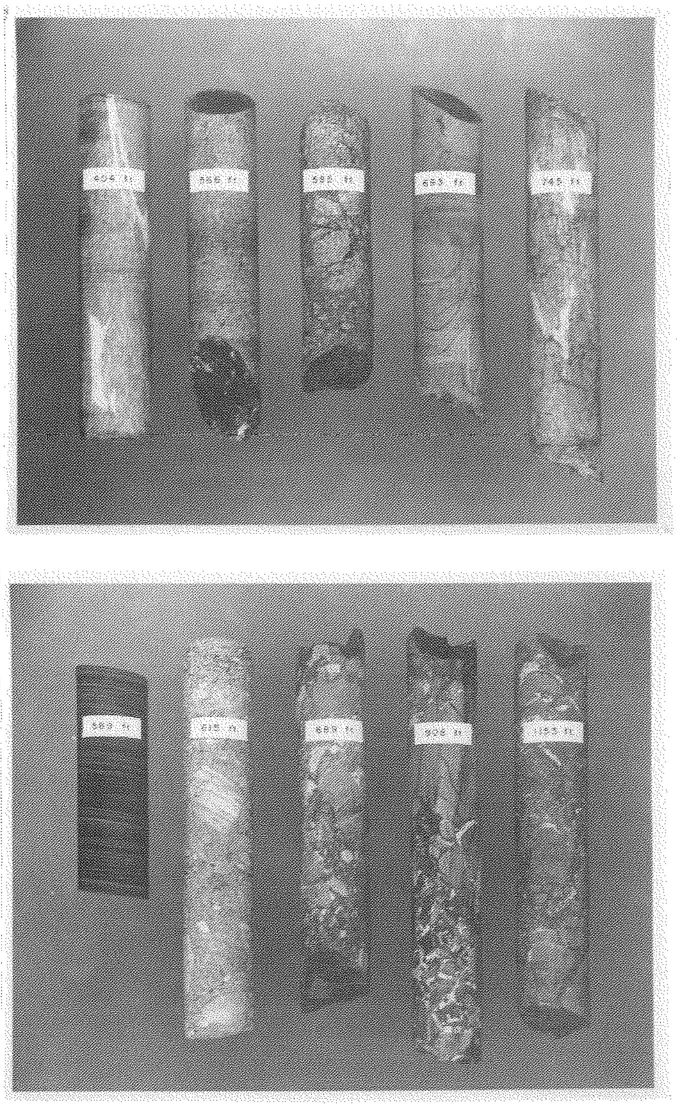
West Hawk Lake, Manitoba (49.8°N, 95.2°W)

West Hawk Lake is a popular summer resort in the Whiteshell Provincial Park in Manitoba, a half mile off the Trans Canada Highway, close to the Ontario border. It lies in the southwest corner of the Superior Province of the Canadian Shield, about ten miles east of its boundary with the Interior Plains[31]. Roughly circular in shape, it has a diameter of about 2¼ miles. There are many cliffs around the boundary of the lake, rising 30 to 50 feet almost vertically from the water. All these cliffs are jointed and fractured.

The possibility that the lake was a meteorite crater was suggested to Ian Halliday in 1957, by Dr. Morley Wilson, of the Geological Survey. At the same time Halliday learned that the Manitoba Department of Mines and Resources had prepared a contour map of the water depths; these exceeded 350 feet at the deepest point, and were greater than 200 feet over much of the lake. Halliday visited the lake in 1958 and was impressed by its possibility as a crater, but everyone was too involved with the study of existing craters to take on a new study. At Beals' suggestion, Halliday and A.A. Griffin undertook the work themselves.

They made two gravity surveys, the first in March 1962, on the ice of the lake, the second, in October, a ground survey covering an area eight miles in diameter centered on the lake. When corrections were made for topography the tell-tale pattern of circular contours, centred on the lake, emerged. The maximum anomaly was about -6 mGals. On the basis of this anomaly a crater depth of about 4000 feet was forecast. The next step was to drill[32].

The first hole was drilled, from the ice, early in 1965. It was located at the centre of the lake where the water depth was 357 feet. The drilling company, Canadian Longyear, using the techniques developed in 1964 at Clearwater Lake East, met the problems posed by this water depth effectively. The hole was drilled to a total depth below the water surface of 2385 feet. The typical impact crater profile was found, some 300 feet of sediments and 1000 feet of breccia underlain by fractured rock still in situ.



Drill cores from the West Hawk Lake crater. The set above, from a hole remote from the centre, shows little fracturing and only occasional brecciation. The lower suite, from a hole near the centre, shows preglacial sediments overlying breccia increasingly fractured and of increasingly greater size. The deeper cores showed extensive evidence of pressure deformation.

In 1966 three additional holes were drilled, one at a distance of 3800 feet from the centre of the crater, the others moderately close to it. These holes all ran into a layer of glacial boulders, which caused great drilling difficulties, including the loss of a string of tools in one of them. The first hole, remote from the centre, showed little fracturing and only occasional brecciation. The hole close to the centre found a thick layer of pre-glacial sediments overlying fractured and brecciated rock. On the basis of the drilling results, the original diameter of the crater was estimated to be about 9000 feet, substantially less than the diameter of the present lake.

The cores from the holes close to the centre of the lake showed some evidence of deformation due to the extreme pressure of the impact. Similar effects had been noted in the cores obtained at Clearwater Lake, and these deformations became a matter of intense study which led to much more positive diagnosis of impact craters.

Dence[42] gives the time of impact as indeterminate, sometime between middle Tertiary (40 million years) and late Precambrian (600 million years).

Lac Couture (60°08′N, 75°20′W)[33]

This lake lies about 156 km southwest of the New Quebec Crater, and is found on the same 8-inch topographical sheet, 35 S.E. It is roughly circular in shape, with a diameter of 8 1/2 miles. The shoreline has numerous inlets; if these are considered part of the feature its diameter is as much as 11 1/4 miles. A large number of islands appear around the circumference of the lake, but there is a large circular area free of islands. McConnell and Tanner made a depth traverse of the lake in 1959, as part of the regional gravity survey, and found a maximum depth of 387 feet, somewhat displaced from the lake centre.

An expedition to northern Quebec by an amphibian aircraft was organized in August 1963 in order to study a number of possible crater sites. The party consisted of Beals and Dence, Professor A.J. Cohen of the University of Pittsburgh and Geoffrey Charlewood, of Heath and Sherwood drilling company. Lac Couture was one of the sites visited.

They noted, as was already evident from the topographic map, that the lake was quite different in form from the surrounding long and narrow Shield lakes, and that there was no evidence of an uplifted rim. There was a well-developed fracture pattern outside the lake but this regular system did not exist inside the lake boundary. Instead, such joints as existed dipped at a variety of angles. Boulders of breccia were found at a number of points; there was not time for the party to define the extent of this brecciated debris but it appeared to have been dredged from the lake bottom by glacier action. When this brecciated material was later examined in the laboratory it was found to have undergone extensive shock and heat metamorphism. The inference of an impact origin was clear.

Carswell (58°27′N, 109°′30°W)[34]

The Carswell structure, in northern Saskatchewan a few miles south of Lake Athabasca, is a puzzling geological formation[35]. It lies in a region of flat-lying Proterozoic sandstone, known as the Athabasca Formation, and consists of high ridges of dolomitic limestone that are remnants of a formation originally lying above the regional sandstone. The ridges are connected by less elevated outcrops of the same material, forming a ring 18 miles across; the ridge has an average width of three miles. The rocks in the ridge are intensely folded, contorted and brecciated, with much overturning of the beds. Gneiss outcrops are found within the ring but do not appear outside it. Large erratics of brecciated Athabaska strata have been found within the ring.

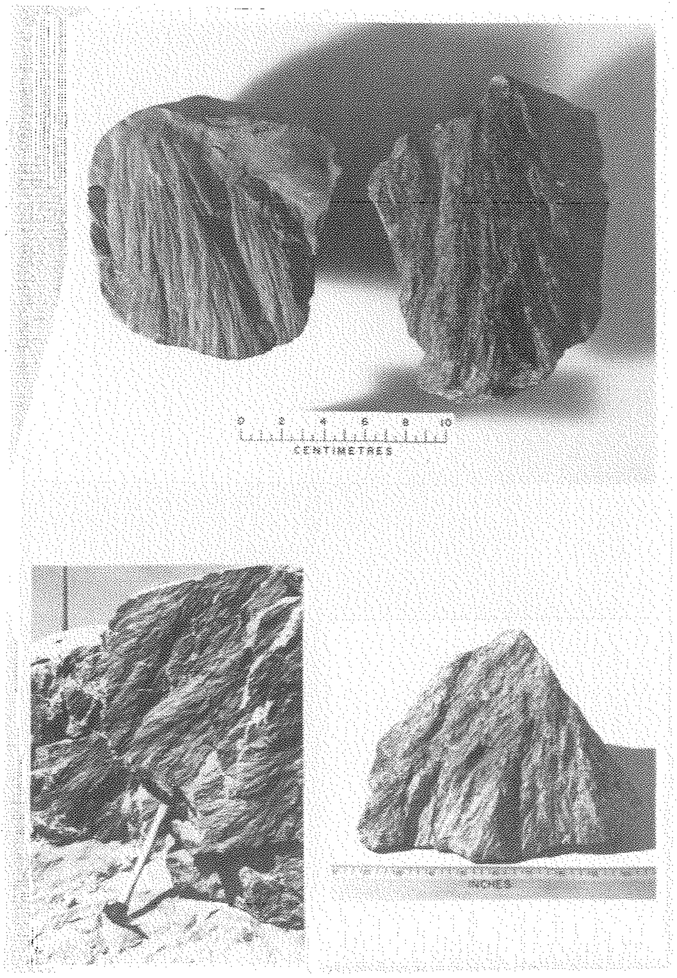
The aeromagnetic maps of the Geological Survey show an area of little variation within the circle in contrast to very contorted lines outside. A concentrated pattern of gravity observations, both inside the feature and in the surrounding area, was established as part of the regional survey during 1960 and 1963. It shows the usual negative circular anomaly, although somewhat distorted. Innes analyzed a gravity profile across the crater and concluded that it was consistent with a deeply eroded crater in which the floor has been raised by isostatic adjustment. The density value obtained for this central gneiss suggests that it has been badly fractured, which is borne out by examination; there is also evidence of shatter cones, a phenomenon to which we shall return presently.

Review of Research to 1964

Two facts have become increasingly evident in the sequence of craters we have described: there is a difference in form between the smaller and larger craters, with the large craters having a badly fractured central uplift; there is petrological evidence of profound changes in the rocks found in craters, particularly in the breccias. These findings were illuminated by great advances made during the same period in the identification of shock effects in rock. These had been discovered in the laboratory under controlled conditions of high pressure and temperature, or in material subjected to nuclear explosions. Their presence in materials found in the Barringer and other suspected craters was providing exciting evidence for their impact origin.

The shock-induced effects are both macro- and microscopic. The principal macroscopic effect is the formation of shatter cones. The impacted rock has a tendency to break into conical pieces, ranging in size from one centimetre to as much as 12 metres; the size is related to the original thickness of the formation and to the spacing of pre-existing fractures. The cones have striations parallel to the conic generator, and are almost always found with the apex pointing toward the source of the pressure. Since shatter cones were first discovered in the late 1940s they have been produced in the laboratory and have been recognized at the site of underground nuclear explosions. They have never been found in volcanic environments. Their presence is regarded as sure evidence of high and impulsive pressure[36], and their orientation as diagnostic of the direction from which that pressure came.

Even surer evidence of extreme pressures is the presence of certain minerals, not previously observed in nature, that were developed in the laboratory under conditions of extreme pressure. The first of these minerals, *coesite*, a dense, heavy form of silica, was discovered in 1953 and named after its discoverer Loring Coes. A second polymorph of silica, also formed under conditions of extreme pressure, was discovered in the USSR in 1961 and named *stishovite* after its discoverer. A third mineral, *maskelynite*, a glassy form of plagioclase feldspar which had been known for many years, was recognized at about the same time as a polymorph resulting from shock at high pressure. Exciting times followed; coesite and stishovite were found in the Barringer crater, then in a number of other suspected craters, but never in volcanoes or volcanic environments, and their presence became generally accepted as an indicator of meteoritic impact. They, and shatter cones, were found in many of the so-called cryptovolcanic structures, indicating that these were indeed the result of impact, as Dietz had been insisting for so long. How stable were they; would they be found in ancient craters? Yes; in 1961 coesite was found in one of the Holleford cores[37], proving that it could persist in a moderate pressure and temperature environment through geologic time. Maskelynite was identified in both the Clearwater West and the Manicouagan craters[38]. It too became a diagnostic tool.



Typical shatter cones.

The recognition of these minerals was only part of the contribution of petrology to the understanding of shock. Thin sections showed that high shock pressure caused changes in certain crystals, principally of quartz and the feldspars, the changes becoming increasingly pronounced with increasing pressure. The changes involve dislocations, fractures or cleavage in the crystals, sometimes with fault-like displacements, and vitrification in varying amounts.

By this time ten suspected craters, of varying size and complexity, had been studied in some detail. Arranged in order of size, they were:

NAME	DIAMETER kms
Holleford	2.35
Brent	3.0
West Hawk	3.3
New Quebec	3.7
Deep Bay	10.
Lac Couture	14.
Clearwater East	21.
Clearwater West	32.
Carswell Lake	32.
Manicouagan	60.

A rather large number of papers reviewing the Canadian program appeared in the years 1964-1966. There is a good deal of duplication in them; the crater work of the Observatory was attracting wide attention and there was pressure to present its findings to a number of different constituencies – astronomers, geologists, petrologists, high-pressure physicists. It was an effort which the Director encouraged.

In the first of the review papers Innes[34] gives new information about the rims of the Deep Bay and New Quebec craters, showing that the fracture patterns and uplift of surrounding strata are remarkably similar to those found in the Barringer Crater. A review paper by Beals and Halliday[39], presented to a General Assembly of the Royal Astronomical Society of Canada, makes two interesting points. The first concerns the frequency of craters. Noting that the frequency of craters on the moon is about 1,000 times greater than on the earth they argue that this is due to obliteration by erosion rather than any intrinsic difference in the two bodies. Their second point concerns the central uplift and the volcanic-like rocks associated with the larger craters. They are quite firm that these are not indicative of volcanic forces, estimating that if only five percent of the impact energy were to be converted to heat, it would melt more than sufficient rock to account for all the observed lavas. Beals[40] elaborates on this point in a paper prepared for the generalized audience of the New York Academy of Science.

In fairness we must speak of the work of K.L. Currie[41], of the Geological Survey of Canada, who was a stout defender of the cryptovolcanic theory of origin for the Canadian craters. Working often in complete cooperation with Observatory personnel, he produced detailed geological maps of four of the craters – New Quebec, Clearwater[2] East and West and Manicouagan. His interpretation was however, completely different. Since no meteoritic material had been found he couldn't accept the impact origin[3]. While he was not prepared to support conventional volcanic action he believed that the central intrusives and the extensive lavas within the larger craters came from within the earth; he could not accept that the lavas had been created, in situ, by the heat of an impact. He makes one interesting point, not mentioned by others: that the four craters lie close to the height of land separating the north-seeking and south-seeking drainage patterns.

When Dence[42] considered the problem of how, and why, the profiles of the craters change with increasing diameter, he established two classes, simple and complex. In defining the differences between the two classes he depended particularly on the findings at the Clearwater Lakes. Clearwater West has an elevated central section of shattered basement rock; outside of this the drill encountered first breccia, then the shattered basement. The shattered rock showed microscopic evidence of shock metamorphism.

The one hole drilled in the smaller, East, crater in 1963 showed little uplift, a layer of post-impact sediments, then the usual section of brecciated rock followed by the fractured zone. This suggested a simple crater, much like that at Brent, and in his first analysis Dence classed Clearwater West as a complex crater, Clearwater East as a simple one. The second hole drilled in Clearwater East, in 1964, showed the central uplift; the crater was shifted into the complex category and a new boundary between the two classes was sought[43]. The next smaller crater was Lac Couture, but no drilling had been done there; what about Deep Bay? Because of drilling difficulties the first hole drilled there, close to the centre of the feature, had not penetrated the post-impact sediments; the second hole, closer to the rim, had found only fractured country rock. Additional drilling was necessary.

Two holes were drilled, from the ice, during the winters of 1964-1965 and 1965-1966[44]. The hole near the centre of the crater encountered badly fractured and altered gneiss, essentially in situ, at 1390 feet. The second hole, 4000 feet closer to the rim, went from sediments directly into a mixed breccia at 1330 feet. Deep Bay was a complex crater, with a central uplift. The boundary between complex and simple craters lay somewhere below its 10 km radius.

Another result of increasing crater diameter was noted. The smaller craters fitted quite well to the Baldwin curve, but the larger ones did not lie on its lunar section. Dence[45] suggests that for each planet there is a limiting depth, depending on the value of gravity and material strength. On Earth the limit in strong rocks is about 600 m; on the Moon it is over four km.

It was not just the size and structure of the Canadian craters that were being studied. In order to apply the new petrological criteria, cores from all the Canadian craters were re-examined and a scale of intensity was established[46], having five degrees, A to E, indicated by the degree of deformation in the quartz and feldspar crystals. The features described were attributed to shock loading at pressures between 40,000 and 250,000 atmospheres. The conclusion: since hypervelocity impact is the only mechanism known to generate such shocks, a meteoritic origin was indicated for all the features.

A number of visitors came to the Observatory, attracted by the opportunity to study the interesting suites of cores. Dr. N.M. Short studied the West Hawk cores, Dr. W.I. Manton, a South African authority on shatter cones, visited Manicouagan, and Professor Wolf von Engelhardt spent three months in Ottawa as a visiting scientist during the summer of 1965. He worked largely on the West Clearwater Lake cores[47]. His visit was reciprocated by Dence, who spent the winter of 1972-1973 in Tubingen and continued a joint study into the West Clearwater Lake cores[48].

Dence[49] gave Brent and Clearwater East special study as representative simple and complex craters. The degrees of shock shown in the Brent cores, particularly in those from the base of the breccia, showed such complexity that a subdivision into six zones was necessary. The correlation between the two scales is about as follows:

Zone Number	Class
i	No equivalent, less shocked than class A
ii	A grading slightly into B
iii	Class B only
iv	Class C, grading slightly into D
v	Class D
vi	Class D, grading into a melt zone
Melt zone	Melt zone

The figure on p. 105 shows, on the left, the general distribution of the various degrees of shock in the Brent Crater and, on the right, the detailed variation over a 500 foot section at the base of the crater. Notice that the igneous-type rock, originally identified as a lava sill, has now been recognized as due to the melting and recrystallization of rock at the point of maximum impact pressure.

Having established these zones for Brent, Dence considers the cores from the central uplift zone of East Clearwater Lake. He finds only two of the zones, ii and iii, represented; these correspond closely to the zones found beneath the crater in Brent. In this case the zones of stronger shock have been eroded from the central raised boss.

The mechanism of crater formation was becoming better understood. The following description[22] is worth quoting.

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[2] The original maps of this area were prepared by H. Bostock.

[3] Ironically, it was Currie who found the initial fragment of shocked rock at the New Quebec Crater, the best proof of its impact origin.

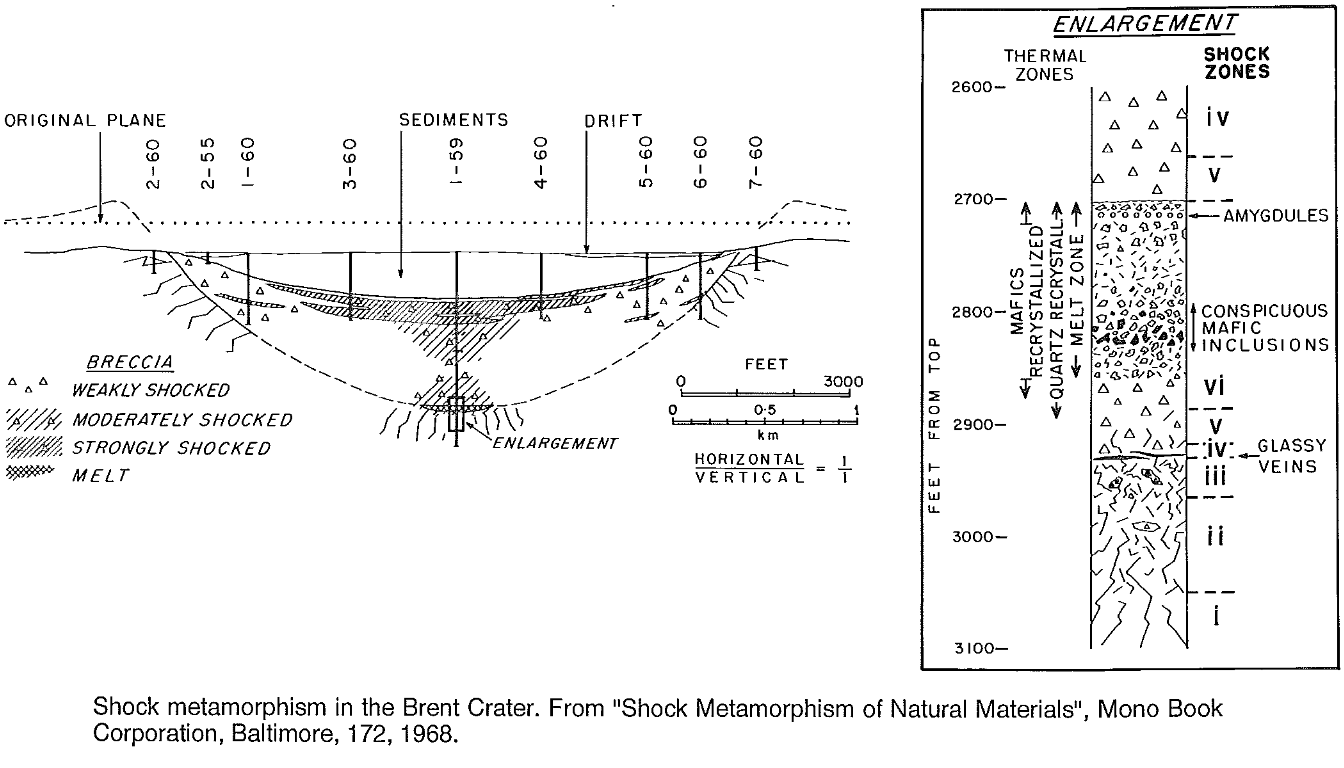
"The sequence of events following the collision of a large meteorite with the earth has been worked out by a number of investigators and may briefly be outlined as follows:

The mass of air displaced by a meteorite of thousands or millions of tons will be insufficient to produce appreciable retardation during its passage through the atmosphere, and it will reach the surface of the earth with little loss of velocity. Upon striking the solid earth it will suffer a very rapid exponential deceleration, coming to rest a few meteorite diameters below the earth's surface. If the velocity of impact is, say, 20 km/sec, the peak pressure generated will be of the order of 10 megabars, comparable to that at the centre of the earth. Such a high instantaneous pressure, which will be shared by the meteorite itself as well as considerable volume of rock into which it imbeds itself, will have two principal effects.

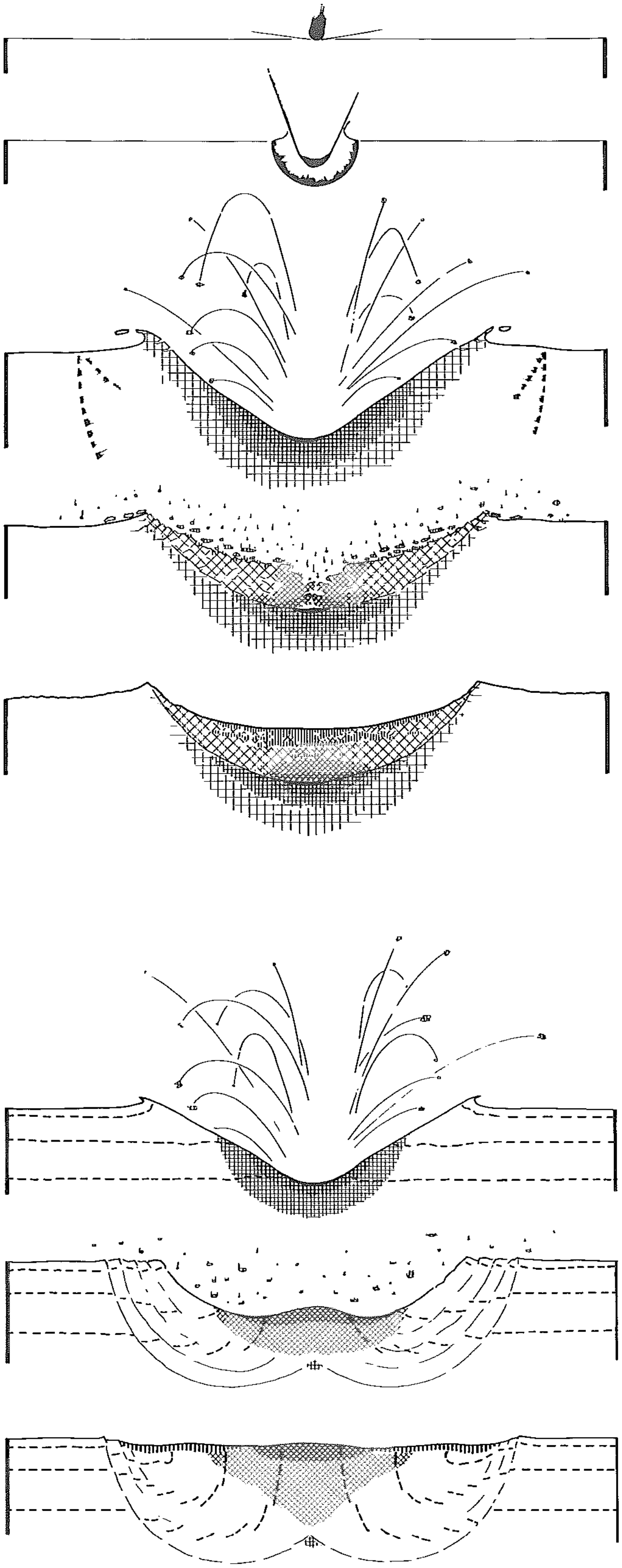
First, a pressure of this magnitude will give rise to a shockwave which will vaporize a certain proportion of the material involved, melt a somewhat larger proportion and shatter large volumes of rock extending to distances of the order of 50 times the diameter of the impacting meteorite. Secondly, as the meteorite comes to rest, the mixture of highly compressed rock and meteorite fragments will receive an upward and outward acceleration mainly from the decompression of shocked material. The effect will be comparable to that of a powerful explosive, producing the external aspect of a crater by ejecting large quantities of debris and uplifting the surrounding strata to form the familiar depression surrounded by the raised rim typical of a meteorite crater."

In the figure on p. 104, Dence suggests how the two classes of craters are formed[49]. The simple crater is illustrated in the upper figure. The first three profiles illustrate the process described above: in the first profile an iron meteorite strikes a massive granitic rock; in the second it has entered the granite and is completely shocked and partly melted. It is being incorporated, along with fused rock, into the cone of ejecta formed by the interaction of the shock front with the free surface. At this point most of the rock behind the expanding shock front will have been fused. The third profile shows the completion of the primary crater or ejecta void. The shock wave and its reflected wave have attenuated into seismic waves. Uplift and overturning of the rim rocks are indicated. Three zones of progressively weaker shock are shown by cross-hatched patterns under the ejecta void. They correspond approximately to the zones of moderate shock, weak shock and brittle fracturing observed at the Brent crater. At the bottom of the void there is a small amount of fused rock and meteorite fragments, indicated in black. The next profile shows the filling of the crater, by slumping and fallback of ejecta. Diagonal patterns are used to indicate the material which moves to the centre of the crater; vertical lines indicate fallback, mostly of fused, highly shocked material. Together these two types of materials form the breccias. The bottom diagram shows the final distribution of shocked material in the simple crater.

The complex crater is illustrated in the lower diagram, which picks up the story at the third stage, the formation of the complete ejecta void. The zones of moderate and weak shock features are shown, and three marker beds indicate an initial horizontal stratigraphy. The middle profile depicts a stage in the formation of a central uplift by deep sliding along slip surfaces. In well-bedded rocks the movement will be largely along bedding planes. At this stage, much of the highly



Shock metamorphism in the Brent Crater. From "Shock Metamorphism of Natural Materials", Mono Book Corporation, Baltimore, 172, 1968.



The formation of simple (above) and complex (below) craters. From "Shock Metamorphism of Natural Materials", Mono Book Corporation, Baltimore, 179-182, 1968.

shocked and fused rocks are still airborne. The lower profile shows the final crater. Moderately shocked rocks form the top of the central uplift, and are surrounded by an annulus composed mainly of highly shocked and melted fallback breccia. In large craters, the melt forms a continuous sheet of igneous rocks, with many inclusions of less strongly shocked fragments at the base and top.

Dence concludes that the rocks within a simple crater have the following properties:

1. Low density.

2. Low, uniform, magnetic intensity.

3. Fracturing, alteration and deformation of the regional basement rock beneath the crater, intensifying toward the centre into a coarse breccia.

4. Overlying this coarse breccia, a finer, better mixed and partly vitrified breccia; in the larger craters, such as Clearwater East and West, where the heat of impact was greater, this layer has coalesced and largely recrystallized into a lava-like matrix.

5. Unrecrystallized fragments in all the breccias show signs of shock metamorphism.

In the complex craters these characteristics are modified by the central uplift but are found in the annulus surrounding the uplift.

How is the central peak formed? The degree of metamorphism in the peak rocks is low to moderate, relative to that in the brecciated ring. As suggested in the description above, Dence believes that the peak is formed by material slipping into the crater under a differential hydrostatic pressure between the rim and the crater floor. This pressure would increase with the crater diameter, and for any medium there would be a limiting crater size above which the slip would take place.

In the light of this new understanding, Manicouagan was once more regarded as a probable crater. The central anorthosite boss, instead of being an intrusive body, was an upwelling of the basement rocks, and the supposed lavas discovered in the crater were in fact the product of shock melting. W.A. Robertson[50] measured the remnant magnetism in these lavas and concluded that it was acquired during the Triassic, some 220 million years ago.

THE THIRD PERIOD - 1964-1970

The Nastapoka Arc

Something more should be said about the Nastapoka Arc, which takes a great bite out of the east coast of Hudson Bay. From the beginning of the Observatory's interest in craters this feature had been suspected of having a meteoritic origin and Beals[51] had compiled the evidence, largely geological and topographical. The arc is closely circular, with a diameter of 300 miles. Its shoreline differs from other sections of Hudson Bay in that it consists of hills rising several hundred feet above the shore, considerably higher than the surrounding Shield. There is geological evidence that it embraces a basin nearly 30,000 feet deep filled with sedimentary and metamorphic rocks with interbedded volcanics. The strata around its boundary all dip toward its centre. In a companion paper[52] Halliday supports the impact interpretation, by comparing the feature with ones of comparable size on the Moon and on Mars, and with established terrestrial craters. He shows the probable profile of a 300-mile crater; its depth is less than the 30,000 feet suggested for the Hudson Bay arc, which could be explained as due to depression of the earth under the weight of sediments. He makes an interesting estimate: to produce a crater 300 miles in diameter would require a meteorite, if iron, 21 miles in diameter, with a volume of 4,000 cubic miles.

A hole was drilled on Neilson Island, within the arc, in 1962. The core lacked any evidence of shock metamorphism[53], but one hole was not regarded as sufficient to settle the question of possible meteoric origin.

Pilot Lake (60°17′N, 111°01′W[54])

Pilot Lake, in the District of Mackenzie, Northwest Territories, is a rather square lake with a number of islands near its shores, but none within an inscribed circle of 3.6 mile diameter. The shape of the lake, and the drainage in and out of it, are quite unlike that of the surrounding lakes, which are long and narrow, their direction controlled by regional jointing or by glaciation. Dence and Innes visited the lake in 1965; they measured its depth, examined lakeshore outcrops and searched for unusual glacial erratics that might have been dredged up from the lake bottom.

They found that the water depth was uniform, and substantially less than at West Hawk Lake, Deep Bay or East Clearwater Lake. Lakeshore outcrops gave no indication of unusual deformation. However a number of samples of the glacial debris contained deformed gneiss or breccia and exhibited extensive fracturing. Petrological examination of these materials showed that they were derived from the country rock and that they showed shock effects of all types from A to D. It was concluded that Pilot Lake was probably of impact origin.

Nicholson Lake (62°40′N, 102°41′W[55])

This feature is also in the District of Mackenzie. It is not noticeably circular, but the association of Paleozoic limestones with fragmented and volcanic-like rocks, reported by several geologists, drew attention to it. Dence and Innes visited it in 1965, while on the way to Pilot Lake. A gravity survey was made of the area and a careful examination and sampling of outcrops was conducted. The gravity survey was extended by Hornal, in 1965 and 1966, as part of the regional survey. The anomaly pattern is the standard one of circular contours centred on the lake, the total negative anomaly being about 6 mGals.

There are islands in the lake, including a large one close to its centre, and an anvil-shaped promontory extends into the lake from the western side. The party found breccia, of various degrees of consolidation, made up of the basement gneiss and of fragments of an Ordovician limestone that had originally overlain the basement. The basement gneiss also showed evidence of fracturing and numbers of the diagnostic shatter cones were found. In the laboratory the breccias showed evidence of A, B and D shock characteristics. It was concluded that Nicholson Lake was of meteoritic impact origin.

Because of the age of the rock making up the breccias, both the Pilot Lake and Nicholson Lake craters are believed to date from the middle or late Paleozoic.

Charlevoix (La Malbaie) (47°32′N, 70°18′W)

In 1966, Jehan Rondot, of the Quebec Department of Natural Resources, was doing routine mapping of the Charlevoix region[56], which lies on the north shore of the St. Lawrence river midway between Baie-St.-Paul and La Malbaie. He found shatter cones but, having no knowledge of shatter cones, or of impact structures, he called them cat's-claw fractures. Because they were unusual, he took some samples back to his office. They were there seen by John Murtaugh, an American graduate student working for the Department of Natural Resources. He recognized what they were, having seen them at Manicouagan. A few days later, on a visit to Ottawa, he told P.B. Robertson about them. Robertson examined a topographic map of the area and recognized an arcuate structure. He visited the region, found more shatter cones and impact breccia, and confirmed the existence of an impact structure.

Only about half the circle is visible, the lower half being cut off by the river and by Logan's Line. The special interest in this feature is that it is centred on one of the most seismic areas in Canada. After its discovery the feature was investigated independently by Rondot[57] and by Robertson[58]. Their two papers provide much the same information.

The visible part of the feature is marked by a broad valley in which two rivers, the Gouffre and the Malbaie, flow. This valley, 32 km in diameter, defines an annular graben in which remnants of an original covering of Ordovician limestone are preserved; the fact that there is no trace of this formation outside the feature suggests a down-throw on the graben of some 780 m or more. Inward from this valley the topography rises abruptly to a broad plateau 300 m above the valley floor, and from this to a ring of hills with an average elevation of 460 m. Then follow a broad inner valley, and finally a central peak, Mont des Eboulements, with an elevation of 780 m.

Both authors report shatter cones within the crater, the intensity of the development having a roughly concentric pattern about the centre, as shown in the figure on the following page. The best developed cones are found at a mean distance of about 7 km from the centre. There is a broad distribution of breccia and the microscopic evidence is for class C and D deformation at the centre, grading to A deformation at a distance of some 5 km from the centre. Both authors conclude that the feature is a fossil meteorite crater and Robertson suggests an origin in the early Paleozoic, Middle Ordovician to late Devonian, 460-365 million years ago. Rondot reports the dating of two samples from within the crater by R. Doig of McGill University as 335-372 my; this would favour the late Devonian impact.

This crater has a special interest because the 1925 St. Lawrence earthquake, magnitude 7.0, was located within its boundaries. Studies beginning in 1970 showed continuing microearthquake activity, the foci closely defining the limits of the crater. The details wll be given in a later chapter.

Mistastin Lake (55°53′N, 63°18′W)

Commercial geologists reported to the Geological Survey the presence of a small area of flat-lying igneous rocks in an area of Labrador where no other such formations exist. The area is at the west end of Lake Mistastin, a roughly circular lake 13 to 16 km across, with a large arcuate island and a number of smaller islands close to its centre. The similarity to Clearwater Lake suggested that the lake might be of meteoritic origin, and F.C. Taylor of the Survey visited the area to make a preliminary examination.

He found[59] that the lake surface is 150 m lower than the mean regional elevation and that, while there is no crater-like rim, the lake is enclosed in a discontinuous ring of low hills with a diameter of 26 km. The only prominences within this ring of hills are the central island and a butte-like hill, just west of the lake on which the flat-lying igneous rocks were exposed. Taylor sampled these lavas, noting inclusions in them of country rock and mineral fragments ranging up to the size of boulders. He also collected samples from a number of locations on the central island.

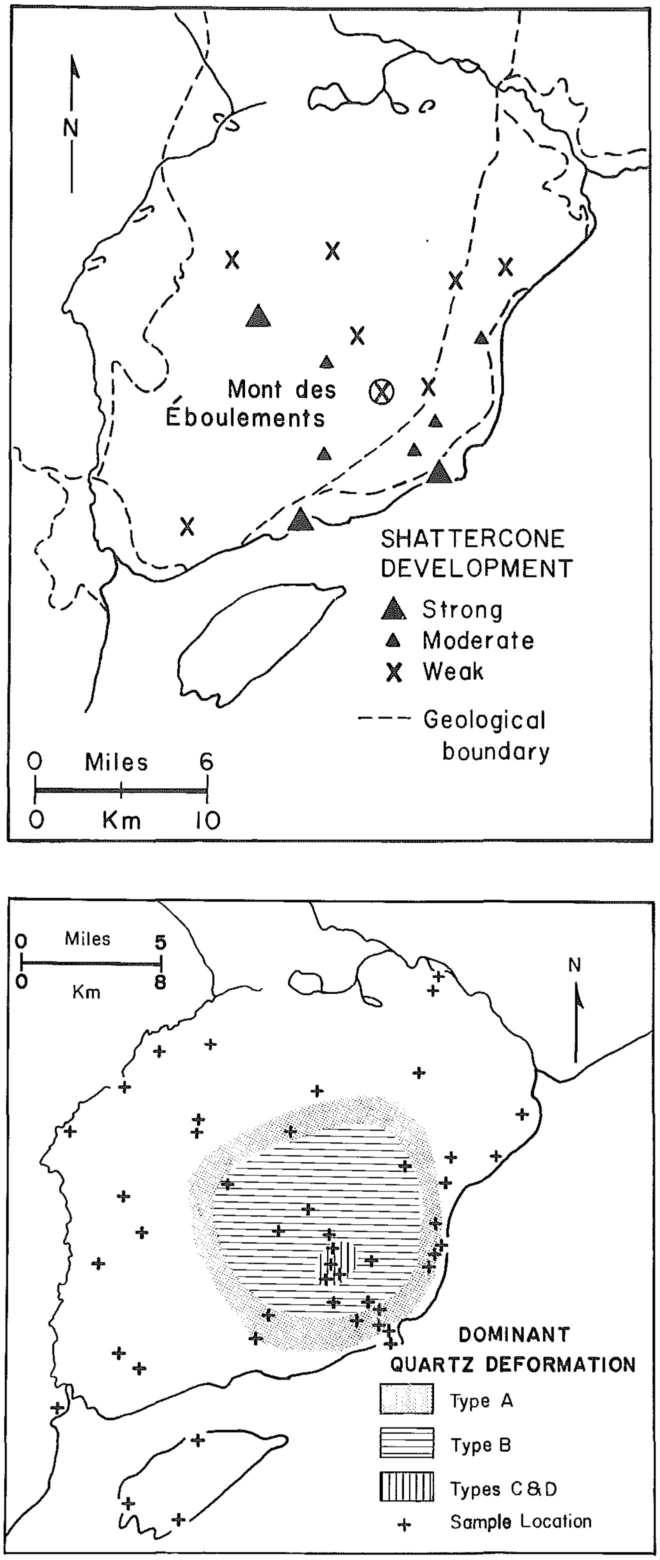
These samples were studied petrologically by Dence. On the central island, and on the western shore of the lake, there was abundant evidence of class A and B deformation, while the inclusions in the lavas showed all degrees of deformation from B to D. It was concluded that the feature was of meteoritic origin. Potassium-Argon dating[60] found an age of 202 ± 25 my, suggesting that the impact had occurred in the late Triassic.

The Sudbury Basin and Lake Wanapitei

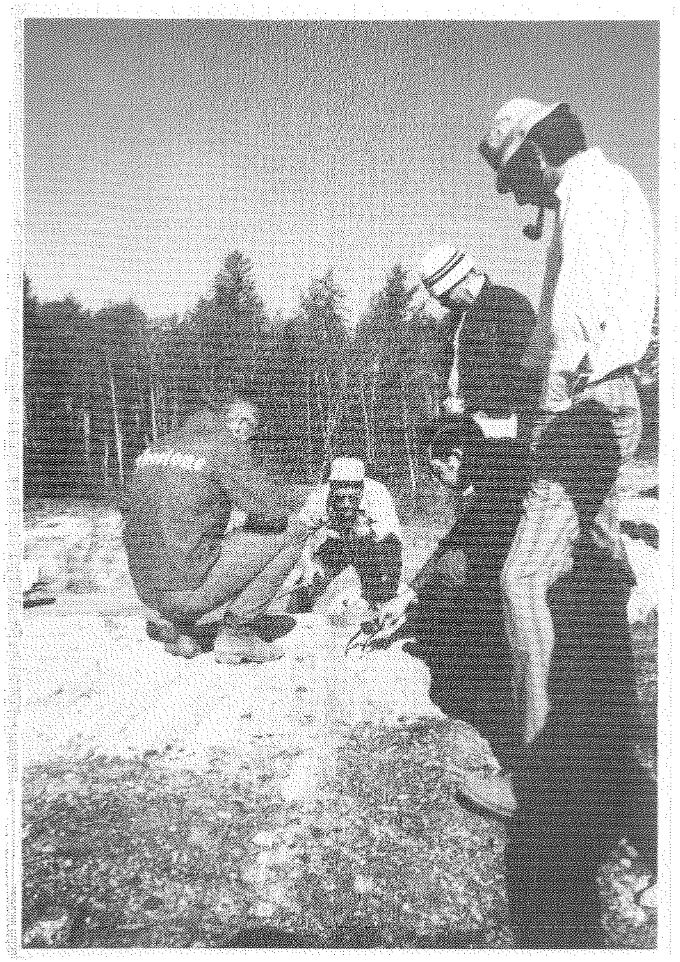
The Sudbury Basin has been called the Gordian Knot of Canadian geology. It has been studied for nearly a century, mining has made its structure at depth known, and yet the processes that initially formed it, and the sequence of events which subsequently deformed it, are still imperfectly known and hotly debated. In the mid-sixties the theory of an impact origin was advanced by Dietz[61], by Guy-Bray[62] and by French[63]; Dence[64] has reviewed their work and provided additional support for the theory.

The basin has few of the identifying features we have come to depend on; it is not crater-shaped, there is no rim and no uplifted or overturned rim rocks, there is not a circular negative gravity anomaly, there is no central uplift. It does however have a great variety of breccia and fragments of this breccia, and of the country rock, exhibit the characteristics of shock metamorphism up to Class B. Shatter cones and Class A shock metamorphism exist in the basement crystalline rocks underlying the basin.

Dence proposes the impact of a large stony meteorite, but he questions that its impact would produce enough heat to account for all the melted rock observed. He therefore



Distribution of shatter cones and quartz deformation throughout the Charlevoix crater. From Meteoritics, 4, 98-102, 1968.



Michael Dence uses his glasses to point out a feature of the Levack complex to G. Cernan (in the Firestone jacket) and Harrison "Jack" Schmidt (facing camera) of the Apollo 17 crew. In addition to his other credentials, Schmidt holds a PhD in Geology. The Levack complex, part of the footwall of the Sudbury structure, shows weak shock metamorphism.

suggests that impact occurred at a time when the region was tectonically active, so that heat from below helped produce the additional melting observed. This would also have increased the depth of melting and allowed for a deeper-than-normal crater and for more drastic movement of the sub-crater rocks. The theory accounts for the blanketing layer of breccia-rich formations but does not provide for sufficient melt material to explain the ore-bearing Sudbury Irruptive.

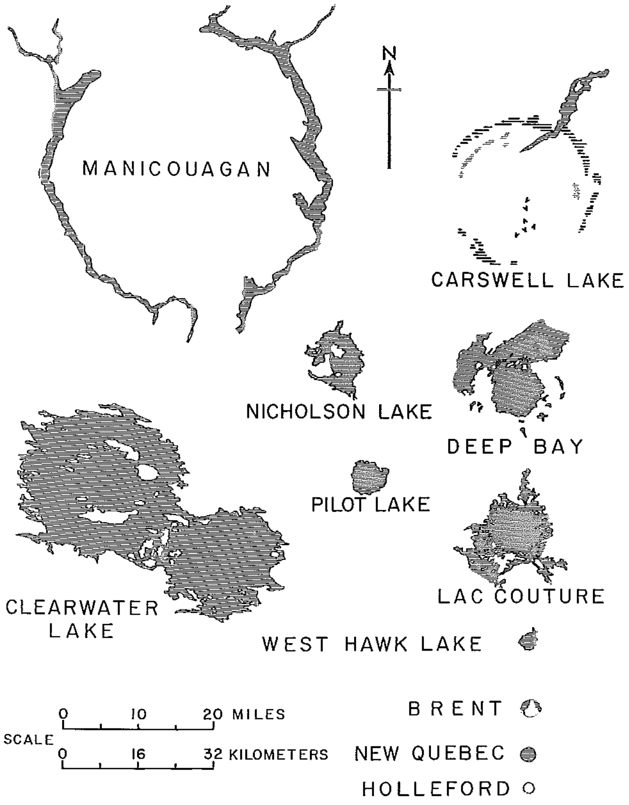
Dence and J. Popelar have presented evidence for an impact origin of Lake Wanapitei (46°45′N, 80°45′W), a lake abutting on the eastern end of the Sudbury Basin[65]. The lake has only a partially circular shape, but the gravity map[66] shows the usual diagnostic circular contours and negative anomaly. The airborne magnetometer shows the suspected crater to be magnetically featureless. Typical crater breccias have been found, apparently dredged from the lake bottom by glacial action, and the fragments of which they are composed show the microscopic effects of shock metamorphism and the presence of coesite. There can be little doubt of the impact origin of the feature.

The close association of the Wanapitei crater with Sudbury raises the question of whether they were formed at the same time by a double meteorite. The geological evidence is against this. The Wanapitei crater is late Eocene or early Oligocene in age, some 1 1/2 billion years younger than the Sudbury structure.

In 1971 Dence led Astronauts John Young and Charles Duke of the Apollo 16 crew over the terrain adjacent to Wanapitei and Sudbury and taught them how to recognize the breccia and shatter cones associated with impact craters. The next year he repeated the instruction for G. Cernan and Harrison "Jack" Schmidt of the Apollo 17 crew. Dence's contribution was recognized by a Public Service Award.

Review of Research to 1970

Beals retired from his position as Dominion Astronomer in 1964, but his interest in the meteorite studies continued unabated. One of the problems that interested him particularly was the relative infrequency of terrestrial as compared to lunar craters. Two explanations suggested themselves. The first was that the greatest bombardment of meteorites occurred early in geological time when the Moon's surface had already



Sketch map showing the Canadian craters, drawn to relative size.

consolidated whereas Earth's surface was still molten or partly molten. He was led to this proposal by his examination of a circular feature made by islands in the Lake of the Woods[67]. These islands proved to be ridges of granite gneiss suggesting a circle 16 km in diameter. Beals speculated that they might have been formed when magma was close to the surface and that the magma rose to fill fractures produced by a meteorite impact.

Alternatively the paucity of terrestrial craters may be due to their obliteration by sedimentation. Assisted by A. Hitchen, Beals[68] set up a scale-model experiment to investigate this matter. A brass casting 12.7 cm in diameter was formed to conform to the shape of a crater, the profile being selected on the evidence of the Barringer and New Quebec craters. This model was set in a waterproof box and covered with water to a depth that would correspond to a deep ocean. Both mechanical and chemical deposition were used, and in both cases the crater was obliterated by a sedimentary deposition equivalent to 2400 feet. Beals argued from this that, for example, any craters beneath the prairies would be quite invisible and suggested that, eventually, some craters might be found by drilling. In a note added in proof he draws attention to one such possible crater.

There are two review papers by Dence that coincide reasonably well with the close of the Observatories Branch, the end of our period of interest. In the first of these[69] he reviews the chemistry, form and distribution of melt rocks in 40 terrestrial structures in which they had been found in connection with rocks showing shock deformation. In general, the chemistry of the melt rocks can be explained as deriving from the source rocks; no contributions are required from deep magmatic sources, but in a few melts there is the suggestion of additions from the meteorite itself. The form and distribution of the material is always, as we have seen, compatible with an impact formation.

In a paper read before the International Geological Congress[70] Dence tabulates all the world's known craters, that is, ones in which meteoritic materials have been found; there are 12 of these. He next tabulates all "probable" impact craters, in which there is evidence of shock metamorphism. There are 42 of these. He analyses them by location, by type (simple or complex) and by age. There are in addition more than 40 "possible" impact craters.

Employing the Canadian data he calculates that the rate of cratering for the Canadian Shield is one crater 20 km or more in diameter per 10^6 sq. km, and per 10^9 years; the rate is five times this in the Quebec-Labrador area alone. He also points out that, where the craters have been dated, it is possible to measure the rate of erosion. For Canadian craters this varies from 0.5 to 3 m per million years. The rate is much lower at the Clearwater Lakes. He concludes that the Canadian Shield has, indeed, been stable for a long time and that erosion rates there are much lower than in adjacent areas of the United States.

The paper makes a statement that we may use to conclude this chapter.

> "The persistent efforts of a small number of scientists over the last twenty years have transformed ideas about interactions between the earth and minor bodies in the solar system. Collisions are now known to have been more numerous and on a larger scale than had been considered by all but a few scientists. It is now apparent that the importance many astronomers and planetologists have given to impact in forming the surface of the Moon, Mars and other terrestrial planets is not incompatible with the geological record on Earth."

We may take great satisfaction in knowing that several of that small number of scientists were our scientists, and that their work is known and respected throughout the world.

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Dr. M.J.S. Innis, Chief of the Gravity Division, 1952-1972.

XI – GRAVITY

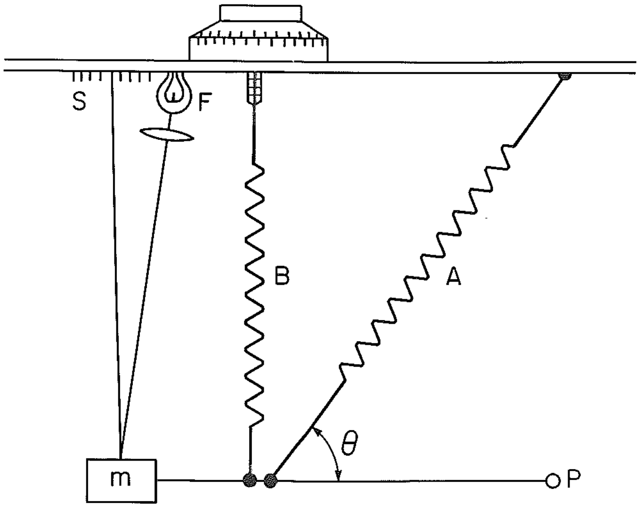
INTRODUCTION

During the period from 1946 to 1970 knowledge of the gravity field in Canada expanded a thousand-fold. Credit for this belongs primarily to M.J.S. Innes, who was Chief of the Gravity Division from 1952 until his retirement in 1971. He was indefatigable in expanding the gravity survey of the country, and he assembled and inspired a staff whose enthusiasm and desire for excellence set a standard recognized nationally and internationally.

I said in an earlier chapter that the Beals administration was characterized by advances in instrumentation, the Hodgson administration by advances in computers. This is nowhere more evident than in the gravity work. The tremendous expansion was made possible by the development of the gravity meter, or gravimeter, and by its continuing improvement in stability, range and sensitivity. Commercial applications of the instrument drove most of this development, but the Observatory's needs were pressed on the manufacturers, and led to important improvements. As the rate of data acquisition increased, the need for improved methods of data reduction, storage and interpretation was met by the development of computers of constantly increasing speed and capacity.

Gravimeters are relative measuring devices, portable, and capable of making an observation in a few minutes. We may think of them as spring balances except that, to provide sensitivity, some trick must be used so that a small change in the value of gravity will provide a large movement in the sensing mass. Such a spring system is said to be astatized.

One way in which astatization is accomplished is illustrated in the figure below. The mass m is carried at the end of a beam, pivoted at P, and is held in position by the spring A. This spring is wound in such a away that its tension, or restoring force, is proportional to its length, rather than to its extension. If there were no tension the length would, theoretically, be zero, and for this reason it is known as a "zero-length" spring. As the mass moves in response to changing gravity the length of the spring, and therefore the restoring force, changes only very slightly and the movement of the mass is considerable. The beam is viewed through a microscope and is returned to a standard reference point on a scale in the eyepiece by means of a second spring, B, operated by a calibrated screw. Since B is not supporting the mass, it can be very weak, and a substantial change in the screw position will be needed to return the mass to its null position.



Differences in gravity are measured in terms of "dial units" of this screw. The conversion to gravity units is made using a scale constant. This constant was supplied by the manufacturer, but it became evident early in the history of gravimeter work that these constants were unreliable. The search for standardization became a constant preoccupation.

Gravimeters may have metallic or quartz components. The former are very sensitive to temperature variations and require temperature control units; the latter have a built-in temperature compensating device and are therefore more portable. The instruments with metallic components have proved to be the more reliable.

The standard unit of gravity, the Gal, so-named to honour Galileo, is too large for field work. Here the unit is the milliGal (mGal), defined by the relation

$$1 \text{ milliGal} = 0.001 \text{ Gal} = 10^{-5} \text{ m/s}$$

Since the earth's gravity field is approximately 10 m/s, a Mgal is one part in 10^6 of the earth's field. For some purposes an accuracy of a tenth, or even a few hundredths of a milliGal was sought.

Early gravimeters had a range of a few hundred milliGals. This was quite adequate for local surveys and for most commercial work, but not for surveys intended to cover the latitude range of Canada. The difference between gravity at the pole and at Ottawa, due simply to the difference in latitude, is about 2600 mGal. "Long-range" gravimeters were developed in the early 1950s, and their range was gradually expanded, but a major improvement was made in 1960 with the arrival of the LaCoste-Romberg gravimeter. It had a world-wide range without resetting, was very rugged, and experienced little drift. Although it was constructed of metallic components and therefore required a battery-operated temperature control system, advanced construction techniques provided a miniaturized version of the instrument that was highly portable, extremely rugged and adaptable to the Canadian environment. In addition to the regular land version, models for measurements on the sea bottom, on ships, in aircraft and in boreholes were eventually developed and used in Canada.

A symbiotic relationship between those participating in the Canadian gravity mapping program and the LaCoste-Romberg company has proved important and beneficial to both. Research and testing by Canadian scientists has on many occasions pointed the way for improvements in the instrument. Examples include the development of a cold weather version of the standard land meter capable of operating, by the turn of a screw, either on land or on the sea ice over the Arctic continental shelf and ocean basin, the conversion of the sea gravimeter for operation on a stabilized platform, and the development of the so-called linear air-sea gravimeter. The cold weather version was so versatile that it has become the standard for land gravity measurements in the Canadian program. The effectiveness of the relationship is evidenced by the appointment in 1986 of H.D. Valliant, a leader in the Observatory's design team, as head of the Lacoste-Romberg gravimeter development group.

Early gravimeters. From the left: Atlas, Worden, North American.

Peter Winter observing with a Lacoste-Romberg gravimeter on Baffin Island.

The Reduction of Gravity Observations

The value of gravity on the earth varies with the latitude and elevation of the point of observation; if the value is to be useful for any geophysical study these effects must be removed from the raw observation. They can be calculated simply, but how does one correct for the material on which the observation is made? This brings us to the question of gravity anomalies, and of how gravity measurements may be expressed so that they can be compared and interpreted in terms of geophysical properties.

One way to do it is to suppose that the observing point is isolated in the air at the appropriate elevation above a spheroidal earth stripped to sea level. The value of gravity at sea level is calculated for the latitude of the station, the effect of the increased distance of the observing point above the centre of the earth is computed, and the sum is the calculated value of gravity at the point. The difference, observed minus calculated, is called the *free-air anomaly*. An alternative method is to suppose that the station is placed on an infinite uniform disk of material, with a density equal to the density of the rocks at the point of observation and a thickness equal to the elevation of the station above sea level. The *Bouguer* anomaly is the difference between the value observed and that so calculated.

It was noted in the survey of India in the 1860s that mountainous regions exerted a smaller effect in intensity and direction than would be predicted if they were simply uncompensated loads on the earth's surface. J.H. Pratt suggested that there was some level of compensation within the earth at which all crustal columns of unit area weigh the same. If there are mountains or dense geologic layers placing an extra load on the surface, this is compensated for by lighter materials below; if there are deficiencies, as in ocean deeps, these are compensated for by heavier material. Under an alternative theory, developed by G.B. Airy, the compensation is accomplished by variations in the thickness, rather than in the density, of the crust, so that the crust thickens to provide "roots" under mountains and thins under ocean deeps. These differing hypotheses are known as the Pratt and Airy theories of isostasy.

The Pratt theory was thoroughly tested in the United States by J.F. Hayford and W. Bowie of the Coast and Geodetic Survey. They found the most probable level of compensation to be 96 km. As we saw in Part I, A.H. Miller was able to determine a level of compensation in both the Mackenzie Basin, and in western Canada, to the considerable satisfaction of Bowie.

Whichever method of reduction is used, corrections must be made for the effects of topography. Hayford and Bowie developed a chart that divides the whole surface of the earth into 33 zones, with the observing station at the centre of the chart. The mean elevation in each zone is calculated, and tables give the total correction, for topography and isostasy, of each zone, the sum to be applied to sea-level gravity.

Similar charts have been developed for determining the Airy anomaly and for correcting for topographic effects in the computation of free-air and Bouguer anomalies.

As the Bouguer anomaly is tied to the known density at the point of observation, a Bouguer anomaly map is an expression of density variations within the earth and of any departures from the assumptions made in calculating it. It is therefore universally used in geologically-related mapping.

Gravity observation may be used for many purposes, from the study of local structures to the analysis of broad geologic provinces, to the definition of the shape of the earth. All require a suitable network of gravity observations, consistent with each other and tied to a national grid of absolute stations. The provision of this network is the primary responsibility of the National Gravity Program, to which we shall now turn our attention.

Early Gravimeter Surveys

The era of the gravimeter had already begun before the close of the Stewart Administration. In 1944 Miller used a gravimeter in Nova Scotia; the instrument had been lent by the Humble Oil Company through the American Geophysical Union. In 1945 the same instrument was used in Ontario, Quebec and the Maritime Provinces. During the summers of 1946 and 1947, using a Mott-Smith gravimeter purchased from the Atlas Corporation, Miller completed the work in the mining areas of Northern Ontario[1] and, made a traverse across Canada, from Amherst, Nova Scotia to Jasper, Alberta. Also in 1947 Innes, using a rented North American gravimeter, made a number of traverses along railway lines in northern Ontario and Manitoba and made the first survey with a fixed-wing aircraft, covering 150,000 square miles in northern Ontario, Manitoba and Saskatchewan[2]. These were important beginnings; despite the primitive operating conditions, the problems experienced and the procedures developed during these formative years were the basis for the vast expansion to come.

The surveys, by air and by road, continued until, by the end of the 1951 season, a total of 10,000 gravimeter observations had been accumulated, of which 1500 were in the Canadian Shield. The intervals between the stations varied from 30 to 50 miles. When the results were compiled it became apparent that there were discrepancies between the work of successive seasons and between the work done with different gravimeters. It was believed that the scale constants of the various gravimeters, as supplied by their manufacturers, were not uniformly accurate, and that they probably changed in some unknown fashion with time. A program, which continues to this day, was initiated to establish gravity standards to provide scale control for gravimeters. This proved to be a complex and exciting process; it led to an equivalent international program, which itself became a global learning process in the performance of gravimeters.

THE ESTABLISHMENT OF CONTROLS

Two problems had to be met – to provide a range for the calibration of gravimeters, and to establish a nation-wide network of control stations to which regional surveys could be tied.

Calibration Lines

Gravimeters drift, with changes in temperature or atmospheric pressure, with vibration during transport, and perhaps with ageing of the spring. Drift can be estimated by repeated observations at a reference station. These reference stations must themselves be established by a similar, tightly controlled, process. In the early days the method used was called *base looping*. Suppose one wishes to establish a series of reference stations ABCD. One measure at A, then at B, then at A again, then at B; then the process is continued in the sequence AB, BA, AB, BC, CB, BC, CD, DC, and so on. The base stations may be some distance apart, and observations may be made at intermediate stations during the looping, but each loop must be completed within about an hour to permit a reliable estimation of drift.

By the beginning of the 1950 field season five gravimeters were available in Ottawa. Using the base-looping technique and averaging the values for all five instruments, S. Saxov, a UNESCO Fellow from Copenhagen, established a gravimeter calibration line between Prescott and Maniwaki[3]. This was the first such line in Canada. In 1954 it was extended to Senneterre. Then, in 1954 and 1955 it was extended southward to Washington. Three gravimeters were employed in the work and the stations were kept close enough to each other that, by repeated looping, the drift of each instrument could be reliably defined. This range became a basic standardization one for Canadian work[4].

When the LaCoste-Romberg long-range gravimeters became available in the early 1960s a more extended calibration range was called for. P.J. Winter attempted to establish such a line in 1964. Travelling in an Air Force Hercules aircraft he tied Resolute to Ottawa and attempted to establish bases at a number of intermediate points. Large drifts were encountered. A.C. Hamilton and B.G. Brulé showed that these were due to aircraft vibration. They made a detailed study of such effects, using a vibrating platform[5]. Vibration isolators were developed for the gravimeter carrying cases.

Pendulum Measurements

Establishment of a second calibration line began in 1952. At the 1951 meetings of the IUGG it had been suggested that a long-range calibration line be set up in North America. Cambridge University agreed to lend Canada pendulums that it had developed and tested in field work. The instruments arrived late in 1951. G.D. Garland, who had recently left the Observatory staff for the University of Toronto, tested and modified the instruments slightly during the winter. In 1952 he established pendulum stations at Mexico City and Monterey (Mexico), Houston (Texas), Tulsa (Oklahoma), Beloit (Kansas), Huron (South Dakota), Madison (Wisconsin), and Winnipeg. Ties were made to both Ottawa and Washington[6] at the beginning and end of the season. During the 1953 field season the line was extended to the north, ten stations being established between Lethbridge and Fairbanks. The lateral displacement of the line, from Winnipeg to Lethbridge, was necessary to permit use of the Alaska Highway[7].

The line was also observed by officers of the US Coast and Geodetic Survey, using their own invar pendulums, and by a team from the University of Wisconsin, using quartz pendulums owned by the Gulf Oil Company. This duplication permitted a comparison of these three important instruments. The internal consistency of the results with the Cambridge pendulums, and their comparison with the Gulf quartz pendulums, was analyzed some years later by Gilbert[8].

Before the Cambridge pendulums were returned to England Garland used them to provide a tie between Teddington, Ottawa and Washington. This was particularly important since absolute determinations of gravity had been made at Teddington and Washington and were in progress at the National Research Council in Ottawa. Garland gives a table comparing his results and earlier ties, particularly those made by Miller in 1928-1929. The results agree to about 10 mGal[9].

The success of the work with the Cambridge pendulums led to the decision to redesign the Mendenhall pendulums, acquired by Klotz in 1902, to bring them up to modern standards of accuracy. This work is described in a later section.

The Cambridge pendulum line covered, between Mexico City and Fairbanks, a range of over 4300 mGal. It was not ideally suited to the calibration of gravimeters; the points were far apart and were not conveniently located with respect to airports. During the summer of 1953 C.H.G. Oldham[10], a summer assistant from the University of Toronto, used a long-range geodetic gravimeter to provide a detailed survey along the Canadian section of the calibration line. The line provided an excellent means of establishing the scale constant of the instrument, which was then used to establish base stations at about 25 mile intervals along the entire range. It was also used to transfer the value at each pendulum station to a point in an airport convenient for calibration runs using aircraft.

Control Networks

The summer of 1951 was devoted to establishing a primary network of base stations throughout Ontario and Quebec; sites were chosen that were easily accessible and not likely to be destroyed by construction or other development. Looping was used to measure drift and, as much as possible, the observation points formed closed polygons so that the misclosure on each loop could be measured. If it was large, an error was indicated, and could be eliminated; if it was small the closure error was distributed statistically over the polygon. By this method a system of primary gravity bases was established, which included all pendulum bases in the area, as well as the national base at Ottawa. Innes and Thompson[11] established 270 bases during the 1952 season, which involved 20,000 miles of automobile travel and 15,000 miles of air travel in the northern region.

In 1955 gravity bases at 48 airports across Canada, from Gander to Vancouver, were established by A.M. Bancroft[12]. He used three gravimeters and travelled by chartered aircraft. Work was laid out to provide six closed loops; the survey was tied to the national base in Ottawa and at two points to the Cambridge pendulum line. Bancroft estimated that the standard deviation of the observations ranged from 0.2 mGal for stations near Ottawa to 0.5 mGal for the most remote locations.

Bancroft[13] extended the network of bases in 1957 to include five points in the Arctic – Resolute, Mould Bay, Isachsen, Eureka and Alert. The long-range geodetic gravimeter, equipped with special temperature protection and an internal calibration system, was used for this work. Resolute was tied to Churchill, an established pendulum point, and the other stations were measured relative to Resolute.

Once the network of base stations had been established it became possible to update existing surveys. There were three programs of this sort, in the Maritime Provinces[14], in southern Quebec[15] and in southern Ontario[16]. In each area long-time accumulations of observations were compiled, consolidated by additional work, and tied to networks of regional bases which were, in turn, tied to the 1952 standard. In all cases, regional maps were prepared and their geological implications discussed.

A somewhat analogous procedure took place in the Prairie Provinces between 1955 and 1960. A reconnaissance coverage of the area already existed, and in about 1955 the major oil companies began to release gravity data. The arrangement was coordinated by the Canadian Society of Exploration Geophysicists; the aim was to obtain one observation per township, that is, a spacing of 6 miles. By 1958 contributions covering 6300 townships were available.

The data showed numerous inconsistencies. Each summer between 1955 and 1960, with the exception of 1956, one or two parties were in the field establishing bases and tying the oil company contributions to them. The effort paid off; it was eventually possible to incorporate the data into the national data base[17]. Collaboration with the oil companies continued; in 1968 a line of 25 calibration stations, stretching from Cardston to Edmonton and covering a range of 500 mGal, was installed at their request.

The extension of the base network continued, often combined with regional surveys. For example, Garland and Tanner[18] established a base network stretching from western Alberta to Vancouver, tied to the Cambridge pendulum stations at Lethbridge, Red Deer and Edmonton. A large number of observations, sufficient for the production of Bouguer and isostatic anomaly maps of southern British Columbia, were tied to this network. With time this procedure of a combined operation was less used; field operations intended to provide only new or improved standards became more common.

International Ties[19]

The increased stability of gravimeters and the availability of commercial flights, made it practical in the early 1950's to tie European to North American bases by repeated gravimeter loops. In 1953 the International Association of Geodesy established a Study Group to advise on this work and to establish standards. Canada's contribution to the program was made in cooperation with the Osservatorio Geofisico Sperimentale of Trieste. Gravimeters of both organizations were shipped back and forth across the Atlantic for three round trips, being read in Europe by Trieste personnel, in Newfoundland or Montreal by Ottawa staff. The ties were to Paris and to Geneva. The results suggested that standards on the two continents agreed to within 0.07 mGal[20].

However, some doubts were cast on the validity of long-distance gravimeter ties. Repeated use of several Observatory gravimeters on the Ottawa-Washington calibration line showed that the scale constants of all of them changed substantially with time. Gilbert analyzed these changes[21] and found a variety of causes, none of which could be controlled. Given the careful looping procedures used in establishing control points this is not a serious matter when the range of observations is not too large. For long range ties, as in those to the Arctic, where the gravity difference is substantial, it may be serious. Gilbert designed an internal calibration device for the long-range North America gravimeter by which a small sapphire ball could be lowered on to the gravimeter beam and the deflection noted. The device was used in the 1957 Arctic work[13] but did not prove satisfactory in the long run. Gilbert concluded that pendulum measurements might be preferable for large gravity differences.

In 1956 the Study Group selected 34 stations, uniformly distributed around the world, to become a First Order World Gravity Net. All countries were invited to cooperate in establishing ties between these stations and in tying their national network to this standard one. Three of the stations were in, or close to Canada – Ottawa, Vancouver and Fairbanks, Alaska.

By this time the revamped Mendenhall pendulums had passed their tests. They were used to make ties between Ottawa and Vancouver and Ottawa and Winnipeg, to which many of the western and northern base networks were tied. The following year (1958) the instruments were used to extend the western pendulum network from Winnipeg to four additional prairie stations[22].

In 1959 the pendulums were used to tie Ottawa to Gander, and to four European stations of the First Order Network – Teddington, Paris, Rome and Bad Harzburg. The experiment was apparently a success, the errors at each station relative to each other being not more than 0.5 mGal[20]. However, as we shall see in a later section, the repeat ties to Ottawa showed some inconsistencies, and it was decided to redesign the instruments still further.

By 1967 the rebuilding was complete and the pendulums were used to complete the establishment of the Canadian primary network. After being checked against the Cambridge line[23] they were used to establish a number of long range ties. Many additional gravimeter ties were made and A.C. Hamilton made the final adjustment of the net, using the Departmental IBM 1620 computer. The network consisted of primary points which had a relative accuracy of ± 0.05 mGal and an absolute accuracy of ± 0.10 mGal. A secondary network of 3000 regional stations and five calibration lines was tied to this primary net.

While this work was nearing completion, the Division was cooperating in the establishment of the *International Gravity Standardization Net 1971* (IGSN 71). This developed from the First Order World Gravity Net already described, making use of the many ties made between its stations and the additional stations tied to it. By the time the analysis began, 25,000 observations, interconnecting 473 bases around the world, had been made. It was decided that this mass of data should be adjusted, not by tying everything to an adopted value at a single station, but by a least squares adjustment of the best absolute, pendulum and gravimeter data. Three different groups, one in Ottaw[24], in which C. Gantar of the Trieste observatory participated, one in Ohio State University[25] and one from the US Airforce Geodetic Squadron[26], analyzed the data independently. They then met to compare their results and to resolve their differences. By adjustment these were reduced to less than 0.1 mGal and IGSN 71, could be recommended to the Study Group. It was adopted as the World gravity standard by the International Association of Geodesy and the IUGG in August 1971.

INSTRUMENT DEVELOPMENT

The Mendenhall Pendulum Redesign – Phase I

As we have seen, the success of the Cambridge pendulum measurements inspired a desire for a set of modern in-house pendulums. After some analysis of the problem, it was decided that this could be most readily accomplished by a redesign of the existing bronze pendulums. The work was assigned to L.G.D. Thompson in 1955.

The elementary theory is simple. Two half-second pendulums, as nearly identical as possible, are mounted on knife edges attached to a rigid frame and swung in antiphase through small amplitudes. The length of time required for a large number of swings, say 3000, is measured for each pendulum and the period calculated. The mean of the two periods, sometimes called the period of the fictitious pendulum, is taken as the best value. The period T_x of the fictitious pendulum at a point x, relative to that at the base o, T_o, is related to the gravity difference, $g_x - g_o$, by the following equation

$$g_x - g_o = g_o \{ T_o/T_x \} .$$

By measuring the periods at the base and at the point x, the value of gravity at the new point can be calculated.

There are many sources of error in the measurements: flexure of the mount (which is reduced by swinging the pendulums in antiphase and limiting the amplitude); resistance of the atmosphere and the effect of carried air (which is limited by swinging the pendulums in a vacuum); friction at the knife edges (which is reduced in the Mendenhall pendulums by the use of agate); movement of the knife edges relative to the pendulum; variation of temperature inside the case (which is eliminated by good insulation and by thermostatically controlled heating coils); errors in timing the duration of the swings.

Thompson[27] set to work systematically to eliminate all these sources of error, adopting an accuracy of one part in 10^7 as his target. The pendulums were gilded to prevent oxidation and were mounted in a heavy aluminum case, from which the air could be evacuated. Provisions were made to attach this case firmly to a pier to reduce swaying. The pendulums were fitted with mirrors and a window provided in the case allowed the motion of the pendulums to be recorded. Provision was made to lower the pendulums on to the knife edges and to start and control the amplitude of the swings from outside the case. The case was wound with five separate heating coils, the numbers of turns in these coils being larger near the base to compensate for heat loss through the feet of the case. A large number of thermistors were installed inside the case to monitor the temperature which, in the final design, was constant throughout the case to 0.01°C. The recording system was external; a light beam passed through the window in the case and was reflected to record the pendulum motions on a chronograph for which the motor drive was controlled by a quartz crystal. Later this timing device was replaced by a direct reading electronic recording system.

In operation, three pendulums were used, forming sets ab, ac, bc, and each set was swung four times for periods of 50 minutes (3000 seconds), which covered 6000 swings. Repeated experiments showed that the equipment met the target specifications which gave it an accuracy of ± 0.1 mGal, quite comparable to the more modern equipment. The instruments were then used extensively, and apparently successfully, in re-defining ties in the Prairie provinces and British Columbia.

However, in measurements made at Ottawa over the three years following the redesign, the readings changed erratically. Hamilton[28] analyzed the variations and found that they exceeded substantially the sum of the possible errors that could be identified in the instrument design. He concluded that when the pendulums were removed for transport from their evacuated, thermostated, case they suffered an unpredictable change in period.

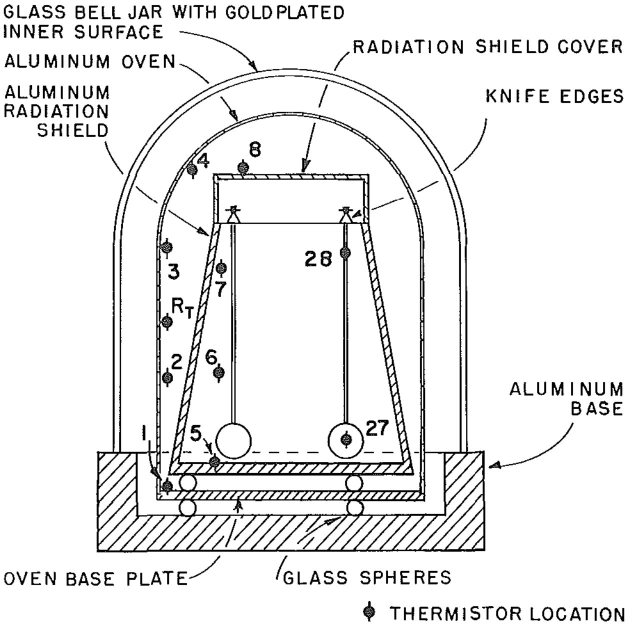
It was decided to continue with the re-design of the pendulums and H.D. Valliant undertook the work.

The Mendenhall Pendulum Re-Design – Phase II

Valliant recognized three aspects to the problem: to provide a rigid support and reproducible conditions for the swinging pendulums; to provide environmental systems that would closely control temperature, pressure and humidity; to provide data acquisition systems to measure and record the pendulum periods, phase and amplitudes and to monitor the environmental conditions.

The first two of these aims were accomplished by providing a three-chamber environmental system (see figure). The inner chamber carried the knife edges on which the pendulums swung; a surrounding chamber was the oven which provided the heat control, and the exterior chamber was a pressure-tight system within which the air could be evacuated to any desired degree.

Each pendulum carried a mirror. Light reflected from these mirrors activated a complex electronic circuit that permitted the movement of the pendulums to be recorded digitally with a high timing accuracy. Valliant described the design in a series of technical papers[29] culminating in a general description of the instrument and of its operation under tests[30]. These tests showed that the instrument was capable of measuring gravity differences to an accuracy of 0.06 mGal.



Different pairs of pendulums – ab, ac, bc – must be swung and the results averaged. One of the results of the tests was to show that, when the three-chambered system had been opened to change the pendulums, it required approximately 35 hours for the system to come to equilibrium. Since the observations with one pair of pendulums requires about 5 hours, this waiting period meant that two working days were required for each pendulum pair.

The new pendulums were ready for field testing by the summer of 1967. A re-run of the western calibration line, from Mexico City to Fairbanks, established their accuracy and reliability, and they were then used to establish a number of long-range ties in the National grid and in the International Standard Net. They proved to be the most accurate pendulums in the world, but, unfortunately, pendulums had been superseded by a new instrument, a transportable "free-fall" absolute instrument developed by J. Faller, now of the University of Colorado. Faller made absolute measurements along the western calibration line and in South America. The instrument eventually came to supply gravity measurements of an accuracy commensurate with the precision of gravimeters, so that accuracy and precision can now be discussed in the same terms. The day of the pendulum was over!

The Vibrating String Gravimeter

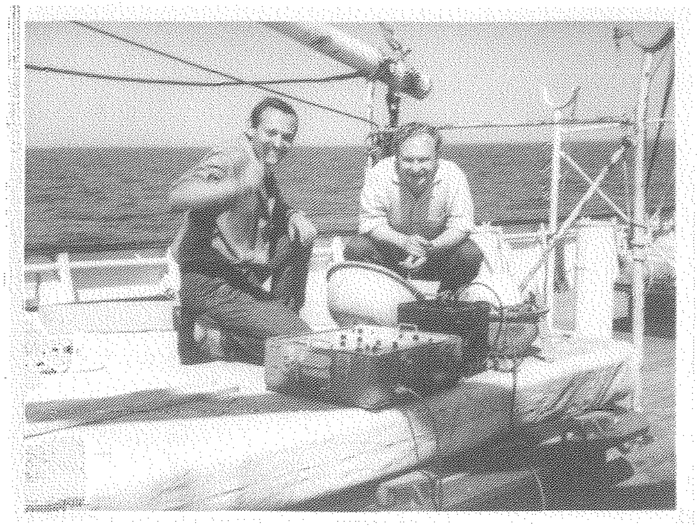
R.L.G. Gilbert began the development of a vibrating-string gravimeter at Cambridge University. When he joined the Division staff in 1956 he brought the partially developed instrument with him, and devoted a great deal of effort to it over the four years he remained in Ottawa. It was hoped that the instrument could be used to measure gravity at sea, or possibly in a drill-hole.

The instrument involved a mass suspended by two parallel metal strips which were caused to vibrate at an audio frequency between the poles of a magnet. The vibrations were sustained at the natural frequency of the strips through the use of feedback in the same way that a quartz crystal is caused to oscillate at its natural frequency. The natural frequency varied as the square root of gravity, and could be determined by integrating the number of oscillations over a given period, usually a few minutes. The non-linear response of the natural frequency to gravity changes was unfortunate in an instrument intended for use at sea, since it meant that the sinusoidal motions of the ship could not be averaged out but must be computed separately and subtracted from the results. Nevertheless it was hoped that the meter might be used at sea under quiet conditions, such as in a submerged submarine.

By late 1957 the basic package was ready for testing and Gilbert was invited to conduct these tests on board a Dutch submarine in the West Indies, where its output could be compared with the results of the Vening-Meinesz pendulums. The tests went ahead early in 1958 but the electronics failed and few results were obtained.

During 1959 the oscillator was redesigned and the construction of the auxiliary equipment was completed. Laboratory tests suggested a potential accuracy of ± 0.5 mGal. Then, in 1960, Gilbert left the Observatory to return to England and the management of the project was turned over to D.R. Bower.

Bower was unable to duplicate the drift performance of the original Gilbert instrument and in 1962 abandoned the all-metal design in favour of an all-quartz design. In this he benefitted by the availability, in Toronto, of M. Dicesaro, the glass blower who made the original quartz sensing unit used in the Worden gravimeter. Although the quartz unit which he built evinced less drift than the Gilbert instrument, the project was overtaken by the demonstrated success of spring-type gravimeters in measuring gravity from surface vessels. Bower felt that the non-linear response of the vibrating string gravimeter made its use in surface ships unattractive when compared with the LaCoste-Romberg or the Askania spring-type gravimeters; he subsequently turned his attention to evaluating the performance of these gravimeters at sea.



H.D. Valliant, right, with the underwater gravimeter and its water-tight case.

Measuring Gravity at Sea

The development of instruments capable of measuring gravity on, or under, water was of the greatest importance to the Division since it permitted the extension of land-based surveys over the continental shelves, Hudson Bay and the Great Lakes. While the Observatory had little to do with the development of these instruments, it had much to do with their evaluation; some description of them is desirable.

The underwater gravimeter used by the Division was that manufactured by Lacoste-Romberg, which became available in 1961. It was[31] essentially their land gravimeter unit mounted in gimbal rings in a water-tight case. It was lowered to the sea bottom by a winch, the position was maintained by the ships engines without the need to anchor, and the readings could be obtained in four or five minutes. It was provided with remote controls; various indications, including level and instrument setting, were displayed on a control panel operated on the ship's deck. The instrument had a low drift rate and good repeatability characteristics, and gave gravity values accurate to about two or three tenths of a milliGal.

Ship-borne, as opposed to underwater, gravimeters had been under development for some years, and in the early 1960's the Bedford Institute of Oceanography was considering the acquisition of one of them. Their operation is of course much more complicated than that of a land-based or sea-bottom gravimeters; the variations in acceleration due to the ship's motion are much larger than the gravity differences to

The underwater gravimeter about to be lowered to the ocean floor.

be measured, and must be filtered from the observations. A variety of equipment was needed to measure the three components of the ship's displacement and acceleration.

In order to test the accuracy of the system a test range was needed over which the values of gravity had been carefully determined. A.K. Goodacre established such a range off Nova Scotia in the spring of 1963. Some 200 measurements were made with a Lacoste-Romberg sea-bottom gravimeter in a 30-mile square. The local Decca navigation system was used to position the stations and the water depth was carefully measured. Goodacre estimated that sea-surface gravity values within the range were accurate to within ± 1 mGal[32].

Later in 1963 the range was used for extensive performance tests of two competing shipborne gravimeters: a LaCoste-Romberg and an Askania. The gravimeters were mounted near the centre of motion in CSS Baffin and 100 runs were made over the range under various weather conditions, including a hurricane! An interesting aspect of this test is that the gyro-stabilized reference system, used as a platform for the array of accelerometers that monitored the ship's motion, was salvaged from the Avro Arrow project which had been cancelled by the government just a few years before. This reference system was refurbished by the NRC with assistance from D.M. Bower and a postdoctorate fellow Peter Watt. It permitted a comprehensive analysis of the gravimeters' performance and demonstrated for the first time the presence of cross-coupling effects and a method for their removal[33]. Accelerometer, gravimeter and navigation (DECCA) data were recorded on magnetic tape and were subsequently computer analyzed to provide information on all aspects of thc tests. As a result of the tests the Askania instrument was selected, and in subsequent years made many surveys, and many traverses of the test range. The Observatory acquired a Lacoste-Romberg instrument in 1972.

EARTH TIDES

Gravity differs from geomagnetism and seismology in that its value at any point can, for most purposes, be regarded as constant; it isn't subject to the movements of a pole or to gravity storms and there are no gravity 'quakes. There would be no need for gravity observatories – except for one thing: the earth is subject to tidal effects which vary with the changing attractions of the sun and moon. The variation is small, between 0.1 and 0.3 mGal, and unimportant in most field work, but a knowledge of its magnitude and period permits a determination of the earth's rigidity.

The interest of the Observatory in Earth tides is not a new one. Readers of Part I will recall Klotz' frustration with the Zöllner pendulums designed by Hecker and sent to Canada as part of a program approved by the International Seismological Association in 1911. That program came to nothing.

In the spring of 1949 the Shell Oil Company organized a world-wide program in which the Observatory and the University of Toronto took part. As in 1911, the aim was to provide information on the rigidity of the earth[34]. At both points, and at many other places around the world, gravimeters were read at fifteen minute intervals over a period of fifteen days. The observations were turned over to the Shell Oil Company. In Toronto, the experiment was continued for an extra 15 days and the results were independently analyzed by M. Reford.

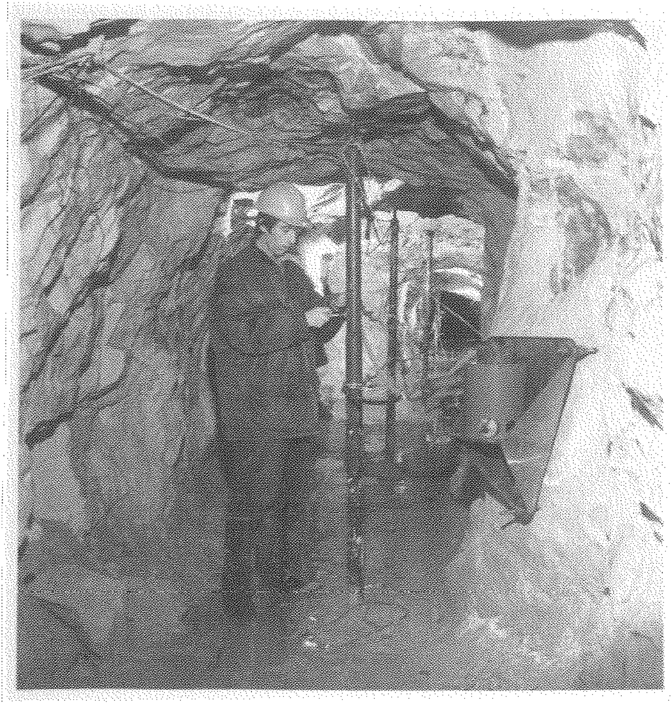
The next interest in Earth tides was during the International Geophysical Year, when the Observatory undertook to record earth tides at three points in Canada. Two gravimeters, one lent by the Rio Canadian Exploration Company of Toronto, were adapted to direct recording and had their sensitivities adapted to permit registrations of gravity differences as small as 0.01 mGal. Both instruments were operated at Ottawa during the summer and early autumn of 1957[35]. In October one of them was transferred to Resolute where it recorded successfully for three months; it was then moved to Meanook. Data from these instruments, up to September 30, 1958, were forwarded to the Centre for Earth Tides (CET) in Brussels. The CET had been established that year in response to a resolution by the IUGG at its Toronto meeting in 1957.

The modern Earth tide program was begun by D.R. Bower in 1966 with the purchase of two Melchior-Verbaandert horizontal pendulums from the CET and a Lacoste-Romberg recording gravimeter. The purposes of the study were:

(i) to extend the existing coverage of continuous, systematic observations of the Earth's tide;

(ii) to investigate the reliability of microgravimetric instruments and the techniques of site selection and installation;

(iii) to investigate the utility of these measures in regional studies of the Earth's crust.



Jim O'Brien with the water-tube tiltmeter in the mine at Poltimore

By 1966 the first stage of the program was in operation. An abandoned asbestos mine had been acquired near Poltimore, Quebec, 30 miles north of Ottawa, the interior and exterior walls near the entrance had been scaled and cleared of debris and the horizontal pendulums were installed in a niche cut into the wall. A precise survey established an east-west line in this niche to permit the exact alignment of the instruments.

The Melchior-Verbaandert pendulum employs a Zöllner suspension. Much of the aggravation inherent in this device was avoided through the adoption of a light-spot follower to convert the pendulum motion directly to electrical output and the use of a servo mechanism to sense when the pendulums were drifting off scale and to re-level them. The earth tide recording gravimeter was set up in a specially prepared dry room within the mine. In 1967 Bower completed a Master's thesis at Carleton University[36] which discusses the motivation and objectives of early work of this kind. The Poltimore site operated more or less continuously for several years.

Bower was on educational leave during 1968 and 1969, reading for his PhD at Durham. By this time it was apparent that the principal outstanding problems in earth tide research were how to measure tidal tilt and how to correct for the perturbing effects of the ocean tide. With these problems in mind, Bower duplicated the Canadian installation in a mine in England. This work led to the development of a general technique for calculating the effects of the global ocean tide on tidal gravity and tilt measurements[37]. This was the first such calculation to be made, and his results for Europe and England[38] are still referred to.

While in England, and with the partial support of the Royal Society, Bower developed a long-baseline hydrostatic tiltmeter[39] to overcome siting problems, later identified as strain-tilt coupling effects, commonly associated with short baseline horizontal pendulums. This tiltmeter was subsequently duplicated and installed in the mine in Canada.

This mainly pre-1970 research led in later years to such diverse applications as the calculation of ocean-tide loading for Very Long Baseline Interferometers and absolute gravity surveys, determination of fracture parameters in hydrology, experimentation in earthquake prediction and to site selection and ocean-tide correction for cryogenic gravimeters of extreme sensitivity and stability.

THE MAPPING OF CANADA'S GRAVITY FIELD

In 1957 Innes issued the first Gravity Map of Canada. It was based on 15,000 gravimeter observations, with station spacing ranging from 10 to 50 km. Its appearance may be said to have marked the end of an initial experimental period; it coincided with a period of program evaluation that led to an explosive and accelerating expansion in the gravity survey of Canada.

In settled areas of Canada the observations had been made on roads, using motor transport. That was the easy part and it was nearly completed. In remote areas, operation had depended on fixed-wing float planes. The parties had been small, with little logistic support, dangerous in the largely unmapped wilderness. Spacing of observations varied, but was everywhere too sparse to permit adequate interpretation.

Denser spacing was required, but if the country was to be surveyed at such spacing in any reasonable time, the rate of operation must be drastically increased.

A new method of conducting field surveys evolved to meet this need. It was accompanied by other new departures – surveys of water areas, made possible by the development of sea-bottom gravimeters, and the exploration of the Arctic islands and polar shelf, which followed the establishment of the Polar Continental Shelf Project in 1959.

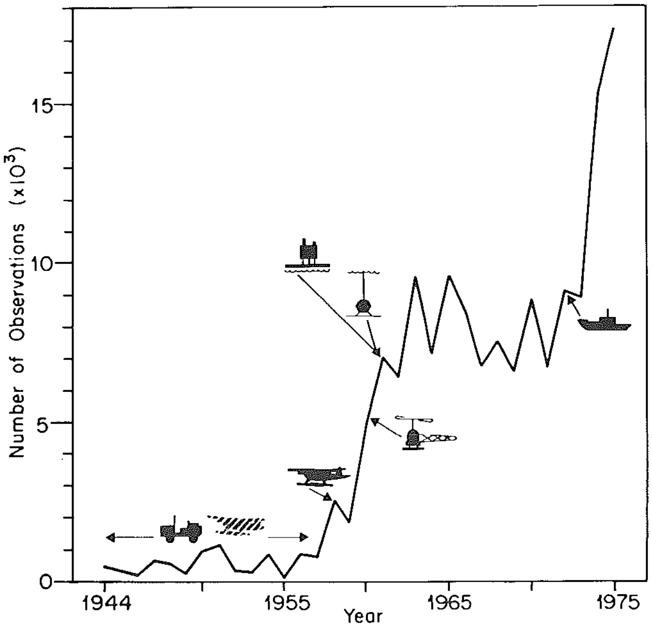
Three categories of gravity surveys are thus involved: land, ice-surface and underwater. In all these categories Lacoste-Romberg gravity meters became the standard, the model G geodetic meter on land, the same instrument, damped, on ice and, heavily damped and remotely controlled, for underwater measurements.

The figure below[40] demonstrates this expanding rate of data acquisition and indicates, by symbols, the reasons for it. The introduction of helicopters in 1960 was one major factor, the beginning of ice-surface and underwater surveys in 1961 was another. The radical increase in 1972, which falls beyond our period of interest, was due to the introduction of systematic sea-surface surveys by the Division.

This tremendous expansion was not easily accomplished. Every step in the process was new – the development and supply of the large field parties in areas increasingly remote, operation in the unfamiliar environments of the ocean and of the Arctic, the establishment of methods to retrieve, store and analyze the constantly increasing stream of data – and every step brought problems. They were met as they arose. J.G. Tanner, one of those involved, has said[41]:

> "The intensity of effort was enormous. *No* problem was too large or difficult. If there was a problem all hands were on deck, day and night, until we beat it. Morris [Innes] gave us lots of leeway and was a great source of encouragement."

We shall now look at some of the problems, and solutions.



Land-Based Surveys

A new program of systematic gravity mapping began in 1957. Planning was coordinated with the Surveys and Mapping Branch, so that gravity surveys were begun only where the topographic sheets were available. Since mapping to a scale of 1:250,000 was progressing rapidly, and was completed in 1967, this posed no serious limitation. Larger parties were organized, of 9 to 12 persons, under the direction of a party chief. Each party was supplied with two or more small aircraft. Until 1960 these were fixed-wing float planes; after 1960 helicopters, of increasing power and range, were used. Each helicopter party was able to obtain about four gravity observations an hour, or 32 per day. A larger, fixed-wing aircraft delivered fuel and supplies. These large parties covered up to 250,000 square miles in a single summer.

Usually two, sometimes more, of these regional surveys were mounted each season, under the direction of Tanner, Hamilton, Hornal or McConnell, and they were the principal contributors to the constantly increasing data bank.

Not all surveys were of this magnitude. Detailed studies of specific geological structures were made, sometimes several in a field season. These included the crater investigations considered in an earlier chapter.

Another source of data was a joint program with the Geodetic Survey to measure gravity at each of the precise levelling bench marks in Canada. These observations were needed by the Survey to determine the fluctuations of the geoid in Canada. Between 1964 and 1970 more than 3100 observation were made in this program.

Under-Water Surveys

A country facing three oceans, with its southern boundary defined in part by a chain of sea-sized lakes, has an obvious need to measure gravity at sea. It was partly in recognition of this need that so much effort was devoted to the development of the vibrating string gravimeter. The availability of sea-bottom gravimeters in the early 1960s led to the abandonment of that project. In 1960, in preparation for the new era, J.R. Weber observed the operation of a Graf underwater meter in Lake Erie, and D.R. Bower participated in a survey of Hudson Bay using a Lacoste-Romberg gravimeter mounted in a United States submarine.

The first use by the Division of the Lacoste-Romberg underwater gravimeter was in 1961. Weber and A.K. Goodacre established 30 stations in Hudson Bay, between Churchill and the Belcher Islands. The traverse was made on the MV THERON. The technique of holding the ship steady, under power, while the observation was made was here tried for the first time. Weber and Goodacre, concerned that this might be difficult to do, had designed a carbide bubbler, to be attached to the gravimeter, so that the captain could see from the bubbles where the instrument was. The device had been tested in Meech Lake. At sea, in Hudson Bay, gale force winds persisted during the entire survey. The captain couldn't see the bubbles, but he was able to maintain the ship's position by observing the angle at which the gravimeter cable entered the water.

After this initial test, underwater surveys became a regular part of the field program. In 1962 and 1963 Goodacre observed at 400 stations in the Gulf of St. Lawrence, and in 1963 he established the testing range for ship-borne gravimeters already described. In 1964 Weber and Goodacre established 230 stations in Lake Superior, and in 1965 were part of a major Departmental study of Hudson Bay. All of these surveys will be discussed in detail in a later section. The Canadian Geophysical Bulletin gives the following information about later underwater surveys:

1966 –	313 stations southwest of Newfoundland
	234 stations off the coast of British Columbia;
1967 –	397 stations in the Gulf of Saint Lawrence
	316 stations off the coast of British Columbia;
1968 –	272 stations in Lakes Ontario and Erie;
1969 –	400 stations in Lakes Ontario, Erie and Huron;
1970 –	692 stations off the east coast.

Arctic Surveys

The advent in 1959 of the Polar Continental Shelf Project also added to the output of gravity data. The Division took full advantage of the facilities it provided, both in staff recruitment and in field work. Not less than eight of the Division staff members were initially recruited in Polar Shelf positions and transferred to the Observatory staff as positions became available. Major surveys, to be described later, were mounted during every field season. As the Project base moved, from Isachsen to Mould Bay in 1964, to Tuktoyaktuk in 1968, gravity surveys of the Queen Elizabeth Islands, of the ice-covered waters between them and of the ice-covered continental shelf were completed and published in the Gravity Map Series.

Data Storage and Retrieval

By the late 1950s the rate of gravity surveys had grown so that the processing, sorting and compilation of the data became an increasingly formidable task. The development of new systems, based on the rapidly expanding capabilities of computers, was undertaken principally by J.G. Tanner and R.J. Buck.

The initial response[42] was to transfer the field data to IBM punch cards and to use an electronic computer to calculate the principal facts; the computer simply carried out the calculations previously made on a hand calculator. It was also programmed to sort the results in any form, for example by degree squares, that might be required.

By 1962[43] the system had become more sophisticated. Four sets of punch cards were used, colour coded. The first was for control stations. The second, known as the traverse card, gave the information about an observation – the control station it was related to, the times of the before and after ties to that control station, the gravimeter used and its scale constant. The third contained details about the observation point, its elevation, ice thickness or water depth if applicable, and the gravimeter reading. From these cards the computer could calculate the value of gravity at the observation point and compute the free-air and Bouguer anomalies. This information was placed on a "Principal Fact" card. These could be

L.W. Sobczak, in a polar storm at Isachsen.

processed to give any desired printout. The programs were designed to operate on an IBM 1620, and all files were stored on magnetic tape.

The variously coloured cards were punched from data sheets prepared in the field. These sheets included space for field officers to compute preliminary values of the anomalies. This was done, using hand calculators, to provide a preliminary scan of the observations while any suspect ones could still be checked.

The use of magnetic tape storage and of the Departmental IBM 1620 continued for some years[44] and many programs were written to process the stored data. Of particular importance was a system of terrain correction developed by Nagy[45] which permitted an added refinement in the computed anomalies.

A flat-bed electronic data plotter was acquired in 1963. Any set of output cards could be used to plot the location of the stations and label them with pertinent data. Purchase of the plotter was a major triumph for the Division. Other mapping agencies within the Department needed data plotters and headquarters administrators were determined that a central facility could service all the needs. The Observatory was able to show that its need to plot not only the location of the points but also selected data would require nearly full time use of the plotter. Headquarters capitulated, which was fortunate. When oil companies found that gravity data could be provided in map form, outside demands for plotted values accelerated enormously.

Automatic plotting of positions on the maps greatly reduced the work of map compilation, even though contouring had to be done by hand. As the rate of data acquisitions and map production increased this was unsatisfactory, and by 1967 a flat-bed plotter, which could plot and contour data supplied by magnetic tape, was in operation.

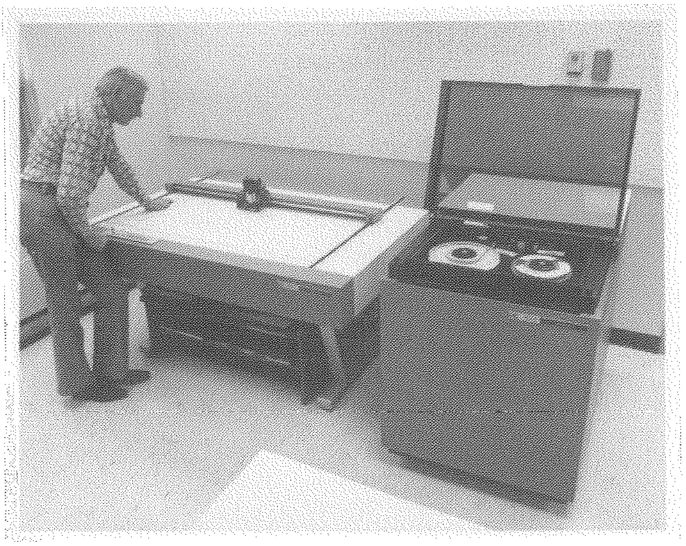
It was not just the plotter that had become inadequate. The storage of data on magnetic tape made for storage problems and slow retrieval times. The advent of disk-pack storage and the commercial availability of high-speed "number-crunching" computers to make use of them, called for a new approach. By 1970, when our story ends, the new system was in place[46]. It was called POGO, for Processing of Gravity Observations. It involved six major programs. The first, a "gravity traverse reduction program" was similar in purpose to the main IBM 1620 program; it processed the new data, checked it for inconsistencies, and produced a final output suitable for disk storage. A second "general" program produced map data, on any scale and for any of four different map projections. A "general utility" program transferred any selected data on the disk file to any desired output format. The "profile plotting" program, given the latitude and longitude of the end points of a profile, selected points within a band of specified width, interpolated them to the line, and plotted the profile to any specified scale. There were, in addition, two programs for updating or reorganizing the disk pack files, and a package of programs for base networks.

One can readily believe that "using this system, the Division [was] able to satisfy the needs of not only its own field officers and scientists but also those of universities, research institutions and the exploration industry".

APPLICATIONS OF THE GRAVITY DATA

Geological and Structural Interpretations

When the regional mapping had made sufficient progress, the data could be combined to produce a gravity map of Canada, which showed the broad features of the gravity field. One such map had been released in 1957, a second, which contained all the data available to the end of 1966, was published in 1968. It shows strong negative Bouguer anomalies over the Cordillera, relatively low anomalies over much of the interior plains[47] and the Arctic islands. These low-anomaly areas are cut in numerous places by bands of strong negative anomalies, which generally coincide with areas of intrusive rocks and were therefore of much interest to geologists in general and to the mining companies in



Peter Winter with the automatic plotter.

particular. These areas were the subject of several more detailed papers. We shall consider the several areas in order, from western to eastern Canada.

Coastal British Columbia and the Cordillera

Garland and Tanner[18] produced the first Bouguer and isostatic maps of southern British Columbia. They were followed by a number of University groups. W.R.H. White, of the seismological section at the Dominion Astrophysical Observatory, working toward his PhD under the supervision of Professor J.C. Savage at the University of British Columbia, made a detailed geophysical study of southern British Columbia[48], including extensive observations of gravity along the east coast of Vancouver Island. Geophysicists from the University of Alberta investigated the southern section of the Rocky Mountain Trench[49], and the University of British Columbia made concentrated studies in the southwestern section of the province[50]. These data, and field surveys made by the Observatory in 1963 and 1966[51], all contributed to the 1968 Gravity Map of Canada. The latter survey extended detailed coverage northward along the mainland coast and on the Queen Charlotte Islands. Underwater surveys of the Strait of Georgia and the Queen Charlotte Sound were completed in 1967[52], too late to be included in the 1968 Gravity Map, but available for discussion in 1969 by Stacey and Stephens[53]. Their paper included a detailed Bouguer anomaly map of the coastal area; it showed a positive anomaly along, and parallel to, the western edge of the islands, and a negative anomaly along the Coast Range, with a belt of average zero anomaly separating them. The positive anomalies along the islands were attributed to the change from continental to oceanic crust, the negative anomaly under the mountains to the thickening of the crust. The intermediate, quasi-zero anomalies showed minor variations which could usually be attributed to density variations in the surface rock.

Stacey next turned his attention to the Cordillera[54]. He made a detailed study of a band 2° wide, from 49°N to 51°N, reaching from the Pacific (132°W) to the Great Plains (112°W). The mountainous region to be covered made accurate terrain corrections vital. Stacey and L.E. Stephens reviewed existing techniques, established the best one to be followed, investigated conditions affecting its accuracy, and devised a computer program to carry it out[55]. This done, Stacey produced a Bouguer anomaly map for the belt described above.

As might have been expected, the anomalies were mostly negative over the highest mountain ranges, approximately zero at sea level, and becoming increasingly positive over the oceans. Considering small areas, 1° in longitude x 1/2° in latitude, and calculating average values of elevation and anomaly, Stacey showed that a second-order relation existed between mean anomaly and mean elevation. This relationship was stronger than anomaly variations due to surface geology.

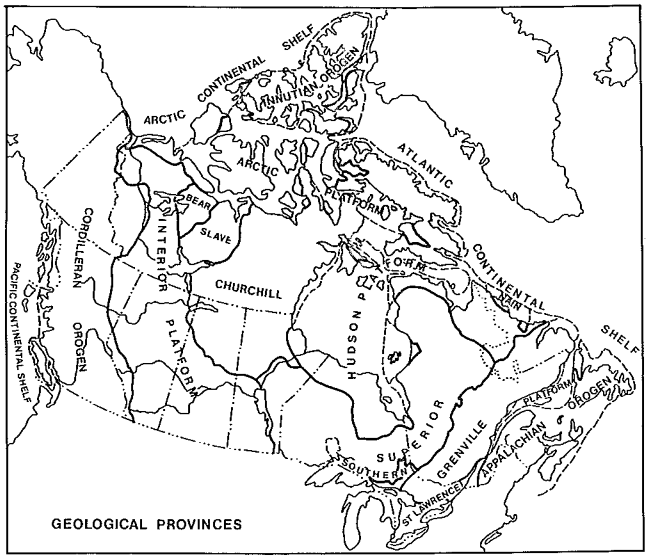
The relationship between mean elevation and mean anomaly suggested an isostatic compensation for the surface topography. Assuming that this was due to "roots" of crustal material, Stacey tried a number of models and found that the anomalies could only be explained by assuming a decrease in the density of the crust and upper mantle west of the Rocky Mountains; he took this to mark the western edge of the Precambrian Shield. Still farther west, under Vancouver Island, he found evidence for the remnants of an oceanic plate subducted under the continental crust of the island.

The Canadian Shield

In 1962 Stockwell[56] introduced the idea of structural provinces within the Canadian Shield. Each of these provinces had its characteristic structural trends and styles of folding. Since the idea was introduced, the boundaries between the provinces have been increasingly well-delineated where structural trends are cut off along unconformities or orogenic fronts, and the validity of the subdivisions of the shield have been confirmed by isotopic dating. The map on this page shows the Geological Provinces in Canada, as currently defined[57].

Gravity data have proved to be a powerful tool in the definition of the boundaries. During the late 1940s, Innes was at the University of Toronto, where he obtained his Doctorate in 1950. Part of his thesis[58] was devoted to the gravity field in Northern Manitoba and Ontario, and this material, much expanded, eventually appeared as an Observatory Publication[59].

The observations on which the study was based were made in the early days of gravimeter work, between 1947 and 1950, during which the problems of reconciling the observations of different seasons and with different gravimeters surfaced. Innes discusses this problem in detail, lists the "principal facts", produces maps of Bouguer and Hayford isostatic anomalies, and discusses their implications. Bouguer anomalies had a range of more than 100 mGal. Most of these could be related to the lithology of the upper part of the crust, but a major tensional feature, the "Kapuskasing high", cutting across the Shield, was defined and two prominent negative belts were interpreted as defining the roots of ancient mountains. Most importantly, the gravity data clearly defined the boundary between the Superior and Churchill



Canadian Structural Provinces. After C.H. Stockwell, Geological Survey of Canada, Paper 80-19, 1982.

provinces. At about the same time, Innes[15] showed that a strong negative anomaly marks the boundary between the Grenville and Superior provinces in Quebec.

In 1965 and 1967 major gravity surveys were made over the northern Prairie Provinces[60] and adjacent areas of the Northwest Territories. More than 2000 samples of fresh Precambrian rock were collected. The samples provided a great deal of information on the densities of various types of Precambrian rocks and of possible variations[61]. Using these densities, Gibb considered the revised Bouguer anomaly map stretching from 92° to 104° west longitude and from 54° to 60° north latitude. Most of the anomalies in the region could be related to the rock-types in the upper crust but in addition they defined the boundary between rocks of different ages. Once again, the boundary of the Churchill-Superior provinces[62] was defined, but in more detail.

As his PhD thesis at the University of Durham[63] J.G. Tanner studied the structural boundaries in the eastern Canadian Shield, particularly that between the Grenville and Superior provinces. He suggested that these anomalies represent the edge effects between two large structural blocks, each with its own properties and each in relative isostatic equilibrium. Density measurements and field geology suggest that the crust in the Grenville front is denser than that of the northern, Superior, province; the theory of isostasy would then demand that it be thicker. This inference was later supported by seismic results[64]. There would thus be two factors affecting the gravity field: the higher density rocks would give a positive anomaly, the deeper root would give a negative one. Over the boundary, the nearer, positive effect would be greater; as one moves away from the boundary the deep-seated negative effect would predominate. A sharp boundary anomaly would result. Tanner did not suggest any mechanism for the phenomenon. Later, working with M.D. Thomas[65], the idea was developed that the effect might be due to continental collision, the deep root of the front being a subduction zone.

By the late 1960s the idea of continental drift, and of collisions between plates of continental dimensions, was well established, although not universally accepted. R.A. Gibb noted[66] that the Slave Province fits nicely into the great arc in Hudson Bay. He suggested that a rupture had occurred some 2390 million years ago in which the Slave Province had been separated, by rifting and ocean spreading, from the Superior Province, leaving the large arc. Gibb and Walcott[67] studied the boundary between the Churchill and Superior provinces in great detail and found that the rock types were everywhere consistent with a boundary formed by the collision between the younger Churchill Province and the stable Superior Province.

Much later, when the gravity pattern of many structural province boundaries had been defined and seismic studies had given some idea of crustal thicknesses, Gibb and Thomas[68] showed that in all cases the gravity pattern is consistent with a collision between an older province and a less dense younger one. As a result of this and later work by the same authors three major geosutures – the Superior-Churchill suture, the Thelon suture and the Grenville suture – were identified. Their recognition and eventual acceptance had a profound effect on later geological interpretations of the Canadian Shield[69].

Lake Superior

In 1964 seismologists in the United States and Canada collaborated on a major study of the crust underlying Lake Superior, to be described in a later chapter. As a contribution to the project Weber and Goodacre made a gravity survey of the lake[70] from the Department of Transport MV PORTE DAUPHINE. They established 230 stations; land ties at four points around the lake established the relation to land surveys on both sides of the border.

Bouguer anomalies ranged from -90 to +25 mGal and there was good correlation with exposed rocks. The mean anomaly over the lake was -25mGal, as compared with the mean anomaly over the surrounding highlands of -45mGal. This difference was to be expected; if isostasy prevails the Bouguer anomalies should be more positive over topographically low areas than over high ones. Further, a thin crust would be expected under the low area. However, on the contrary, the seismic data showed a thickened crust! Of this, more later.

The Hudson Bay Lowlands

In 1965 the Department mounted a major study of Hudson Bay. It involved two ships, CSS HUDSON and MV THERON, and 18 shore parties, and combined the resources of the Geological Survey, the Canadian Hydrographic Service, the Bedford Institute of Oceanography and the Dominion Observatory. W.R. Pelletier, of the Survey, was Project Leader and Chief Scientist on the HUDSON; Goodacre was Chief Scientist on the Theron.

A shallow seismic profile, to measure the thickness of the sediments, was run by the Geological Survey[71], and a crustal seismic profile by the Bedford Institute[72]. Extensive gravity surveys were made; underwater gravimeters were operated on the HUDSON by Weber, on the THERON by Goodacre. Approximately 800 gravity stations were established[73]. In addition, continuous gravity traverses were made with an Askania surface meter, mounted on the HUDSON, and operated by R.V. Cooper.

The gravity data were presented in a Bouguer anomaly map[74]. The anomalies ranged from -70 to +10 mGal, with a mean value over the lake of -25 mGal. This value is on the average 14 mGal higher than the mean anomalies over the surrounding Canadian Shield areas. There were a number of areas of relatively high positive anomaly which could be well accounted for as extensions of structures already known in adjacent areas of the Shield.

A gravity profile was prepared to coincide with the seismic traverses that had crossed the central part of the Bay, from Churchill to the Ottawa Islands. The shallow seismic survey suggested three principal sedimentary layers underlying the lake to a total depth of about 2 km. These were shown to have little effect on the gravity field. The deep seismic sounding had shown the crust-mantle boundary to vary from a maximum of 42.7 km toward the centre of the lake to a minimum of 26.4 km near Chesterfield Inlet, but to be quite irregular. There was no correspondence between its variations and those of the gravity field. It was concluded that the gravity variations were due to systematic density variations in the Precambrian basement rocks; alternatively, seismic velocities in the crust might be changing irregularly, with resultant errors in the computed depths. Weber and Goodacre investigated this in a separate paper[75] and found that either possibility would explain the observations, and that the relationship might change from one geological province to another.

How is one to explain the relatively positive mean anomaly of 14 mGal over the lake, in light of the fact that the crust is irregular and, in general, somewhat thicker than normal for the Shield? It is necessary to postulate denser than normal basement rocks. Perhaps these dense rocks have caused a depression and a consequent sedimentary basin.

The implications of the gravity field to isostasy and to the question of post-glacial uplift were investigated in a separate paper[76]. The authors produced a map of the free-air anomalies and concluded that, after eliminating the effects of local structures, they were left with a target-shaped pattern of negative anomalies, reaching a magnitude of -25 mGal over Hudson Bay. They estimated that the crust has yet to rise 180 m before isostatic balance is restored. These results confirm an earlier study by Innes and Argun-Weston[77].

The results of the underwater surveys in Lake Superior and Hudson Bay were inconsistent with conventional thinking in two ways: the usual idea that a thin crust was associated with a topographical low area and positive Bouguer anomaly was not supported; and the density contrast at the crust-mantle boundary proved to be 0.2 g/cm^3 rather than the 0.6 g/cm^3 normally assumed. Goodacre[78] considered all the data available about the nature of the crust-mantle boundary – density differences, the change in seismic velocities at the boundary, the temperature and the temperature gradient. Comparing these values with laboratory measurements of the physical properties of rocks he showed that such rocks as amphibolite and intermediate to basic granulite were constitutes of the lower crust, and that garnet peridotite was probably an important constituent of the upper mantle. He also showed that these mineral assemblages, and in consequence the crust-mantle boundary, were stable over very long periods of time.

Alexandria Area, Eastern Ontario

The area considered in this study covers 2500 square miles centred on Alexandria, a village lying some 50 miles east of Ottawa. The area is flat, obviating the need for terrain corrections, and, except for the extreme boundaries, is covered by flat-lying Ordovician sediments. The faulting of the area has been well defined.

The early survey by Saxov[3], in which he set up the Prescott-Maniwaki calibration line, had been concentrated in this area. L.W. Sobczak[79] used Saxov's observations and others made between 1945 and 1964 to produce a Bouguer anomaly map[80]. Rock densities measured by Saxov and those from two drill holes were available, and he augmented these with a large number of density measurements from rocks in the survey area.

The densities of the Palaeozoic limestones are much the same as the underlying Precambrian rocks, but where the Precambrian rocks penetrated the limestones as intrusives they produced strong anomalies which permitted their close definition. In most cases there was also good correlation of the magnetic field with these intrusives.

Appalachian Area

The Appalachian province includes the Maritime Provinces and Newfoundland, and adjacent areas of Quebec south of Logan's Line and as far west as the Richelieu River.

A number of studies relating to this area have already been mentioned. Garland[14] collected all the data available to the end of 1950 and published tables of the principal facts and a Bouguer anomaly map of the three Maritime Provinces. In general, the variations in the gravity field could be related to the type of basement rock; in several cases he was able to estimate the thickness of these formations. Particular anomalies, especially those in the Moncton area, received special attention. Tanner and R.J. Uffen[81] made a similarly detailed study of Gaspé.

The first major use of the underwater gravimeter was in the Gulf of St. Lawrence, during the 1962 and 1963 field seasons[82]; 450 stations at intervals of about 13 km were observed south of a line running from the Gaspé Peninsula to Cape Breton. Goodacre published a Bouguer anomaly map in the Gravity Map Series, and discussed it in detail in a separate paper[31]. It is fascinating to see how the Gulf observations complement and bring together the maps already published for the adjacent land areas[14,81]. A number of sea-bottom anomalies were detected and analyzed. There was not good agreement with the sedimentary thickness variations mapped seismically by Willmore and Scheidegger[83].

During the summer of 1964 the Division conducted a major survey of Newfoundland and adjacent areas of Quebec and Labrador[84]. The results are discussed in detail by D.F. Weaver[85]. The range in Bouguer anomaly was not large, lying approximately between +50 and -60 mgal, but there was an interesting separation of positive anomalies, to the south, from negative anomalies to the north. The line of separation could be drawn at approximately 50° N, cutting the Great Northern Peninsula off from the rest of Newfoundland. The negative values continue to the north, in Labrador and Quebec, but seem to be changing to positive values again to the east, oceanward from the Peninsula.

What accounts for the difference? Seismological evidence indicates that the crust south of the line is thicker and more complex than that to the north. If one supposes some corresponding variation in the densities of the crust and upper mantle, the gravity observations are consistent with the seismic model. Weaver postulates that the line separating negative from positive anomalies may mark the boundary of the Appalachian and Grenville geologic provinces. In the light of the later work of Tanner, the pattern is now recognized as an example of a boundary effect.

There are a number of anomalies in the map, and these were investigated. Most of the negative anomalies proved to be related to the surface presence of granite, the positive ones to large areas of basic rock, or to ultra-basic intrusions.

The Royal Society of Canada convened two symposia, in 1966[86] and 1967[87], relating to the Appalachian. Observatory personnel took part in both. Of particular present interest is a contribution by Innes and Argun-Weston in which they discuss gravity measurements in the Appalachian and their implications[88]. Citing all the authors who have conducted gravity studies in the area, they find that the range of Bouguer anomalies is everywhere of the same order as that found by Weaver in Newfoundland, with the northern Shield areas showing generally negative anomalies, the southern Appalachian province generally positive ones. This trend, which is now recognized as a boundary effect marking the boundary of the Appalachian and Grenville provinces, breaks down in the middle section of the range, south-east of Quebec city, but continues through eastern Quebec and New Brunswick and, as we have seen, through Newfoundland. These general trends are vitiated at various places, but these exceptions can generally be explained in terms of surface rocks or of intrusions at depth.

The question of isostatic balance is interesting. The Appalachian region is under-compensated by about 20 mGal, the Shield is overcompensated by about 10 mGal. The authors attribute this to differences in crustal thickness, the Appalachian area having a thicker and more complex crust.

The Canadian Arctic

Few gravity observations were made in the Arctic archipelago prior to the setting up of the Polar Continental Shelf Project in 1958. From that time major gravity programs were carried out annually. The results of these studies appeared regularly in the Gravity Map Series[89], but a limited number of formal publications had appeared during our period of interest.

The first of these papers, by Weber[90], considers the gravity field over the continental shelf off Ellef Ringnes and Meighen Islands. The difficulties experienced with operating gravity metres on ice, particularly on ocean ice, have already been mentioned, but Weber estimates that the observations made using a LaCoste-Romberg helicopter meter were accurate to ± 0.3 mGal. Observations were at 10 km intervals over an area some 185 km wide, extending more than 100 km from the northern tip of Ellef Ringnes Island. At each point water thickness was measured with an echo sounder or by seismic means.

The free-air anomaly map showed a major positive anomaly centred about 140 km off the continental shelf at the western edge of the area surveyed. The feature was terminated abruptly to the east. Weber suggests that this might be due to a thinning of the crust at the margin of the basin. This effect had been found at a number of places on the Atlantic seaboard of North America, and would suggest that the Arctic was a true ocean. Alternatively the effect could be attributed to a large intrusion of basic rock. The data, and the extent of the survey at that time, were too limited to permit a conclusion.

A.W.J. Berkhout[91] discusses the gravity field over a large area of the District of Franklin. The surveys, which covered Prince of Wales and Somerset islands and the northern part of Baffin Island, were made in 1962 by Weber, and in 1965 and 1966 by Berkhout and Sobczak. The area had been intensively surveyed geologically in Operation Franklin, a major project of the Geological Survey, so that Berkhout had good geological data with which to correlate the gravity anomalies.

When the regional gravity trend had been estimated and allowed for, some interesting features remained. A series of negative anomalies across the northern half of the survey area could be correlated with outcrops of Proterozoic metasediments and could be used to extend these formations under adjacent Lower Palaeozoic cover. It was suggested that these lows represented the remnants of a series, probably connected, of Upper Proterozoic sedimentary basins. South of this area there was evidence of a series of northerly-trending fault blocks along which basement uplift had occurred in Proterozoic times.

Sobczak and Weber[92] discuss the crustal structure of the Queen Elizabeth Islands and the Polar Continental margin in the light of all available geophysical data – gravity, magnetic, hydrographic and seismic. They also make use of a major collection of density values accumulated throughout the years of the Polar Continental Shelf Project[93].

Throughout the area, positive Bouguer anomalies are found to correlate with fold belts and ocean basins, negative ones with old sedimentary basins. The most remarkable feature of the study is a series of elliptically-shaped free-air anomalies overlying the continental margin and paralleling the continental break. These are from 150 to 300 km in length, 120 km in width, and have amplitudes of more than 100 mGal. One of these has already been met in Weber's work described earlier[90]. The cause is here ascribed to the transition from continental to oceanic structure, with a thinning of the crust, and the presence of a thick sedimentary layer. This was one of the possible explanations proposed by Weber in the earlier paper.

The pattern is now recognized as diagnostic of a passive continental margin. The explanation proposed by Sobczak and Weber is not sufficient to explain the size of the anomalies, and there is still not agreement about its source.

The free-air anomalies also demonstrate that the region is in approximate isostatic equilibrium.

North Pole Expeditions

These expeditions, in 1967 and 1969, were made possible through the logistic support of the Polar Continental Shelf Project[94]. They had two aims, one scientific, one practical. The scientific aim was to compare observed values of gravity with those given by the International Spheroid; the practical aim was to develop techniques for using the sea ice as a platform for geophysical measurements.

At the time of the first expedition, radio navigation aids were not available in the polar regions and satellite receivers had not yet become commercially available. The expedition would test a technique of precise polar navigation developed by the Control Data Corporation of Minneapolis.

The party consisted of seven members under the direction of J.R. Weber. On May 6 it was landed on the ice about 40 km from the Pole by a Bristol Freighter aircraft. It was hoped that the drift of the ice would carry the station over the Pole; it was intended to measure gravity and deflections of the vertical at regular intervals as the drift progressed.

Measurements began immediately to establish the position of the station and to track its drift. The method involved the continuous viewing of a number of celestial targets and determining ice drift and atmospheric refraction by computer. Since the computers of the day could not be taken to the Pole, the data had to be taken to the computer. Zenith angles of the sun and of a number of stars were observed more or less continuously and were transmitted, via amateur radio operators in Alert and Ottawa, to a computer in Minneapolis. The computed positions, the rate and direction of the ice drift and a prediction of probable ice movement during the next few hours, were relayed back to the ice station by the same radio link.

Supply problems made it necessary to abandon the ice station on May 14; personnel were evacuated to Alert by two single-engine Otter aircraft. The drift of the station was monitored throughout the entire period. Fog crystals in the air limited the visibility of the stars, but observations of the sun were possible except for a 28-hour period of overcast. It was estimated that positions were accurate to ± 200 m when only the sun was used as a target, ± 50 m when stars and planets were included. Drift speeds varied from 300 to 700 m/hr.

The drift path was not, as hoped, over the pole, but in a circular arc around it. Gravity observations were taken at regular intervals but no measurements of the deflection of the vertical were made.

By the time of the second expedition in 1969 commercial satellite receivers were available. The expedition was landed on a floe, some 35 km to the west of the pole on April 12 and drifted with it until May 3. Theodolite observations of stars and satellite position fixes were obtained continuously except for a period of four days when a storm blew the station 40 km to the southwest and made observations impossible. Two acoustic transponders were placed on the sea bottom and a transducer at the camp measured drift relative to them once a minute.

The party visits the pole by Bristol Freighter. From the left: Mike Perlman, Ivy Iverson, Leif Lundgaars, Axel Geiger, Hans Weber, Bob Lillestrand, Neil Anderson. Twenty-one years later Hans' son Richard stood on the same spot, having skiied there as a member of the Canada-Russian *Polar Bridge* team.

Using a Twin Otter aircraft, spot gravity and depth measurements were made at ten points between the station and Alert, and at five points over the Lomonosov Ridge. The positions were accurately located by observations of the sun. Forty points were observed from the ice station as it drifted.

Three drift paths had been established, one astronomically, one by means of the satellite receiver, and one with respect to the sea-bottom acoustic transponders. The astronomical determinations were influenced by the deflection of the vertical, the satellite observations were not. By comparing the two paths Lillestrand and Weber[95] were able to determine the deflection of the vertical as the drift progressed. The direction of the deflection remained constant within the limits of error, but the amount increased during the drift, being nearly twice as much at the end as it had been at the beginning. During the latter part of the drift the floe was approaching the Lomonosov Ridge at right angles. The increased deflection of the vertical could be explained if the Ridge consisted of sedimentary rocks; the gravity observations, although limited in number, were consistent with this interpretation. The hypothesis supported one proposed by J.T. Wilson some years earlier, that the Lomonosov Ridge is a continental fragment; it was substantiated ten years later by gravity and seismic observations made during the LOREX 79 Expedition.

Local Structures

Gravity data may be used to define purely local structures. The investigation of meteorite craters, discussed in an earlier chapter, is one important application. We shall here describe some other interesting studies.

Aid to Upper Mantle Drilling Projects

As part of the Upper Mantle Project, and as a guide to the deep-drilling program of the Geological Survey, a gravity survey was conducted in the Mount Albert region of Gaspé[96] in 1961. In the following year, and for the same purpose, a gravity survey was made over the Muskox extrusive in the District of Mackenzie[97]. A description of this work was eventually included in a general discussion by Hornal of the gravity anomaly field in the Coppermine area[98].

Arctic Piercement Domes

Gravity surveys in the Sverdrup Basin and Axel Heiberg Island in the early 1960s included within their area a number of piercement domes. These domes had been studied extensively by geologists, and in 1969 A. Spector and R.W. Hornal reconsidered the gravity data[99]. Treating each dome as a right-circular cylinder, and using a computer program developed by Nagy[100], they were able to account for the observed anomalies in terms of a high-density anhydrite zone overlying a low-density zone of gypsum and/or rock salt. They were not, however, able to establish the thickness of the zones precisely.

The Round Lake Batholith

When the gravity map for the mining areas of northeastern Ontario and north-western Quebec was published[101] there were a number of interesting features on it, one of which was a roughly circular negative anomaly over a batholith a short distance south-east of Kirkland Lake. This anomaly was discussed briefly in the Map Series article accompanying the maps, and in more detail in a separate paper by Gibb and J. van Boeckel[102]. While the general shape of the anomaly could be explained in terms of a density difference between the granitic batholith and the surrounding volcanic rocks, the variations from the general contours were harder to account for. Two possible explanations were advanced, faulting within the batholith or density variations within it. In their analysis the authors made use of a program developed by Nagy, in which the assumed model (the batholith) is made up of a large number of prismatic blocks[103].

The Darnley Bay Anomaly

The largest isolated gravity anomaly discovered in Canada up to that time appeared in a map of the Mackenzie Basin and Beaufort Sea published in 1970[104]. The anomaly was centred over Darnley Bay, on the Arctic coast about 400 km east of the Mackenzie Delta; it had a circular shape and a Bouguer anomaly value rising 130 mGal above the background. The original authors, Hornal *et al*[104] obtained a satisfactory fit to the observed anomaly curve by postulating a basic intrusive in the form of an inverted cone, but they recognized that the solution was not unique. Later, Stacey[105] showed that an upright but truncated cone with a central core of more dense material could account equally well for the anomaly. He concluded that the gravity measurements could only define the body as being a basic intrusion of conical shape and that other geophysical measurements would be needed to define it precisely.

Magnetic data became available shortly afterwards from airborne magnetometer flights made in 1969 and 1970. They showed a large magnetic anomaly, somewhat more elongated than the gravity one. R.P. Riddihough and G.V. Haines[106] found that bodies of uniform magnetization having the dimensions of those proposed in the gravity solution produced too broad a magnetic anomaly; the source of the magnetic anomaly was smaller, more elongated, and probably lay within the postulated basic intrusion. They were able to define its general properties.

The fact that the magnetic anomaly did not coincide with the gravity one is not surprising. The temperature of crystallization of these intrusions is over 1000°C, well above the Curie point, and they undoubtedly affect the country rocks. It would be remarkable if the two anomalies did coincide.

Sudbury Area

The first gravity study of the Sudbury Basin and its vicinity was made at the request of the Committee on Research in the Geological Sciences. Observations began in 1948 and continued through 1951, totalling about 500; 160 rock samples were collected for density determinations. It was found that the Bouguer anomalies were largely controlled by density variations in the surface rocks. Because the structure was known in considerable detail from mining operations, it was possible to compute a theoretical anomaly curve across the basin; the observed curve agreed with this reasonably well. It did not however support the then accepted hypothesis of a broad funnel-shaped fissure beneath the basin that had acted as a conduit for the nickel eruptive[107]. The chapter on meteorite craters provides some idea of why this should be so.

Additional data gradually accumulated. The area was included in a survey by Gibb and McConnell in 1963- 1965[108]. Mining companies in the area made extensive surveys on their own and supplied the data to the Observatory. In 1969 J. Popelar combined all the available data, surveyed an additional eight profiles across the feature and made additional observations to fill in the blank areas. The results were issued in 1971 in the Gravity Map Series[109], in which the accompanying map, based on 2,302 observations, was contoured at 2 mgal intervals. Popelar was able to correlate many of the anomalies with geological features, but "the Sudbury structure itself cannot be related to a simple gravity anomaly".

Glaciological Investigations

Gravity measurements can be useful in contouring the rock surface beneath glaciers or snow fields.

The method is theoretically straightforward. If one assumes that the bedrock is at a uniform elevation under the ice, then the difference in the Bouguer anomaly at a station on the ice, as compared with that at a point on the adjacent bedrock, is entirely due to the infinite disk of ice assumed in computing the anomaly. When this effect is removed there should be no difference in the Bouguer anomalies between the two positions. When there is a difference it is due to differences in the regional gravity field, or to differences in the elevation of the bedrock. If the variation in the regional field is known, the difference in elevation, that is the depth to bedrock, may be determined. One must, of course, apply a terrain connection at each observation point.

All the surveys to be reviewed were made in areas where the regional gravity pattern was unknown at the time. It had to be estimated by supposing it to vary uniformly between a number of points measured off the ice cap or, what is equivalent, at a number of points where the depth to bedrock was known from drilling or from seismic measurements. Variation in density in the rocks beneath the ice present a possible source of error. In a large ice sheet this is almost impossible to assess.

The first Observatory study of an icecap was made by C.A. Littlewood on the Barnes Ice cap of Baffin Island during the summer of 1950[110]. He was attached to a party of the Arctic Institute. A number of traverses were made over the ice cap, or parts of it. The usual assumptions about regional gravity were made, but Littlewood had only the most general information on rock densities and seems not to have made terrain corrections. He produced profiles of the ice thickness and of the bedrock; the maximum thickness of the ice was 1500 feet.

In 1958 J.R. Weber and H. Sandstrom, graduate students respectively at the Universities of Alberta and Toronto, and K.C. Arnold, a geographer from Cambridge University, made an important study of the Gilman glacier and its adjoining ice cap on northern Ellesmere Island[111]. Using a refraction seismograph, they measured ice velocity profiles at several points, for both longitudinal and transverse waves. In addition, twelve bedrock profiles were established by reflection seismograph. Then, using a Worden gravimeter lent by the Observatory, they made gravity observations at more than 200 points over the area of the seismic survey. Using ice thickness as determined by the seismic work to define the regional anomaly, a map of the bedrock terrain and ice thickness was prepared, based on the gravity measurements alone.

In connection with the survey the group made an historic trip by dog team, across the northern tip of Ellesmere Island. The traverse took them from the Arctic Ocean at Clements Markham Inlet, across the United States range to the Gilman glacier. They made gravity observations as they went. After the season's work on the glacier the traverse was continued, using the dogs as pack dogs, to Lake Hazen and along the Ruggles river to Chandler Fiord on the Robson Channel. This traverse still, in 1990, provides the only gravity data for that part of northern Ellesmere Island.

There was of course no survey of the area. On the first leg of the trip, position and elevation were determined by subtense bar triangulation and by taking simultaneous vertical angles at distances between one and two km. On the second leg, elevations were determined by a continuous level survey with observation points at intervals of 50 to 100 m. When the party reached the Chandler Fiord the sea level difference at the two ends of the traverse, that is the survey error in the 120 km traverse, was only 70 cm!

Weber joined the Observatory staff in 1960 where, as we have seen, he continued to be very much involved in arctic studies.

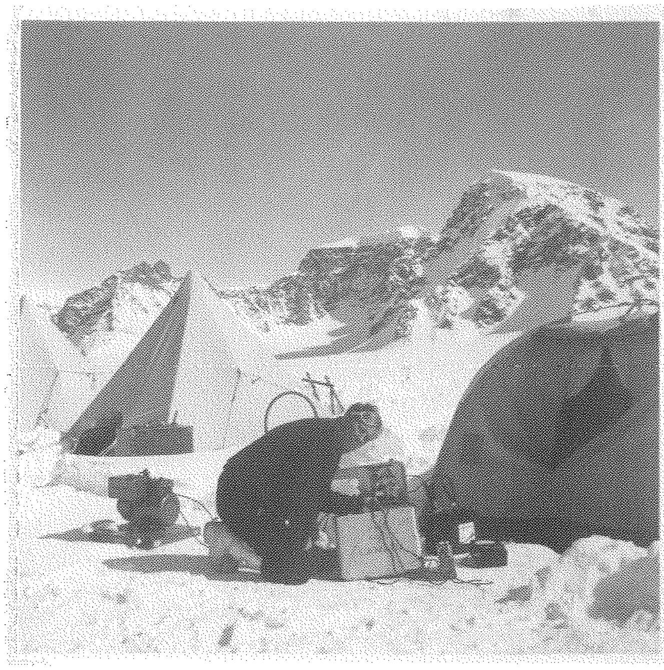
In 1960, as part of the early Polar Continental Shelf Project, R W. Hornal[112] occupied 156 stations on the Meighen Island Ice Cap. Again ice thickness was known at a number of points from seismic measurements and, using these to establish the regional gravity pattern, he was able to determine ice thicknesses over the ice cap. They varied from less than 100 to about 500 feet, in good agreement with seismic depth determinations.

In 1963 A. Spector[113] made a survey of four ice caps on Melville Island and reported on them as part of a Master's thesis at the University of Toronto. As in the other investigations we have discussed, the regional anomaly variation was estimated from off-cap observations, but he collected a number of rock samples from which to estimate an average density, and was able to make terrain corrections out to distances of 18.8 km.

In 1962 the Observatory initiated a long term program to study changes in the thickness and movement of the Penny Ice Cap, on Baffin Island. An array of 14 aluminum poles was permanently drilled into the ice across the crest of the ice cap, and the position and elevation of these were surveyed relative to four base points on solid rock. It was proposed to monitor changes in position and elevation of these poles at intervals of a few years.

Distance from the poles to the bedrock base-points ranged from 15 to 23 km. Rather than re-survey the pole positions on each return trip, an expensive undertaking, it was proposed to use gravity observations to monitor elevation changes. Gravity values, tied to the outcrops, were established for each of the pole positions. Any subsequent change in gravity would be entirely due to change in elevation. In 1965, when the first of the periodic re-surveys was made, the elevation changes of the poles were measured by both gravimeter and by re-surveying. The two values agreed to within 0.08 m.

In 1965 depths were measured along a profile about 15 km long by four different methods – seismic reflection, gravity, electrical resistivity and radar sounding. The radar sounding was done using a radar altimeter lent by the RCAF. It was pulled at a steady rate behind a motor toboggan and provided a continuous bedrock profile, although there was some difficulty of interpre- tation. As shown in the figure on the following page, the four methods agreed remarkably, at depths ranging from 200 to 800 meters[114]. Gravity differences were measured again in 1971.



Hans Weber measures gravity at the base camp on the Penny Ice Cap.

The radar antenna and towing motor toboggan.

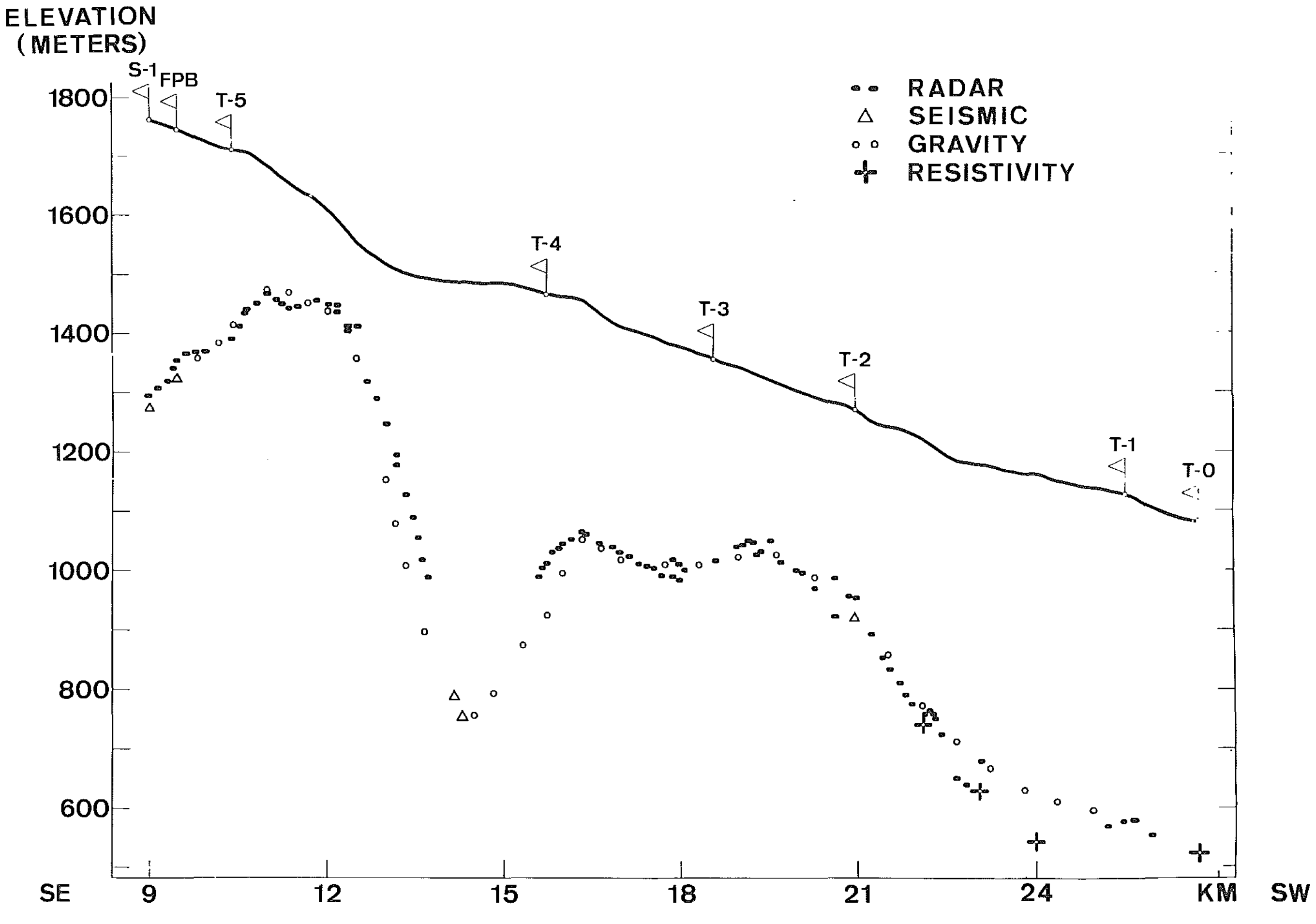
The long-term measurements of this ice cap has become a matter of great importance in the 1990s, with the growing concern about global warming. The program has now been extended to include four additional ice caps as part of a Global Change Program; the Geological Survey of Greenland is considering a similar program on the Greenland Ice Cap.

STUDIES OF ISOSTASY

The magnitude of isostatic anomalies will depend on the model, Airy or Pratt/Hayford/Bowie, selected, on the depth of compensation, and on the assumed density distribution. It is usually possible by adroit selection of parameters to reduce the mean isostatic anomalies to low values, and this has been taken as confirmation of the validity of the theory of isostasy. Where it could not be accomplished, the residual anomalies were attributed to uncompensated structures. This approach was taken in much of the early work of the Observatory. For example, as shown in Part I, Miller was able to fit his pendulum observations in the Mackenzie basin to a Hayford model with a depth of compensation of 91 km[115], and the foothills and mountain areas of Alberta and British Columbia could be compensated at depths ranging from 85 to 114 km[116].

In his study of the gravity field in central Quebec, already referred to[15], Innes computed the isostatic anomalies along a line reaching northwest from the St. Lawrence river at Baie Comeau, crossing the Grenville province. The Hayford anomaly, with a depth of compensation of 114 km, had a nearly zero average at sea level and over much of the Grenville province but was very strongly negative at its northern boundary. Innes attributed this to the roots of mountains, still present although the mountains themselves have long since eroded. Later, as part of a survey of the Gaspé Peninsula, Tanner and Uffen[117] extended this line to the southeast, across the Shickshock mountains. Here they found a positive anomaly of nearly 40 mGal. They had to conclude that the mountains were not supported isostatically. They suggested two possibilities: either the mountains were sustained by the strength of the crust or they were due to a horst intrusion of Precambrian gneisses.

Garland and Tanner, applying an Airy model[18] to their observations in the western Cordillera, found that the mountains were compensated, although areas of strong negative anomaly still existed. These were attributed to extensive areas of granite extending into the crust.



Ice profile and depth to bedrock on the Penny Ice Cap, measured by a variety of methods. From Journal of Glaciology, 9, 52.

In his PhD Thesis, in which he analyzed the gravity field in northern Ontario and Manitoba, Innes[59] recognized a tendency for the isostatic anomalies to become more negative in the direction of Hudson Bay. It was not easy to establish this in the presence of other sources of anomaly. On the supposition that the other sources would average out, he defined a number of concentric rings, centred at a point in the Bay, and averaged the anomalies throughout each ring. The means did indeed define an anomaly of -30 mGal which he attributed to incomplete isostatic adjustment following the withdrawal of Pleistocene glaciation. Later, when the density of observation in the Hudson Bay lowlands had been greatly increased, and when Hudson Bay itself had been surveyed by sea-bottom gravimeters, this isostatic anomaly was confirmed[118,119]. It suggests that the area has yet to rise 180 m before isostatic equilibrium is established; such a rise would essentially eliminate both Hudson and James Bays as physiographic features.

A Post-Doctorate Fellow, Yasuo Shimazu, brought a new method of analyzing gravity data to Canada. In this method, developed by Professor C. Tsuboi, it is assumed that variations in gravity are caused by anomalous mass distributions at the base of the crust; Shimazu applied this analysis to all the gravity data available in Canada in 1961[120]. The method assumes densities for the crust and mantle and seeks average crustal thicknesses which minimize the function

$$\Sigma\{\text{Isostatic Gravity Anomaly}\}$$

The results, crustal thicknesses of 36 and 48 km in the Canadian Shield and the southern Cordillera respectively, are in good agreement with values found by seismic methods. Both areas were found to be isostatically over-compensated, the central Prairie regions somewhat under-compensated. Values of the deflection of the vertical and fluctuations of the geoid were obtained.

The assumption that mass distributions at the base of the crust are responsible for gravity observations is obviously an over-simplification, as is any other assumption about structure at depth. The method has the advantage that large quantities of data may be computer analyzed; when the method is applied on a regional and continental scale, local anomalies tend to be averaged out. Despite the promising results of Shimazu's work the method was not used after his departure.

R.I. Walcott published an important series of papers that considered the lithosphere and its relation to loading. In a first paper[121], extending the work of Innes, he showed that central Canada, which had been covered by the Laurentide Ice Sheet, is overcompensated, that a substantial amount of uplift has still to occur, and that this will require something between 10 and 20 thousand years. He was more interested in the shape of the depression and in how it related to the boundary of the original ice sheet. This requires a knowledge of the flexure parameter of the crust, a parameter that governs the wave-length and amplitude of a depression resulting from a given load; it involves the thickness, density and elastic properties of the crust.

To obtain the flexure parameter, Walcott turned to the Caribou Mountains in Alberta, in which a large mesa-like erosion remenant rests on flat-lying sediments which had been thoroughly investigated by drilling. He found that a particular formation dipped under the mountain with a uniform dip to the southwest of 0.44%; it showed no effect from the mountain. Theoretical deflections were calculated for various values of the flexure parameter. At small values of the parameter the deflections were substantial and would certainly modify the regional dip; by increasing the values of the parameter until its effect just became unnoticeable, Walcott was able to establish a maximum value. With this value he returned to a reconsideration of the Laurentide residuals.

Three effects are forecast: first, a "forebulge", centred 280 km beyond the ice boundary, caused by lateral extrusion of mantle material from beneath the crust underlying the ice sheet; second,elastic depression of the crust, both under the ice load and a few hundred km beyond its edge, superimposed on regional warping caused by the movement of magma; third; a maximum stress difference, several hundred km behind the ice margin, along which faulting might occur. Possible correlations with tide gauge levels and the positions of raised beaches are suggested.

Walcott next considered[122] the effect on the lithosphere of loading by sediments. Where sedimentary basins are broad, differential vertical movements can result, causing an amplification of the original structure and the growth of an arch. Where sedimentary basins are narrower, faulting and related uplift can result. He illustrated both cases with reference to Canadian topographic features, and, in a third paper[123], used his findings to explain the lithospheric structure under the Hawaiian Ridge in terms of loading by volcanic rocks. Finally, combining all his data, he determined the thickness, viscosity and flexural rigidity of the lithosphere in a variety of geological regions[124].

These papers by Walcott were a major contribution to the understanding of the effects of crustal loading. In 1965 a new program was begun to study possible vertical movements of the crust as large lakes are formed behind hydro-electric dams. Detailed gravity measurements in the environs of the expected lakes were to be made before and after the dam closures. Preclosure measurements were made in 1965 at the Manicouagan dam site, in 1966 and 1967 in connection with a large dam on the Saskatchewan River near Elbow, Saskatchewan, and in 1967 at the W.A.C. Bennett dam on the Peace River, but no post-closure measurements were available during the period of our interest.

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Morris Innes retired in October, 1972. The day of the ceremonies coincided with a meeting of the Associate Committee on Geodesy and Geophysics, and many of the senior geophysicists of Canada were able to attend. Messages were read from geophysicists from many countries, people who had worked with Innes or had been helped by his generous cooperation. It was a warm and happy occasion.

The most impressive event of the afternoon was the display of two maps, one showing the handful of gravity observations that existed when Innes joined the staff, the other displaying the more than 110,000 observations available at his retirement! He hadn't made all these observations, although he had made many of them, but he had selected, trained, encouraged, driven and supported the people who had made them. And he had found the time and energy for a parallel career, in his pioneering work with meteorite craters.

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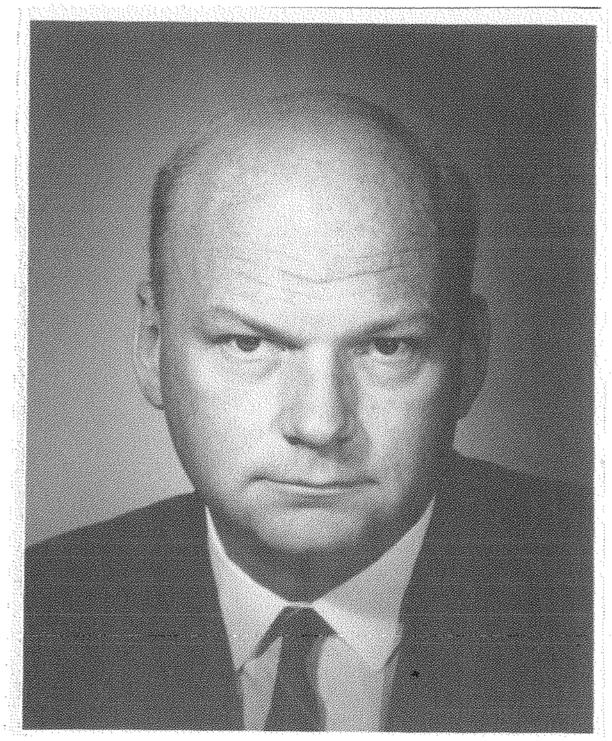
R.G. Madill, Chief of the Geomagnetic Division, 1940-1963.



Dr. P.H. Serson, Chief of the Geomagnetic Division, 1963-1981.

XII – GEOMAGNETISM

INTRODUCTION

The main geomagnetic field of the earth may be derived from a potential function and expressed in terms of spherical harmonics. It is of internal origin. The fit of theory to observation is better the higher the order of harmonics used. The first two terms provide a first approximation – a dipole passing through the centre of the earth inclined to the axis of rotation at an angle of 11°. The corresponding poles are called the "geomagnetic" poles. If the dipole field portrayed the main geomagnetic field accurately, the total magnetic force over these geomagnetic poles would be vertical. This does not happen; the "north magnetic dip pole" is some 550 miles to the southwest. The difference between the dipole field and the actual main field is called the "non-dipole field".

The geomagnetic dipole field is used to define a "geomagnetic coordinate system", useful in theoretical studies. The north geomagnetic pole is currently at 78.8°N, 70.9°W, the south geomagnetic pole at 78.8°S, 109.1°E. The zero magnetic meridian passes through these geomagnetic poles and the south geographic pole. Magnetic observatories normally give their position in both geographic and geomagnetic coordinates.

What is the source of the internal field, and why does it change, slowly, with time? It is due to motions within the fluid, electrically-conducting, earth's core. As we shall see later, the polarity of the field has changed repeatedly in geological time; the model must also explain this. All of the proposed models require some driving energy, the source of which is not established. For our purposes it suffices to know that there is a large internal field which varies slowly with time.

There are variable fields superimposed on this steady state field. They range from regular diurnal changes to violent disturbances known as magnetic storms. To study the former, we must eliminate the effects of the latter. This is most readily done by limiting analysis to days when there are no storms, and by including data from many observatories. Large magnetic storms normally affect stations all over the world, although not to the same extent. The International Association of Geomagnetism and Aeronomy (IAGA), after receiving reports from these observatories, establishes lists of the five most quiet days and the five most disturbed days in each month. These are called, for simplicity, "quiet" days and "disturbed" days; data related to these days are indicated by subscripts q and d respectively.

The quiet day magnetograms for any one station show a fairly regular diurnal variation, but the amplitude of the variation depends on the magnetic latitude of the observatory. For stations in the same magnetic latitude the records are quite similar when plotted against local time. Harmonic analysis which includes these diurnal variation shows that the field causing them is almost entirely external to the earth. Two periods are involved, a solar one of 24 hours and a slightly longer lunar one; harmonics of these periods are also significant and measurable. A small part of the diurnal variations derive from electrical currents within the earth, induced there by the external field.

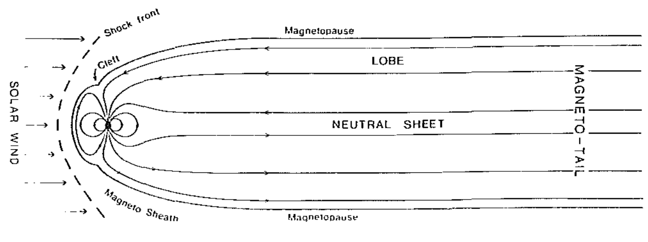
The daily variation on a world-wide basis is explained in terms of "equivalent currents", flowing in a conducting layer enveloping the earth. The current flows consist of four sets of ovals, two in the northern hemisphere and two in the southern; in each case one lies in the daylight section, the other over the night area. The daily movement of an observatory from one current system to the other accounts for the observed variation[1].

Magnetic storms are caused by currents flowing in this conducting layer. They are most prominent in the auroral zones, from which they spread to lower latitudes. The currents are caused by a flow of plasma expelled from the sun by the pressure gradient in its visible outer layer. This plasma flow is known as the solar wind. It is a property of plasmas that they carry with them the magnetic field of their source, so that the earth is being bombarded not only by the plasma but by its magnetic field. Since the earth has its own, internal, field the incoming field divides, something like water being deflected around a piling in a fast-flowing river. This provides a sort of sheath around the earth and its magnetic field called the magnetosphere. The lines of force in the earth's field lead the plasma's charged particles into the ionosphere where they maintain its ionization.

The strength of the solar wind varies with the condition of the sun's photosphere, and this depends on the numbers of sunspots. Magnetic storms originate in sunspots; the solar wind is the mechanism which delivers the effect.

When currents circulate in the ionosphere they induce currents in the earth. Part of the diurnal effects are therefore internal. These earth currents can be measured and may be used to study the electrical conductivity of the earth's interior. They are much stronger during magnetic storms and may disrupt telegraph communications and power line transmission and cause corrosion in pipelines.

Auroras are an important consequence of the solar wind. They are caused by ions moving into the upper atmosphere along lines of force in the earth's magnetic field, which direct



Magnetic field lines and structure of the magnetosphere. From W.D.Parkinson "Introduction to Geomagnetism", 251, Elsevier, New York.

them into the vicinity of the magnetic poles. The auroras are not however a maximum at the poles. In the northern hemisphere the greatest frequency, called the "auroral zone" occurs in a narrow, circular band with a radius of about 15°, centred on the geomagnetic pole. Churchill is in the zone, Baker Lake is just north of it, and Alert, Mould Bay and Resolute Bay are well inside it, in what is known as the "polar cap".

Clearly a magnetogram is a complex record of many magnetic fields, with a broad variation of period and amplitude. The table on this page, copied from Parkinson[2] provides an excellent summary. The unit "Nt" (nano tesla) used in the table is the equivalent of one gamma[2]. The gamma is equal to 10^{-5} Gauss, the CGS unit of magnetic intensity.

How is a station operator to convey these complexities to his colleagues? Through annual bulletins, known as *Year Books*, listing hourly values of the magnetic elements, monthly mean values, hourly ranges of the elements and a range of indices established by international agreement. For some purposes even such detailed bulletins are inadequate and the magnetograms themselves are required.

MAGNETIC OBSERVATORIES

Instruments[3,4]

A standard station must be able to determine the true base value of the magnetic field and the variations imposed on this base. Instruments for the former purpose are called absolute magnetometers, those for the latter, variometers.

Three parameters are normally used to define the magnetic field at any point: H, the magnitude of the horizontal component, called the horizontal intensity; D, the azimuth of the horizontal component, measured clockwise from geographical north, and known as the declination; and I, the angle of the total field with the horizontal, regarded as positive if it is inclined downward, and known as the magnetic dip or inclination. Alternatively it can be expressed as three orthogonal components, X measured to the north, Y to the east, and Z vertically downward.

The instruments used in magnetic observatories, prior to the end of WWII, had not changed in principle for many decades. Absolute measurements of D and I depended on observing the position of rest of suitably mounted bar magnets. The absolute value of H could be determined by counteracting the field by passing a measured current through a Helmholtz coil. Variometers consisted of suitably mounted magnets held at right angles to the component to be measured by counterweights or fixed magnets. Change in the magnetic field disturbed this balance and produced a measurable deflection. With these standard instruments, it was possible to measure declination with an accuracy of 1′, the force components to the nearest gamma.

In 1947 P.H. Serson and W.L.W. Hannaford[5] developed a portable magnetometer of a new design, using a fluxgate as the magnetic detector. The instrument was intended for field work at high latitudes, where the weak horizontal component and frequent large disturbances prevented accurate measurements

Table II – The various magnetic fields and their properties. From W.D. Parkinson, "Introduction to Geomagnetism", 6, Elsevier, New York.

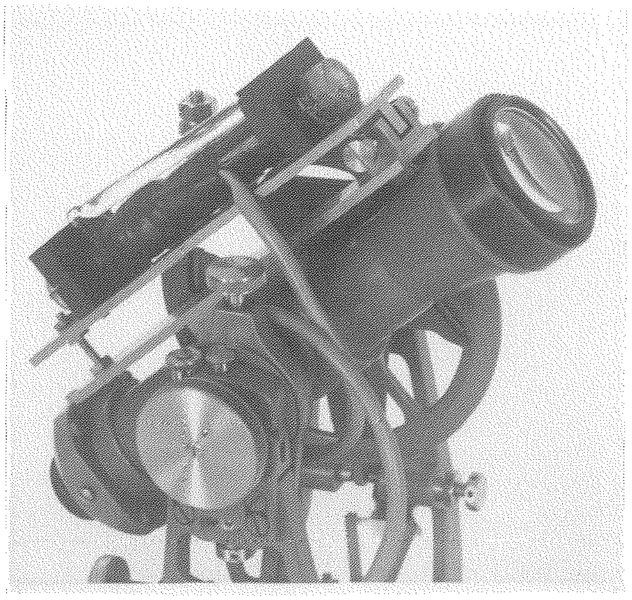
Constituent field	Location of source	Intensity (maximum)	Morphology	Time variation	Measured by	
1 Main field	outer core	50,000 nT (70,000 nT)	mainly dipole	secular variation order of 1000 yrs; reversals order of 10^6 yrs	regional surveys (aircraft, ships, satellites or ground obs.)	controls all other fields directly or indirectly; used in navigation
2 Local field	crust above Curie point geotherm	mean 100 nT (as high as 10^5 nT)	very irregular, wave lengths as short as 1 m	none	local surveys (surface or airborne)	used for geophysical exploration and ocean floor spreading rates
3 Regular storm field	magnetosphere	150 nT (500 nT)	approximately uniform external field	4 to 10 hour; recovery takes 2 to 3 days	observatory magnetographs	monitors solar activity
4 Irregular storm field & substorms	ionosphere and magnetosphere	100 nT (200 nT in auroral zones)	global, but more intense near auroral zones	periods of 5 to 100 minutes	observatory and temporary magnetographs	ditto
5 Diurnal variation	ionosphere	50 nT (200 nT at equator)	global; mainly P_2^1 and P_3^2 harmonics	periodic 24, 12, and 8 hour periods	observatory magnetographs	indicates ionospheric tidal winds
6 Pulsations	magnetosphere	few nT (100 nT for P_g)	quasi-global, more intense near auroral zones	quasi-periodic 1 to 300 sec	rapid-run and induction magnetographs	indicate resonances in magnetosphere
7 Induced fields	crust upper mantle and oceans	about $\frac{1}{2}$ of above four fields	generally global but irregular in places	same as above four fields	observatory and temporary magnetographs	indicate conductivity distribution in crust and mantle

with the classical instruments employing suspended magnets. The fluxgate principle was not new; it had had several military applications during the Second World War.

In the portable magnetometer^5 the fluxgate detector is mounted on the telescope of a non-magnetic theodolite, with its sensitive axis parallel to the optical axis of the telescope. The detector contains two strips of Mumetal, a ferromagnetic alloy of high permeability, mounted side by side; each strip is surrounded by its own primary coil of fine wire, with the two primary coils connected in opposition. This whole assembly is surrounded by a common secondary coil in the shape of a long cylinder.

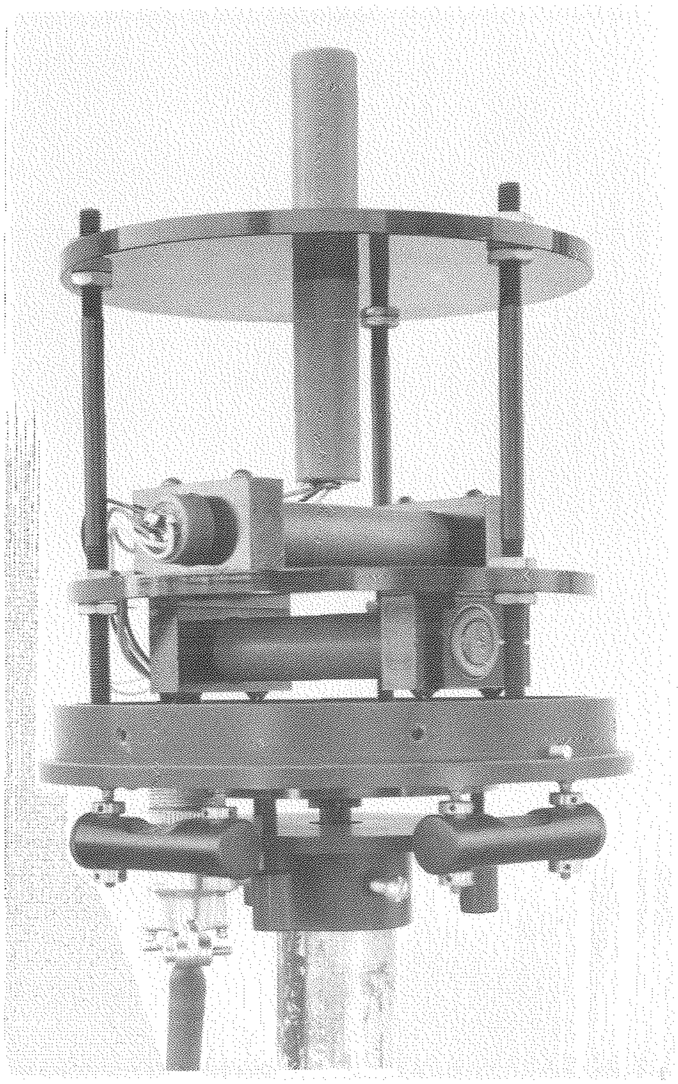
An alternating current of audio frequency passes continuously through the primary coils, subjecting the cores to an alternating magnetic field strong enough to saturate the cores about one third of the time. If the frequency of the exciting current is 500 Hz, the cores are saturated 1000 times per second. The two cores together may be regarded as a single rod of ferromagnetic material whose permeability alternates between a high value and a low value 1000 times per second. If there is a component of the earth's magnetic field in the direction of the axis, the resulting magnetic flux through the secondary winding is modulated at 1000 Hz, and a voltage is induced in the secondary at this frequency, with an amplitude proportional to the magnetic component and phase depending on its sense. This signal passes through a narrow-band amplifier to a phase-sensitive detector and is displayed by a centre-zero meter.

In the observing procedure, the fluxgate is used only as a null detector; its actual sensitivity is unimportant provided it is sufficiently large. To determine the declination, the telescope is set horizontally and is rotated about the vertical until a null is indicated, with the telescope pointing magnetic east. Other nulls are found with the telescope pointing magnetic



The fluxgate detector mounted on the theodolite telescope. From Canadian Journal of Technology, 34, 233, 1956.

Observing with the portable fluxgate magnetometer.



The head of the three-component recording magnetometer, with cover removed. From Canadian Journal of Physics, 35, 1391, 1957.

west, and with the telescope inverted to remove any errors due to misalignment of the fluxgate axis with the telescope's optical axis. The direction of magnetic north is at right angles to the means of these directions, and can be compared with the direction of true north, obtained by astronomical observations with the theodolite, to determine the magnetic declination. The magnetic dip or inclination is determined by tilting the telescope in the vertical plane containing magnetic north to obtain a null, and reading the vertical circle. The fluxgate axis is now set in the direction of the total magnetic vector and a direct current passing through the solenoid surrounding the fluxgate is adjusted until it produces a magnetic field equal and opposite to the geomagnetic total field, as indicated by a null reading. Knowing the constant of the solenoid, one can calculate the total geomagnetic intensity.

In the field, an experienced operator can complete all the operations at a point in 15 minutes, with an accuracy of a few tenths of a minute of arc in D and I, and of 10 to 50 gammas in absolute value. This method of measuring total intensity, which involved standard cells and sensitive galvanometers more suitable for the laboratory than for field conditions, became obsolete in 1957 with the introduction of the portable proton precession magnetometers, and the fluxgate-theodolite combination is now used only for measuring the angles D and I.

In 1957 Serson[6] developed an electrical recording magnetometer using the fluxgate principle. Three elements, mounted in a single head, measure D, H, and Z, or alternatively, X, Y, and Z. The head could be positioned at some distance from the recorder or other extraneous magnetic influences. The electronics of the system provide a base line for the records and accurately calibrated scales for the three components.

This instrument was so successful in its trials that commercial production was turned over to Canadian Applied Research Limited, who contributed much to the final mechanical design. The instruments became stand-by variometers at all Canadian magnetic observatories. They could operate at standard- and low-gain, and so supplied data when the standard variometers went off scale during magnetic storms.

F. Primdahl, a postdoctoral fellow from the Danish Meteorological Institute, produced a remarkable series of papers during his term in Ottawa in the late 1960s. Recording fluxgate magnetometers were not proving stable enough for primary equipment at standard observatories. There were unexplained temperature effects and occasional sudden shifts in output level. His project was to produce a fluxgate magnetometer with the required long-term stability. He first made a thorough review of the literature[7] and of the theory[8] of fluxgate magnetometers. Then, seeking to overcome all sources of instability, particularly temperature sensitivity[9], he produced an instrument which, in short term testing against a proton precession magnetometer, met the design specifications[10]. A test over a nine-month period[11] showed a base-line drift of only 35 gammas.

Primdahl joined Serson in producing, for IAGA, a review of magnetometer developments from 1950 onwards[12]. The review took the form of a bibliography of the more significant papers.

The third instrument developed during this period, the proton precession magnetometer, depended on a fundamental principle of nuclear physics. Most atomic nuclei possess a magnetic moment and angular momentum. When they are subjected to a strong magnetic field the torque tends to align the magnetic moment parallel to the field. However, because of its angular momentum, it does not move immediately into line with the field, but precesses about the field direction. The frequency of this precession is related to the strength of the magnetic field.

In the proton precession magnetometer the protons in a sample of water are subjected to a polarizing magnetic field at right angles to that of the earth. Then the field is switched off; the protons, which had been aligned to the imposed field, precess about the direction of the earth's field with a precession frequency directly proportional to the geomagnetic total field strength. The proton magnetometer measures this frequency by suitable electronics and determines the field very accurately.

The proton precession magnetometer became the standard of absolute field measurement at all Canadian observatories.

The Expanding Canadian Observatory Network

In 1948, when our story resumes from Part I, only Agincourt and Meanook, the two magnetic observatories transferred from the Meteorological Service in 1936, were in operation. They were well equipped with a variety of absolute instruments and each had two sets of variometers; the "Kew pattern", dating from the 1880s, and the "LaCour pattern", of about 1930. Both sets measured H, D, and Z.

Staff transferred with the observatories continued to operate them. W.E. Jackson, Officer-in-Charge at Agincourt, supervised the operation of the stations until his retirement in 1944. W.E. Ross assumed charge at Agincourt, H.E. Cook continued in charge at Meanook. There was little input from the Ottawa staff, whose primary concern was the magnetic survey of Canada.

However, the need for more magnetic observatories, particularly in the Arctic and sub-Arctic, was recognized and plans for expansion were begun in the immediate post-war years of the Stewart administration. By 1948[13] a temporary station had been established at Baker Lake and plans were well advanced for an observatory at Resolute Bay.

There were many problems, logistical and technical, in maintaining stations in the Arctic and the lessons learned with these early stations at Baker Lake and Resolute Bay were invaluable. By the time of the IGY the Division was able to install new stations, and to update existing ones to modern standards. The sequence of installations, and closures was as follows:

Yellowknife – operated from July 1957 to August 1958;

Victoria – opened July, 1957;

Fort Churchill – opened July, 1957, by the Defence Research Board, as a variometer station; upgraded to Observatory status in 1968;

Alert – opened October, 1961;

Mould Bay – opened August, 1962;

Great Whale River – opened in January, 1965, as a "conjugate point" to Byrd, in Antarctica; publication of magnetic data began in January, 1967;

Agincourt – closed March 31, 1969, after 71 years of operation;

Ottawa – opened July, 1968, as a replacement for Agincourt;

St. John's – opened August, 1968.

All stations had essentially the same instrumentation: Ruska or Askania variometers, with a three-component fluxgate magnetometer as standby, and a portable fluxgate magnetometer and a nuclear proton precession magnetometer as absolute instruments.

The Victoria Geomagnetic Observatory was established in response to the IGY. Bernard Caner was recruited from the University of Alberta as its Officer-in-Charge; he was joined in 1960 by D.R. Auld. With seismologists W.G. Milne and W.R.H. White they formed a small nucleus which by 1968 grew into an independent "Victoria Geophysical Observatory". They were established in Observatory House, vacated by DAO staff after Petrie's death

Between 1953 and 1955[14] a detailed survey was made of the environs of the Meanook Observatory to provide a calibration range for the airborne magnetometer. The survey covered an area of 15 miles radius. At the same time 530 additional acres of land surrounding the station were purchased to protect the station from encroachment of electromagnetic disturbances, and to provide a range for electromagnetic induction experiments.

The closing of the Agincourt Magnetic Observatory, and the development of the Ottawa Magnetic Laboratory was described in Chapter VI.

Observatory Year Books

The data from each station were published as "Year Books", the form of which varied somewhat from station to station and from time to time. Generally they included the mean hourly values of H, D and Z or, equivalently, of X, Y and Z. In addition, an attempt was made to describe the appearance of the magnetograms. This was done in a variety of ways, by listing monthly mean values for all days, and for quiet and disturbed days, by listing hourly ranges for the three components, by assigning a variety of magnetic character indices. In addition to published values, information was forwarded directly to IAGA on various indices, and on "sudden commencements, bays and pulsations", features normally easily identifiable on magnetograms. They were not easily identifiable on records of the Arctic observatories; for these the hourly range in the principal horizontal component was supplied instead.

The K index[15], one of those reported, was adopted by IAGA in 1939. It describes the magnetic activity in successive three hour periods, on the most active component. The diurnal effect is removed from the reading and the magnitude of the three-hour average is indicated by a number between zero and nine. Eight numbers thus describe the activity in eight consecutive three-hour periods of the Greenwich mean day. After some experience in the use of the K index, Ottawa personnel decided that it was not useful in high latitudes[16]. In about 1960, they began to publish three-hour range indices for each component, as well as the K indices derived from them, and in addition published values for earlier years for a number of stations[17]. Another index, the Q, was listed for selected days of the IGY at the request of IAGA; it is similar to the K index, but based on a 15-minute period rather than a three-hour one[18].

The Year Books are listed in the references, chronologically by stations, in the order Agincourt[19], Meanook[20], Baker Lake[21], Resolute Bay[22], Yellowknife[23], Fort Churchill[24], Victoria[25], Alert[26], Mould Bay[27], Great Whale River[28], St. John's[29], Ottawa[30]. Examination of the dates of issue will show considerable delay in publication. Much of the delay was in preparing the material. Up until about 1968 the tables were typed on an electric typewriter and reproduced by photo-offset. Most of this formidable task was done by Mrs. Jean Hastie. In about 1968 the operation was transferred to computer. The mean hourly values were scaled and punched on cards; the computer developed and printed the remaining tables which were then printed by photo-offset.

The scaling of magnetograms is a tedious task. In 1962 a device was developed at Victoria[31] to digitize the records. A computer applied the appropriate scale factors to the digitized values, computed hourly and daily means, and printed the results in a form suitable for publication by photo-offset printing. Increasingly sophisticated versions of the reader were produced; they were used to process the Victoria data from 1962 to 1968, at which time a final report on the digitizer was prepared[32].

Direct digital recording of magnetic data was first proposed in the early 1960s. It would have simplified station operation, eliminated the need for digitization and facilitated computer analysis of the output. It was not such a simple matter. In 1962[32] Caner and Whitham report that "their technical complexity inevitably results in lower reliability than can be obtained for photographic variographs." However the Division pursued their development and the first of the new instruments was installed at St. Johns' observatory on a trial basis in December 1969[33]. The trial was successful; today all magnetic observatories in developed countries use digital recording techniques.

MAPPING THE MAIN MAGNETIC FIELD

Almost everyone depends, directly or indirectly, on a knowledge of the earth's magnetic field. When we travel by air or at sea we are guided by the magnetic compass; when our property is surveyed, lines may be defined by their magnetic direction. But the magnetic field is constantly changing and charts must be issued at regular intervals, showing the values of the magnetic elements and their rate of change.

Prior to 1940 the publication of these charts in Canada was the responsibility of the Topographical Survey. Since magnetic declination is the most important element both for navigation and for surveying, most of the maps were of declination. In 1942 the mapping responsibility was transferred to the Observatory and it became a major preoccupation during the next several years.

There are normally two aspects to the problem: to know what the several elements of the field are at any particular epoch throughout the area to be mapped and to know how those elements change with time. There is a third aspect for Canada due to the presence of the north magnetic pole. The magnetic meridians are contorted in its vicinity, making their mapping difficult, and its position is constantly changing. We shall consider this problem first.

The Search for the North Magnetic Pole

The method for locating the magnetic pole has been well stated by Madill[34]: "The only way to fix accurately the position of the magnetic pole is to compute first a position using data from stations not too distant. The declination data will establish the centre of convergence of the magnetic meridians, the inclination data will establish the point where the dip should be 90 degrees and the horizontal force data will establish its vanishing point. The next step is to surround the area indicated with magnetic stations which will further restrict the pole area. The mean pole point must then be found by an intensive ground survey in case the earth's field is deformed by the presence of certain geological formations."

Much of the initial work of locating the pole was done during the last years of the Stewart administration. In 1945 Serson observed at Coppermine and Cambridge Bay, which he reached by Canso aircraft. In 1946 Innes, as a member of Exercise Muskox, made observations at seventeen points along the traverse, which reached as far north as Denmark Bay. During the summer of 1946 observations were made at 14 additional points, including Fort Ross, during the cruise of the Nascopie. When all these data were analyzed, they suggested that the pole was in northwestern Somerset Island or northeastern Prince of Wales Island. The strategy now called for observations close to the pole.

Field measurements were begun in 1947. The RCAF assigned a Canso amphibian aircraft to the task, and Serson and Clark made observations at 10 points, generally to the south and west of the suspected position. One of them was at a lake in northeastern Prince of Wales Island where they made hourly observations over a period of 21 hours. These showed that the pole followed a roughly circular daily orbit with a diameter of some 25 miles on a magnetically quiet day, of 50 miles on a magnetically disturbed day. At the same time Cumming, travelling on the USS Edisto, made observations at eight points, generally north and west of the pole. In 1948 the RCAF again supplied a Canso aircraft and a number of points were established to considerable distance west and northwest of the pole. These were primarily of interest for the preparation of magnetic charts, but they did establish the point that there were no local poles on either Bathurst Island or the Boothia Peninsula[35]. During the 1947 and 1948 work the portable fluxgate magnetometer described earlier was used in the field for the first time. The last survey made in cooperation with the RCAF was by K. Whitham and R.D. Hutchison in 1950; the Canso aircraft was damaged early in the season, but a second one was available and the work was completed on schedule.

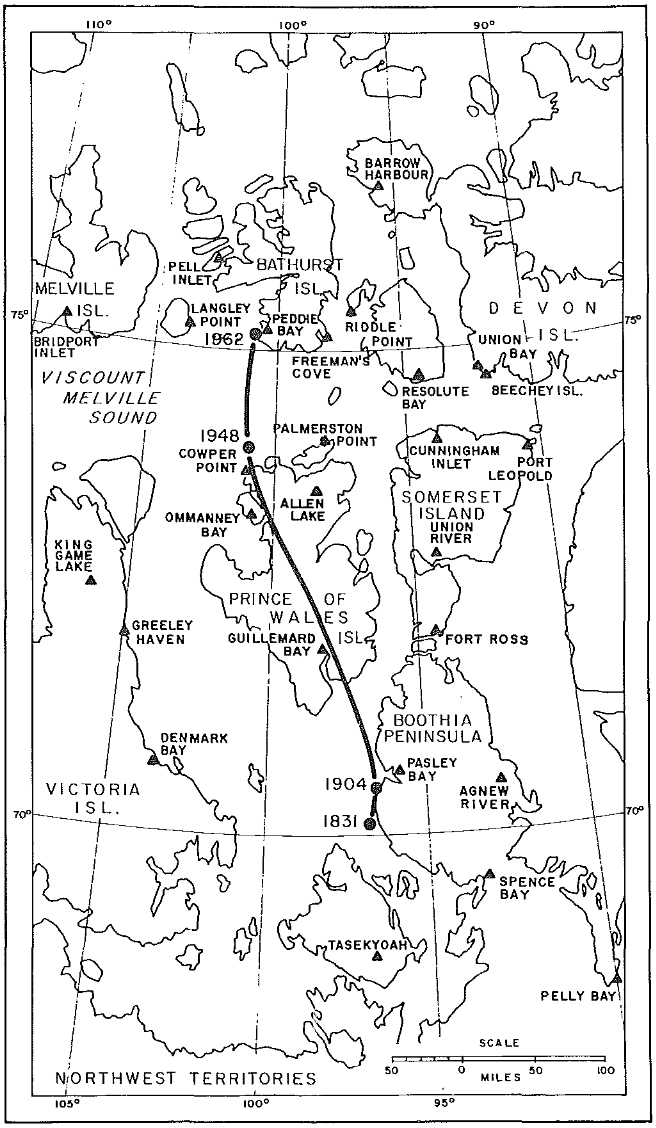
Ralph Hutchison and Paul Serson are given an official sendoff for the 1948 search for the North Magnetic Pole. Back Row – John Drake, R.G.Madill, C.S. Beals, ?, John Carroll, John Goldsmith; front row – Hutchison, Serson, John Jenness. The aircraft is a Canso.

As a result of this field work the Division placed the pole in northern Prince of Wales Island. This is 200 miles from the 1904 position in Boothia Peninsula established by Amundsen, indicating a northern migration of roughly five miles per year. This rate of migration was maintained. In 1955, working from the variometer records at Resolute Bay, 120 miles ENE from the pole, K. Whitham and E.I. Loomer[36] found that the pole had moved to 74°17′N, 99°55′W, 17′ north of its 1947 position. They also found, as suggested by the 1947 results, that the pole had a diurnal motion. It was elliptical, rather than circular, with the major axis varying from 13 miles on a quiet day to 30 miles or more on a disturbed day.

By 1960, 50 months of variometer records from Resolute Bay were available for analysis, and the magnetic charts for 1955.0 had been published. In analyzing these, Whitham *et al*[37], used a set of coordinates, U and V, which are orthogonal coordinates in a polar stereographic projection based on Greenwich. This "Greenwich grid system" was used increasingly in analyzing Arctic data. They found that the position of the pole was less definitely located than had been supposed, but that the rate of migration could be fairly closely defined. They fixed it as 5.5 miles per year to the north, and 0.7 miles per year to the east.

By 1962 the pole was expected to have reached the southern end of Bathhurst Island. E.I. Loomer and E. Dawson[38], working from the CCG ships N.B. McLean and d'Iberville, made a detailed survey of the region surrounding the expected position.

They occupied six stations, using a portable fluxgate and a proton magnetometer. Local gradients were checked within 300 feet of the selected stations to ensure that no anomalies existed near the observing site, and diurnal fluctuations were eliminated from the observations by reference to the variometer readings at Resolute. On the basis of these observations they constructed maps showing isolines of all the magnetic elements – D, H, I, X and Y; from these maps they located the pole for 1962.5 at 75.1°N, 100°W. The figure below shows this position, the location of all points of observation between 1946 and 1962, and the migration path of the pole. The paper also provided a map of the mean daily paths of the pole on international quiet and disturbed days, based on Resolute data for the period July 1961 to June 1962. The diameter of the circle for disturbed days is about 25 miles, but displacement of the pole by as much as 100 miles could occur during severe magnetic storms.



The migration of the north magnetic pole, 1831-1962. The triangles indicate observation points between 1946 and 1952.

Secular Variations

We saw in Part I that as French and Madill conducted their field surveys they observed each summer at a number of new locations, but made a point of re-observing at stations occupied in earlier years. In this way they were able to document the long-term, "secular" changes in the strength and direction of the magnetic field.

With the concentration of Divisional resources on the tracing of the pole immediately after the war, observations for secular change were somewhat neglected. When E. Dawson was made responsible for the production of magnetic charts in 1953, he recognized that secular change would have to be observed in a more systematic way. He began to plan for a network of repeat stations that would provide the uniform coverage necessary for the accurate definition of secular change across the country. This network was established by 1962[39]. It consisted of 103 stations, 31 primary, 72 secondary, divided into nine zones. Many of the stations had a long history of repeated observations, so that the network carried the observations back in time. It was planned to reoccupy the primary stations every five years, the secondary every ten. In fact, observations were made at most stations at least every five years. The USC&GS and Observatory exchanged data from stations close to International boundaries.

It has been suggested since the time of Halley that part of the secular change can be explained by a westward drift of the non-dipole field. In the period between 1950 and 1960 this observation was placed on a sound scientific base by a number of authors. The studies sought a mechanism that would explain the source of the earth's field and account for the changes of polarity observed in palaeomagnetic studies. This was found in gradually changing convection currents in the earth's core. The study of secular change and of westward drift was an important tool for defining these currents.

The 1955.0 magnetic charts for Canada included "isopors" for the various elements D, I, H, Z and F. These are lines of equal annual change. Whitham[40] investigated these changes, seeking to establish a rate of westward drift that would account for them. Introducing a factor for the decay of the field, he found that at the present time westward drift was negligible in Canada. He confirmed this by examination of five declination charts covering the period from 1922.5 to 1955.0. How could this be reconciled with the much larger world average? By repeating the analysis of these world studies but limiting them to Canadian data, he confirmed the low drift. He concluded that the high world figures came from a limited number of areas of high drift.

The studies of secular variation noted above defined the present rate. Were these rates constant throughout historic time? T. Yukutate[41], a postdoctoral fellow, investigated this question by considering data from long-established observatories and from rock magnetism studies of recent lavas, brick-kilns and the like. He found that over the past several hundred years the average westward drift has been about twice the present rate.

The Airborne Magnetometer

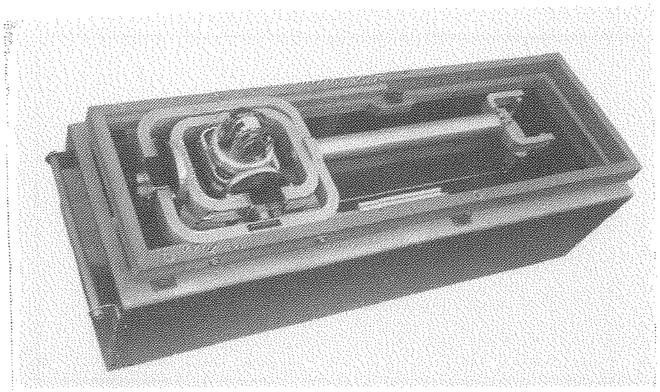
The second aspect of the mapping problem is to provide an initial base map on which to apply the secular variations. Canada is too large to depend on ground observations to provide this base map. The solution – an airborne magnetometer which could complete the magnetic survey of Canada within a reasonable time.

The idea of developing a three-component airborne magnetometer in Canada was first proposed in 1946 by the Subcommittee on Navigation of the Associate Committee on Aeronautical Research of the NRC. The work of this Subcommittee was taken over by the Navigation Research Panel of the Defence Research Board, and they supported the project. The Central Experimental and Proving Establishment, RCAF, built a prototype, and tested it in 1950. It was not a success but it pointed the way in which development should go. The Defence Research Board, represented by S.Z. Mack, and the Dominion Observatory, represented by P.H. Serson and K. Whitham, collaborated in the development[42].

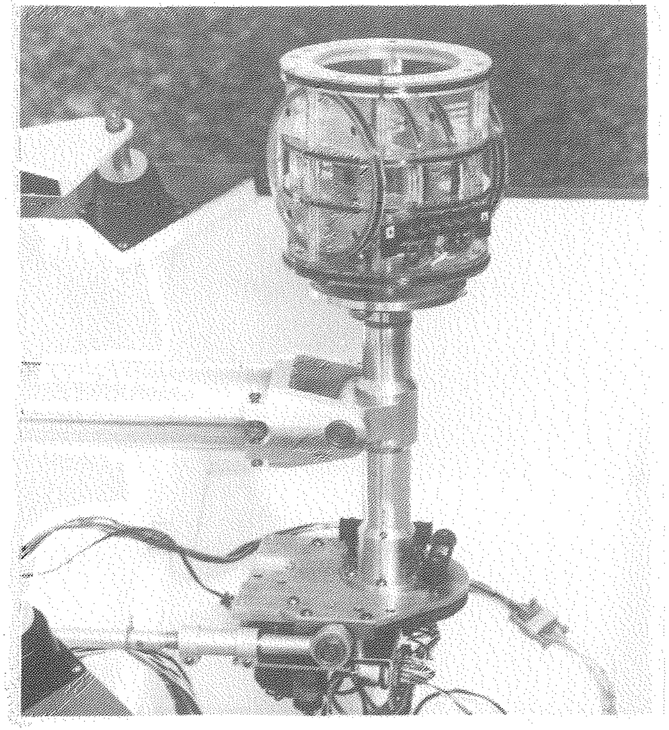
Three-component airborne magnetometers were being built in the United States and Britain at the same time, but these instruments had design parameters that made them unsuitable for the Canadian need. Data from the United States magnetometer required extensive and expensive computer processing. The navigation requirements of the British instrument meant that it could only be flown in clear weather and where a Decca navigation system was available. We required an instrument that would provide data for magnetic charts over vast unpopulated areas, in the form and to the accuracy needed.

Development of the Instrument

To meet this need the Canadian instrument measured the horizontal component, declination and vertical component, in the usual units of degrees and oersteds. Design specifications called for an accuracy of 0.1 degrees in declination, and of 10 gammas (10^{-4} gauss) in each of the components. The directional reference system was to be the vertical, with an accuracy of one minute of arc, and geographic north, with an accuracy of 0.1 degrees in azimuth.



The stabilized platform of the airborne magnetometer with the gyros which react to the roll and pitch of the aircraft. The sensing head was mounted in the bracket to the right.



The magnetometer head, with three, mutually perpendicular, fluxgate sensors. One sensor kept the head pointed toward magnetic north, the other two sensors were maintained in zero fields by currents flowing in the surrounding coils. The currents necesary to null the two components were a measure of the geomagnetic field.

These design targets were met in the actual measurements but errors in correcting for the aircraft's magnetic field introduced uncertainties of 60 gammas in the horizontal and 30 gammas in the vertical. But, since the magnetic charts would be produced on a scale of 100 miles to the inch with contour intervals of 1000 gamma intervals, the attainable accuracy was still better than the accuracy with which the charts could be read.

The design of the instrument was extremely complex and we must content ourselves with an understanding of its basic principles. The fundamental problem was to provide a stable, horizontal platform for the magnetometer head, uninfluenced by the varying accelerations of the aircraft. This was accomplished by mounting the head on a platform stabilized by two servomotors. Error signals from two gyroscopes, reacting respectively to the roll and pitch of the aircraft, controlled the servomotors. A third gyroscope, and servomotor, maintained the azimuth of the platform. Complex electronic circuits supplied the necessary information on pitch, yaw, acceleration and aircraft bearing to these gyros.

The magnetometer head, which measured H, Z, D and the magnetic heading of the aircraft, provided both a continuous output and values averaged over successive five-minute intervals. These average values were more than adequate for the needs of charting and greatly simplified the work.

Astro-navigation, augmented with ground identification where possible, determined the position of the aircraft. To provide a running record of the orientation of the stable platform, observations of the sun or of some star were made at 10-minute intervals. A sextant measured the altitude and bearing of the body with respect to the stable platform.

W.L.W. Hannaford with the complete magnetometer assembly mounted in the aircraft.

Frede Andersen at the control and recording panel.

In 1953 and 1954 the equipment was mounted for short periods in a North Star aircraft of the RCAF. Because the plane was not available for longer periods, no attempt was made to compensate for its magnetic field. This was estimated by frequent flights over areas of known magnetic intensity, one near Meanook, one in a magnetically flat area near Ste. Rosaire, Quebec. A number of flights over various parts of Canada, at a standard elevation of 9000 feet, provided data that could be checked against the magnetic charts for 1955.0, then in preparation. There was good agreement except in areas north of the Arctic Circle, where the preliminary 1955.0 charts were found to be in error. A number of other flights were made in 1954: two over the North Atlantic, from Gander to London, with the return flight from London to Goose Bay, via Iceland; one between Sydney, Nova Scotia, and Bermuda, with an additional flight from Bermuda extending 800 miles to the east.

In 1955 a survey of the Prairie Provinces covered an area of 700,000 square miles. Twelve lines were flown along parallels of latitude one degree apart, at an altitude of 9000 feet. Three east-west lines provided 36 intersections of flight lines from which an indication of the over-all accuracy of the survey could be obtained. The survey required three weeks. The analysis of the intersection data suggested that aircraft position was the principal source of error, but that the accuracy was well within the requirements of charting. Serson and Hannaford[43] provided an analysis of these errors. Among other conclusions their study showed that the most probable source of the "noise" on the mean traverse values was thermo-remanent magnetism of basic rocks at depths of 10 to 12 km. In a later paper[44] Whitham found that these depths are probably accurate to ±12%.

At the end of the 1955 season the instrument underwent substantial modifications. New gear trains were built, more powerful servomotors were installed, and some of the electronic circuits were redesigned[45]. The work was completed in time for a field season in 1957, a survey covering parts of Ontario and Quebec, including Hudson Bay, using a chartered DC 3 aircraft. The two-engine aircraft did not provide the range and performance needed for safe and efficient operation[46]. From this time on all surveys used four-engine aircraft, and the work became routine. We tabulate on the following page the surveys of the several succeeding seasons.

The stabilized platform and magnetometer head were redesigned and rebuilt during 1964 and no programs were flown for three years following the Scandinavian work. This was due to the need to reduce those results and to react to some of the problems they raised. We shall return to this matter.

Serson and Whitham[57] chronicled the design modifications in a paper, written originally in 1957, modified in 1966 and finally published in 1971.

Interpretation of Results

The method of presenting the flight data evolved over the years. For the period 1953 to 1955, 5-minute averages were corrected for the magnetic field of the aircraft and for the error in the directional gyro, and reduced to sea level. The reduced

Year	A/C	Line Miles	Location
1957	DC-3	25,000	Parts of Ontario and Quebec, including Hudson Bay[46]
1958	B-17	21,000	British Columbia[47]
	B-17	22,000	Vancouver-Australia-Philippines-Japan
1959	DC-4	36,536	60N°-70°N; 60W°-141°W[48]
1960	DC-4	45,000	Six N-S lines, Frobisher to Bermuda, covering eastern Quebec, the Maritimes and Davis Strait Four transatlantic lines 200 miles apart, 50°N to 64°N[49]
1961	DC-4	45,000	Central Canada, border to 64°N, 66°W to 122°W[50]
1962			Program postponed - austerity[51]
1963	DC-6	38,000	Greenland, Arctic archipelago and Arctic Ocean[52]
1964			New stable platform and magnetometer head being designed and constructed[53]
1965	DC-6	79,000	Greenland, Iceland, North Atlantic and Scandinavia[54]
1966			Not operated
1967			Not operated
1968			Not operated
1969	DC-6	47,000	British Columbia and northeast Pacific Ocean[55]
1970	DC-6	50,000	Canadian Arctic Archipelago, northern Greenland, and Arctic Ocean to the Pole[56]

values were written directly on a chart with a scale of 1:3,000,000 (47 miles per inch), the data being written at the mid-point of the 5-minute, 20 mile segment of the flight line. Where flight lines crossed, the two sets of values were compared and, if necessary, adjusted. The maps produced, both for western Canada and for the area east of Bermuda, were difficult to contour. Despite the smoothing produced by the use of the 5-minute averages and by the high level (9000 feet) at which the lines were flown, anomalies of several hundred gammas occurred frequently. Were these to be averaged out to produce a map of the smooth field? If so, the same result could have been attained, at much less expense, by broader line spacing. But what effect would broader line spacing have on the accuracy of the charts?

Serson and Hannaford[43] found, as expected, that the charting error increased with the line spacing, but that an airborne survey with lines 50 km apart, costing five times as much as one with lines 250 km apart, would produce charts only 35 per cent more accurate.

By the time the 1963 Arctic survey was completed, the Departmental IBM 1620 computer was in operation. A more sophisticated analysis of the data could be undertaken[58]. As in earlier work, these data were in the form of 5-minute averages of D, H and Z. Instead of plotting them directly on charts, a third-degree polynomial was fitted to each set to obtain a least-square best fit to the data. Values for the several charts were derived from the polynomial values.

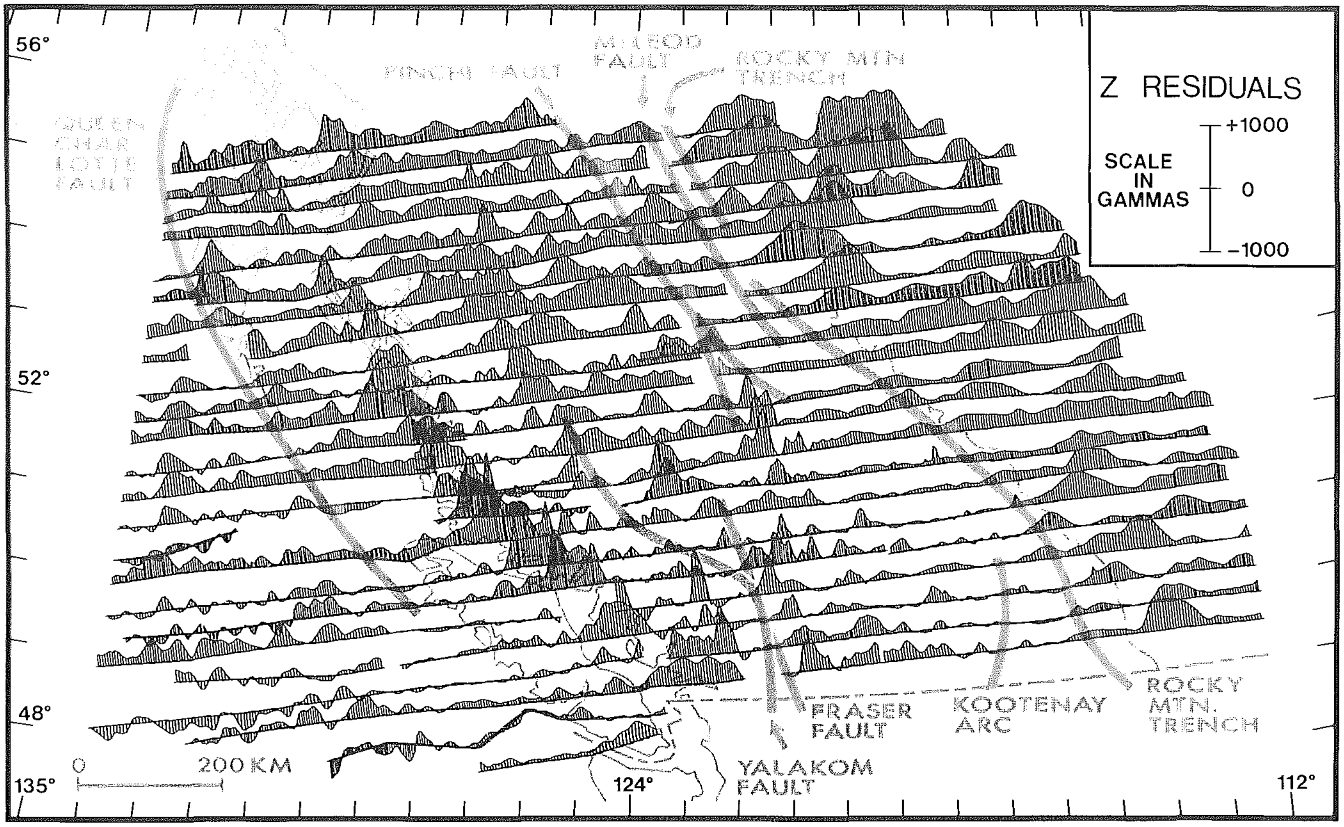
The polynomial only displayed variations with dimensions of some 1600 km. Anomalies due to smaller structures showed as differences from the polynomial values. In this paper, for the first time, these anomalies were mapped as profiles along the flight paths, positive anomalies as a peak above the path, negative anomalies as a trough below it. These served as a basis for conventional anomaly maps.

The appropriate scientific organizations of Denmark, Norway, Sweden and Finland cooperated in the 1965 survey of Iceland and Scandinavia[59]. The line spacing was 20 nautical miles (37 km) and the average flight altitude 3 km. A survey of the north Atlantic, from the coast of Norway to the west coast of Greenland, was flown at a line spacing of 100 nautical miles (185 km) and an average altitude 5 km. In addition to the redesigned airborne magnetometer the aircraft carried a proton-precession magnetometer in a tail-stinger. It provided a running measure of the total field.

The usual precautions were taken for calibrating the magnetic field of the aircraft, with flights over calibration ranges in both Canada and in Scandinavia. The aircraft magnetism did not remain constant, but varied from one flight to another. Residuals from third-degree polynomials fitted to the data revealed errors that persisted throughout each flight. Corrections were made on this basis. Comparison of values at some 70 points of intersection provided a measure of the standard error.

G.V. Haines[60] fitted a third degree polynomial to the tabulated data for Scandinavia to produce regional maps of the north, east and vertical components. He also computed and plotted the differences between his values and those obtained earlier, based on conventional ground-based observations and using a completely different method of analysis. Finally, he mapped the residuals in three components. Later[61], anomaly maps were published for all three sections of the survey. Several large anomaly systems were indicated. Those for Scandinavia were later analyzed by R.P. Riddihough[62], those for Iceland by Serson, Hannaford and Haines[63].

In the 1969 survey of British Columbia and adjacent areas of the Pacific[64], flight lines were again separated by 20 nautical miles, aircraft elevation varied from 15,000 to 22,000 feet. Analysis followed procedures established with the Scandinavian data. The data were first analyzed to provide information on variable magnetic effects of the aircraft. Then a third order polynomial was fitted to the 5-minute averages, and polynomial values of D, H and Z were displayed in contour maps, in this case produced by a computer. Anomaly maps were not based on the difference between observed mean values and the polynomial values. Instead, standard world



Z residuals from the International Geomagnetic Reference Field for British Columbia and adjacent areas of the Pacific Ocean. Geographic and tectonic features are indicated beneath the overlay.

maps of the three components, produced by IAGA and known as the International Geomagnetic Reference Field (IGRF), were used. IGRF values were subtracted from the 5-minute averages, and the resulting anomalies shown as residual profiles[65].

The anomaly maps show a marked demarcation between the magnetically flat ocean, and the continent. The paper presents some discussion of the continental field and its anomalies but a more complete discussion was given later, by Haines, Hannaford and Riddihough[66]. From the anomalies based on the IGRF they produced a contoured anomaly map and compared this with the major tectonic features. They found three distinct magnetic zones, an Oceanic zone, a Cordilleran zone extending from the edge of the continental shelf to the Rocky Mountain Trench and a Shield zone, east of the Trench. Within these zones the variations could generally be related to the mapped geology and tectonics, but there were areas, particularly close to the boundaries, that could not be readily explained. Berry, Jacoby, Niblett and Stacey[67] discussed their relation to other geophysical properties.

The 1970 survey covered most of Arctic Canada north of latitude 68°, northern Greenland, and the Canadian sector of the Arctic Ocean to the geographic pole. Average elevation was 12,000 feet, line spacing 120 nautical miles. The collection and reduction of the data followed established procedures. Anomalies, computed as a difference between the polynomial fit and IGRF values, were presented as residual profiles. The polynomial position of the magnetic dip pole showed it to have moved 40 km northeast from its 1963.9 position, indicating a drift rate of six km/year.

The magnetic anomalies were discussed in a separate paper[68]. In addition to a map of the anomalies, a second map, limited to magnetic "highs" and "lows", provides a comparison with other geophysical data. As in British Columbia, the vertical field increases markedly when coming on to the continent from the ocean. On the continent itself, the inland areas coinciding with a stable Precambrian platform show much higher anomalies than the coastal regions.

Geomagnetic Charts

Dawson and Dalgetty[39] give a history of magnetic charts in Canada, from which the table on the following page is taken. Until 1932 charts were issued by the Topographical Surveys Branch of the Department of the Interior. The first chart to carry the Observatory name was a Polar chart for 1942.5; we saw in Part I that this was an effort solely of Madill. It is probable that the change of responsibility followed the 1936 reorganization which saw the demise of the Department of the Interior and the transfer of the geophysical assets of the Meteorological Service to the Observatory.

E. Dawson joined the Divisional staff in 1953 and assumed responsibility for chart production. In 1954 the IAGA established rules for the publication of national charts: charts of declination were to be published at five year

Table III – Compendium of Canadian magnetic charts produced since 1883.

Chart	Area	Epoch	Scale Mi/in	No. of Stns.	Compiling Agency	Notes
D, I, F	Canada	1844	100	300	Lefroy	south of 60°N, produced in 4 sections
D	W. Canada	1904.0	——	——	T.S.	D.L.S. use only
D	W. Canada E. Canada	1911.0	100	4,500	T.S.	in two sections
D	W. Canada	1912.0	35	4,000	T.S.	
D	W. Canada	1914.0	300	6,800	T.S.	produced in 2 sections
D, $\dot{D}$	W. Canada	1917.0	100	9,400	T.S.	first graphical showing of secular change up to 60°N; all future charts to show isopors
I, $\dot{I}$	W. Canada	1917.0	100	900	T.S.	
H, $\dot{H}$	W. Canada	1917.0	100	900	T.S.	
D	W. Canada	1922.0	100	20,000	T.S.	first Canadian magnetic chart showing location of NMP on northern end of King William Island
I	W. Canada	1922.0	197.3	1,200	T.S.	
H	W. Canada	1922.0	197.3	1,200	T.S.	
D	Canada	1922.0	100	——	T.S.	
D	Canada	1927.0	100	24,000	T.S.	
I	Canada	1927.0	300	1,700	T.S.	
D	Canada	1932.0	100	30,000	T.S.	
H	Canada	1932.0	300	3,750	T.S.	
D, $\dot{D}$	Canada	1940.0	100	——	S.E.B.	Surveys and engineering Br.
D.I.H. Mag. Mer.	Polar	1942.5	100	——	Dom.Obs.	Gnomonic projection
D	N.W.T., Yukon	1948.5	80	——	Dom.Obs.	
D	Canada	1948.5	100	——	Dom.Obs.	
D	Canada	1955.0	100	20,000	Dom.Obs.	
I, H, Z, F	Canada	1955.0	100	2,000	Dom.Obs.	
D	Man.	1955.0	20	3,470	Dom.Obs.	compiled mainly from D observations made by T.S.
D	Sask.	1955.0	20	6,250	Dom.Obs.	
D	Alta.	1955.0	20	6,650	Dom.Obs.	
D	Canada	1960.0	100	24,000	Dom.Obs.	including 5,000 airborne observations
Special	Arctic Canada	1960	137.5		Dom.Obs.	
D, I, H, Z, F	Canada	1965.0	100	18,000	Dom.Obs.	Observations from 1947 on
Special	Arctic Canada	1965.0	120		Dom.Obs.	

intervals, those for the other elements at ten year intervals, both sequences to start in 1955. Dawson's first responsibility was to produce a complete set of charts for epoch 1955.0.

There were approximately 22,000 component values on file – a component value being an observation of D, I, H, Z or F. About 25 % of these values came from the Observatory's growing magnetic survey program. The Topographical Survey and the Meteorological Service were important Canadian sources. Major American contributors were the USC&GS and the Carnegie Institute. The US Naval Oceanographic Office, which had flown an airborne magnetometer between Alaska and Greenland in the early 1950s, provided data from that part of the flight over Canadian territory. Dawson produced charts for I, H, Z, F and D.

Meanwhile Anne B. Cook, at Meanook, collected additional observations of declination made by Topographic Survey officers in western Canada since 1907. These data, plus those already on file, were used to produce declination charts for the Prairie provinces, epoch 1955.0. The charts, at a scale of 20 miles per inch, compiled from over 16,000 observations, showed considerably more detail than the national ones.

24,000 observations of declination, including 5000 produced by the airborne magnetometer, were used to compile the 1960.0 D chart for Canada.

In constructing the charts for 1965.0, Dawson and Dalgetty[39] had 13,600 airborne magnetometer observations available, and they decided to limit data to observations made since 1947. This gave them 4,400 surface observations, providing uniform coverage of the country. Secular change data from United States, Greenland and the Soviet Union, and from the secular change stations already described, updated these observations to 1965.0. Because of the distortions produced by the magnetic dip pole, the data were treated in two sections. The first covered the country from the southern border to 65°N, the second from 60°N to 85°N. The 5° overlap provided an adjustment zone to reconcile the two computations. The calculations were made on the Departmental computer.

Once the 1965.0 values of each magnetic element were available, the charting could begin. This required an averaging of the values to eliminate local anomalies, and the careful merging of the data from the overlap area. Again the Departmental computer was used. The resulting maps covered D, H, Z, F, I, X, Y, and their annual rate of change, as well as G, U, and V which relate to the Greenwich grid system.

The declination chart for 1970 was published, as required by international agreement.

In searching old land titles, surveyors must retrace the original compass bearings, and this of course requires a knowledge of the then-existent value of declination. Working backwards from the 1965.0 charts, Dawson and Dalgetty[69] provided declination tables for 72 places in eastern Canada. For most locations these covered the years from 1750 to 1965.

STUDIES OF THE VARIABLE FIELD

The Arctic Observatories

The importance of Canada's Arctic observatories in defining the earth's magnetic field has already been pointed out, and the response of the Observatory to this responsibility has been documented. In addition to the permanent observatories, nine variometer stations were installed during the IGY. These stations, together with the then seven permanent observatories, provided a line of observation points lying within ±20° of a single geomagnetic longitude. The line reached from low geomagnetic latitude, through the auroral zone, into the polar cap.

The IGY marks the beginning of the Division's research based on the records produced in its network of stations. Until then the observations had been reported through Year Books, but no attempt had been made explain them. The IGY changed all this, and the stimulation it gave to the study of solar-terrestrial relations ensured that the change would be a permanent one.

Early Studies

This line of stations required several years to establish and the analysis of the magnetic field in Arctic Canada didn't wait for its completion. It had been suggested that an inner auroral zone existed at the geomagnetic latitude of Resolute. If this were so, the disturbance patterns at Baker Lake, on the northern edge of the auroral zone, and at Resolute on the polar cap, should be quite different. Whitham and Loomer[70] compared K indices at the two stations.

In producing K indices, one must establish a scale for each station, appropriate to its activity. With the lower limit of K9 set at 2500 gammas at Baker Lake and 1500 gammas at Resolute Bay, the authors found similar frequency distribution curves for the two stations. Having established equivalence, they used K indices to select days when Resolute Bay was at least twice as disturbed as Baker Lake. Even on these days there was a good correlation of disturbances between the two stations. There was no support for the existence of an inner auroral zone.

Shortly afterwards Whitham and Loomer produced a major study of the characteristics of the magnetic records at Resolute and Baker Lake[71]. The paper was divided into two parts; the first considered cyclic field changes, the other transient changes. In the first section, locally disturbed and quiet days were selected, and separated by seasons. The 24-hour means of the three components X, Y and Z, for each group, were analyzed harmonically. At each station there was a persistent 24-hour cycle. At Resolute Bay this was substantially independent of the intensity of the disturbance or of the season. At Baker Lake the daily variations were much more marked, changing form as the disturbance increased, and showing more seasonal variation. At both stations there was a suggestion of a 12-hour wave, more pronounced at Baker Lake.

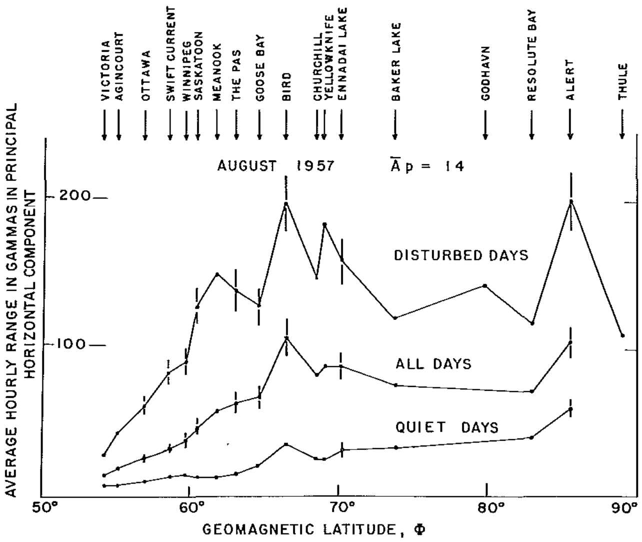
The second half of the study, which considered transient field changes at the two stations, introduced terms which we have not yet met: sudden commencement storms (SC), non-sudden commencement storms (NSC), and bay phenomena. These are terms used in bulletins to describe the morphology of magnetic storms. The first two define the sharpness of the storm's beginning. In a bay the record of H or Z is deflected from the base and returns some time later, the record resembling an indentation of the coastline in a geographical map.

Whitham and Loomer[71] compared SCs observed at Resolute Bay and Baker Lake with those listed in international bulletins. They found that at Resolute, within the polar cap, SCs could be identified only half as frequently as in more moderate latitudes; at Baker Lake, on the edge of the auroral zone, they could be observed only one-third as frequently. The numbers of SCs were not related to the local time, but their amplitudes were, being much higher during daylight hours. The amplitudes at Baker Lake were, on average, about twice those at Resolute Bay.

Bay disturbances were common at both stations, particularly at Baker Lake, and they had a recurrence pattern of slightly less than 24 hours. An attempt was made to infer a current field responsible for the storms and bays. To a first approximation the same field could account for both.

These results were of interest to commercial exploration companies flying total-field airborne magnetometer in the north. How severe were the magnetic storms, could the results be corrected with reference to one of the permanent observatories, was there an optimum time of day or season of year to operate? Using both the K and the S indices Whitham and Loomer[72] produced charts giving the mean diurnal variations in different seasons. They showed that the magnetic daily range in northern Canada would not exceed that at Baker Lake by a factor of more than two at the worst of times and that during quiet periods it would agree within 10%. Considering all the sources of error they concluded that surveys of sufficient accuracy could be attained only by using short flight lines and by flying at magnetically quiet times of day. The study was extended to more southern latitudes in a later paper[73], supported by observations from four temporary observatories.

The nine variometer stations operated during the IGY combined with the permanent stations to cover a complete range of latitude lying within ±20° of a single geomagnetic longitude. Whitham, Loomer and E.R. Niblett[74] analyzed the horizontal component records. Plotting hourly-range indices for the stations arranged by latitude, they obtained a series of plots similar to the one shown on the following page. These plots showed, as expected, a maximum of intensity throughout the auroral region, and confirmed the good correlation of the disturbances at Resolute Bay and Baker Lake.



Irregular magnetic activity for stations arranged by geomagnetic latitude. From Journal of Geophysical Research, 65, 3963, 1960.

Daily fluctuations showed two maxima, one at night the other during the day. Soviet scientists had suggested a second, afternoon, maximum, confined to the auroral zone. It could not be unambiguously established at the Canadian stations. Loomer and Whitham returned to this question in a second paper[75] in which they studied the hourly range data on all three components for the entire year 1960. Again they found only marginal evidence for the second class of daytime activity.

The Alert Anomaly

The most striking fact that emerged from the study of latitudinal variation is illustrated in the figure reproduced above – the very high activity in the principal horizontal component at Alert. This was accompanied by a low activity in the vertical component. Was this evidence of the suspected inner auroral zone? The authors called for additional variometer stations between Resolute Bay and Alert to settle this question.

A beginning of this extra network was provided during the 1961 field season. With the help of the Defence Research Board and Polar Continental Shelf scientists, three-component electrical variometers were operated at five widely dispersed positions within the polar cap zone. Analyzing these new observations, Whitham and F. Andersen[76] concluded that the "Alert effect" could not be due to an inner auroral zone. They also excluded a number of other ionosphere-related possibilities. The effect could only be explained in terms of currents induced in a large body of anomalous conductivity within the earth. The body, striking NE-SW, was centred SE of Alert and reached within 15 km of the surface. A thinner than normal crust, with higher than normal temperature, was indicated, but no other geophysical evidence was available to support or oppose this conclusion.

In 1962[77] two three-component magnetic variometers were operated at points along a line through Alert, running at right angles to the postulated anomalous body. The first set of points were at distances of 120 km on either side of the body, the second set at distances of 50 km on either side.One horizontal component was oriented parallel to the body, the other at right angles to it. As expected, the perpendicular component at Alert, lying close to the body, produced an anomalously high level of activity, the vertical component a slightly lower level. When the variometer stations were at distances of 50 km the effect was still noted; at distances of 120 km it had disappeared. From this fact it was possible to make an estimate of the dimensions of the feature. Assuming a cylindrical shape, a radius of about 50 km and a depth of 50 to 70 km was suggested.

This was the first use in Canada of a technique known as geomagnetic depth sounding which was shortly to become important. Without measuring the electric currents in the earth, their presence and magnitude is inferred from their relative influence on the magnetic field. We shall see the further applications of this in a later section.

Gravity profiles were taken along the line of the variometer stations and along a parallel line 150 km to the southwest. Interpretation of the profiles was difficult because of a lack of regional control, but with the most favourable interpretation it did not fit the hypothetical cylinder unless a crustal thinning of some 20 km was also accepted.

The Mould Bay Anomaly

Meanwhile another anomalous site had been discovered at Mould Bay. The first magnetograms received from this station, in April 1962, showed serious attenuation of the short period vertical variations. Whitham[78] found the most probable cause to be a flat-lying sheet of highly conducting material beneath the station. He estimated it to be 10 to 20 km thick, and to lie near the base of the crust. This would imply anomalously high temperatures. Clearly, heat flow measurements were needed.

Using a method developed at Cambridge University, a probe 2.75 m long and 2.5 cm in diameter was made out of seamless steel tubing and fitted with thermistors at distances of 10 cm, 110 cm, and 210 cm from its lower end. Wires from the thermistors ran up the tube to a recording unit placed above it. Here a Wheatstone Bridge and a switching arrangement measured the temperature differences over 100 cm and 200 cm and recorded them on a battery-powered paper recorder.

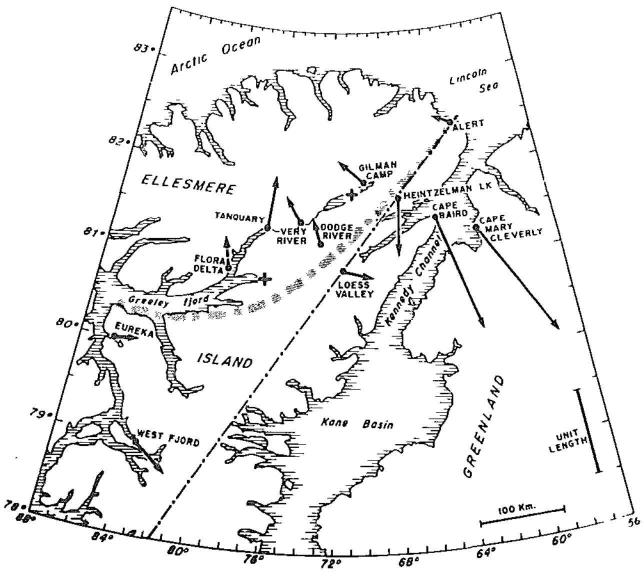
During the spring of 1964 the probe was used at three sites in McClure Strait, south of Prince Patrick Island and Mould Bay. It was lowered to within 90 m of the bottom through a hole drilled in the ice, and held there so that the thermistors could adjust to the temperature of the sea bottom. It was then allowed to fall freely, and usually penetrated the bottom sediments fully, being stopped by a broad plate at the bottom of the recording unit. The system was allowed to rest there for about thirty minutes, until the thermistors had come to equilibrium. The temperature differences could be read from the record. A bottom sample provided data for thermal conductivity. This involved the measurement of the water resistivity and of the percentage of water in the core; the conductivity of the sediment is negligible in comparison with that of the contained water.

The work was successful, but showed no evidence of the suspected temperature anomaly[79]. The following season the work was extended, with an improved version of the probe, to provide two measurements in the Arctic Ocean, off Prince Patrick Island, three observations in the Crozier Channel immediately south-east of the Island and one in Kellott Strait, somewhat further to the southeast. Again the measurements were successful; again they showed no evidence of the supposed high-temperature layer[80].

Whitham[81] reviewed the earlier theoretical work relating to both the Mould Bay and Alert anomalies without finding any errors in the conclusions or being able to find satisfactory alternatives. T. Rikitake, who was at the Observatory as a Distinguished Visitor, and Whitham[82] further reviewed and extended the theoretical basis for interpreting the Alert anomaly. A body with high electrical conductivity, striking NE/SW, continued to be the favoured model: "an upheaval of about 100 km of the 1400°-1500°C isotherm to within 25-30 km of the surface appears capable of accounting for the gross characteristics of the response, but the solution is not unique, and may be in error by as much as 60%." They called for "carefully designed experimental studies of the heat flow, gravitational (including tidal), seismic, geodetic, rheological and possibly tectonic consequences", a rather wistful litany considering the difficulties of conducting field studies in such a remote and forbidding land.

However, work did continue. In 1963 magneto-telluric experiments, a technique to be described in a later section, were carried out at Alert and at Lake Hazen[83]. The results were completely inconsistent with the magnetic variation findings. Aeromagnetic total-field surveys added a further complication[84]. They showed low values over Ellesmere Island and the adjacent Sverdrup Basin in the area of the supposed high-conductivity body.

Niblett and Whitham[85] reviewed the work on the two anomalies up to 1970, the close of our period of interest. We shall consider first the Alert anomaly. In 1967 measurements of three magnetic components and of two horizontal telluric components were made at eleven sites forming three traverses at right angles to the suspected feature. The magnetic variations confirmed the earlier, 1962, results: the components parallel to the anomaly were all of about the same amplitude as Alert, the components at right angles were much lower than those at Alert. However a new technique of analysis had become available, the "Wiese vector". It "is expected to point away from a zone of high conductivity, so that along a profile of stations transverse to the strike of a pronounced anomaly, the arrow should point systematically away from the zone on either side, and nearly vanish over the top of the zone." These vectors are shown in the figure below. The zone that they define is not the simple NE/SW one postulated earlier, but the more complex curved one indicated by the shaded line, and extending as far as Eureka[86]. The resistivity values computed from the telluric measurements supported the idea of high conductivity in this zone. As for other geophysical evidence, no seismic crustal work has been done on Ellesmere Island and there is no evidence of any seismic activity in the anomalous zone.



Map of northern Ellesmere Island, showing locations of field stations and the Wiese vector at each station. The broad dashed line represents the path of the anomalous zone derived from the Wiese vector analysis. From Journal of Geomagnetism and Geoelectricity, 22, 100, 1970.

Additional work had been done also at Mould Bay. During the summer of 1963, fluxgate magnetometers were operated 100 km north, east, south and west of Mould Bay without finding any reduction of the anomaly. In 1964 the same instruments were operated along the north shore of Melville Island in the direction of Resolute Bay and showed that the anomaly was limited to the east at a distance of 200 km.

Seismic crustal measurements in the vicinity of Mould Bay[87] did not suggest a Pn velocity lowered by elevated mantle temperatures. While there is some seismic activity in the western Arctic, in particular an earthquake swarm at Mould Bay[88] in 1965, it appears to be tectonic in origin and not related in any way to the magnetic anomaly. Additional heat flow measurements in the channels surrounding Mould Bay showed some variation, but there was nothing to suggest any abnormal thermal effects.

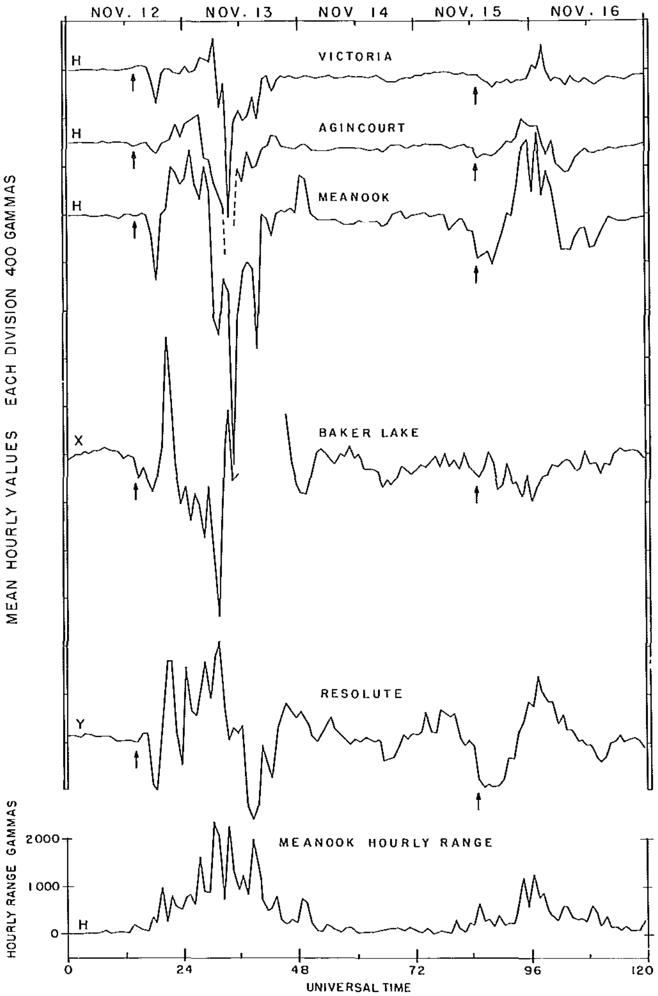
The puzzles of both the Alert and Mould Bay anomalies remained unresolved. They still do.

Special Days

There are a number of interesting studies of magnetic records on particular days. Two of these papers deal with attempts to study the effects of a solar eclipse.

The expected effects, caused by slight changes in the ionosphere due to the temporary reduction in insolation, are normally less than the noise level. Detection at a single station is usually impossible. This was the case for the total eclipse of July 20 1963, which J.L. Roy observed at Disraeli, Quebec, using a portable Askania variograph[89]. In the case of the next eclipse, of March 7 1970, R.J. Stening *et al*[90] used an array of four stations at right angles to the path of totality. One of them, at Dartmouth, N.S., was in the path of totality, the others were at different distances from it. Transistorized fluxgate magnetometers were used, with filters to reduce the longer period disturbances. It was possible to correlate the short period disturbances at the four stations and to measure the amplitudes of each disturbance at each station. The ratios of the amplitude at the several pairs of stations showed the effect of the eclipse clearly.

In mid-November, 1960, a very large magnetic storm was recorded world-wide. Niblett[91] provided a description of its appearance on the records of Canadian stations, where "the disturbances . . . were comparable with the largest that have been recorded in this country." There were two storms in the four-day period. The records at Victoria, Agincourt and Meanook were similar but the amplitudes increased with geomagnetic latitude. At Baker Lake and Resolute Bay the disturbance was intense, but much less regular. At Baker Lake the second storm was effectively obscured by high background noise.



The magnetic storms of November 12-16, 1960, as recorded at Canadian stations. From Canadian Journal of Physics, 39, 620, 1961.

A nuclear explosion in the atmosphere over Johnston Island (16.7°N, 169.4°W) on July 9, 1962, was recorded at Victoria[92] on a high speed pen-recording variometer, permitting a timing accuracy of 0.1 sec. The record consisted of a slight initial motion followed by a large main movement. This died out fairly rapidly in a series of short-period oscillations. Longer period oscillations continued for about 2.5 minutes.

The transmission time of 11.2 seconds from the explosion to Victoria was too short to be accounted for by hydromagnetic propagation in the ionosphere and too long to be due to speed-of-light transmission. Some combination of the two was required but, in the absence of observatories at other distances, no explanation could be attempted.

Solar Effects

We know that the variable part of the earth's magnetic field is due to current systems in the earth's upper atmosphere and in space, caused by the sun. Much of the analyses of seasonal and daily variation already described relate to these effects. Other papers concern the relationship more specifically.

R. Pratap, a postdoctoral fellow, considered the mechanism by which solar flares produce "crochets", small bays of short duration, on magnetograms[93]. Assuming that the effect was due to a change in the ionospheric conductivity induced by the flare, Pratap computed the amplitude and phase of a crochet at a medium and at a low-latitude station and found a reasonable correlation with observation.

Another postdoctoral fellow, T. Yukutake, selected a group of 27 observatories that had been in operation for 55 years or more. He analyzed the annual mean values of all three components in a number of ways, searching for a periodicity related to the sunspot cycle. The existence of an 11-year cycle was established, with an amplitude varying in the range 10 ± 7 gammas in the principal horizontal component. Harmonic analysis showed that 16% of the effect was due to currents induced in the deep mantle. Yukutake was able to estimate the electrical conductivity of the mantle[94].

Micropulsations

Micropulsations are small fluctuations that appear in all elements. They are usually sinusoidal, well enough developed that a definite period can be assigned to them. Periods range from 20 to 300 seconds, the amplitude is less than 3 gamma, and they may appear simultaneously over the entire earth.

It had been suggested that pulsations were more intense near the auroral zone. Whitham and Loomer[95] compared pulsations recorded at Meanook, 500 km south of the centre of the auroral zone, and at Agincourt, 1200 km south. The numbers were not significantly different. At both stations they increased about two-fold between sunspot maximum and sunspot minimum. The numbers were higher on quiet than on disturbed days, which may simply have been due the difficulty of identifying pulsations on a disturbed trace. About 4 per cent of the pulsations appeared on both stations, which have a linear separation of 2700 kms.

No regular pulsations could be identified at Resolute Bay, within the polar cap, and the high level of disturbance at Baker Lake tended to mask any pulsations that might have existed there. Some pulsations were found in Baker Lake records by searching at times when pulsations were occurring at Meanook. They seemed to be of significantly shorter period than those at Meanook.

In explaining the observed properties the authors conclude that the pulsations are due to magnetohydrodynamic waves in the upper parts of the ionosphere.

By the late 1960s a more sophisticated classification of pulsations had been established. They were separated into continuous (Pc) and irregular (Pi) pulsations and a number of classes based on period range, as follows:

Pc1 = 0.2-5 sec.
Pc2 = 5-10 sec.
Pc3 = 10-45 sec.
Pc4 = 45-150 sec.
Pc5 = 150-600 sec.
Pc6 = > 600 sec.

Pi1 = 1-40 sec.
Pi2 = 40-150 sec.
Pi3 = > 150 sec.

In 1968, J.C. Gupta and R.J. Stening, a postdoctoral fellow, designed a program to study micropulsations. Identical instruments were operated at four stations – Ottawa, Meanook, Baker Lake and Resolute Bay – from July 13 to August 15. At each station two horizontal component fluxgate magnetometers, equipped with band pass filters designed to eliminate both high-frequency noise and long-period field changes, recorded at a rate of 15 mm/min on hot-wire recorders. Time marks were accurate to 2 seconds or better, permitting a correlation between stations. The periods and amplitudes of Pc3, Pc4 and Pi2 oscillations, their variations with time of day at each station, and the correlation of their amplitudes with various indices, were reported in a series of papers[96]. Since the stations provided a profile from well south of the auroral zone to the polar cap, and since the instruments were standardized and designed for the particular range of interest, the papers provide valuable information on the morphology of these particular pulsations.

Sq Variations

The daily variations seen in magnetograms are due to currents in the ionosphere. When the investigation of these currents is confined to quiet days the variations are called Sq variations and the inferred current system is known as the Sq system. Stening[97], used the International Geomagnetic Reference Field (IGRF) to show how the Sq system can be calculated from the geomagnetic field. The magnetic values are used to determine the electrical properties of the ionosphere dynamo and the foci of the current system are derived from this.

Instead of using the IGRF, Gupta[98] measured the daily variations at 108 magnetic stations, distributed world-wide, for the eight most quiet days of the IGY. He computed the amplitudes and phases of the first four harmonics and compared them with equivalent values found earlier for the Second Polar Year (August 1932-August 1933). The IGY occurred during a particularly active sunspot maximum, the Second Polar Year (SPY) at a minimum. Gupta reports that the form of the daily variations are similar but "as expected, larger . . . ranges of the daily variation are found during the IGY than during the SPY period."

Gupta finds evidence of daily north and south migration of the current loops responsible for the Sq variation systems.

Aurora

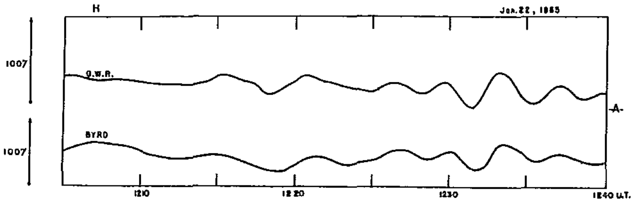
Proof of the coincidence of aurora with magnetic disturbance[99] was one of the important findings to come out of the Canadian IGY program. J.K. Walker selected eight periods of moderate polar magnetic storms, and measured the disturbances in H and Z at each of 37 observatories or variation stations. Using theory developed by R.D. Hutchison[100], he calculated the changing current flow lines responsible for the changing magnetic field. He compared these flow-line patterns with the changing position and intensity of aurora as displayed by all-sky cameras and auroral intensity recorders. The coincidence between the two patterns was striking.

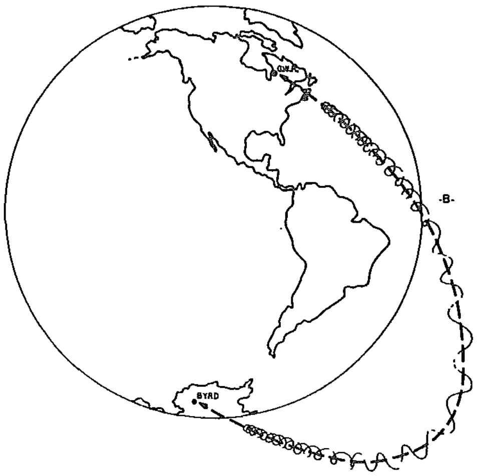
Loomer and G. Jansen van Beek[101] used magnetic substorms to study the auroral electrojet. Substorms are large negative bays frequently observed in the records of stations in the auroral belt. By analyzing the associated current systems, the authors were able to plot the course and changes in the electrojet.

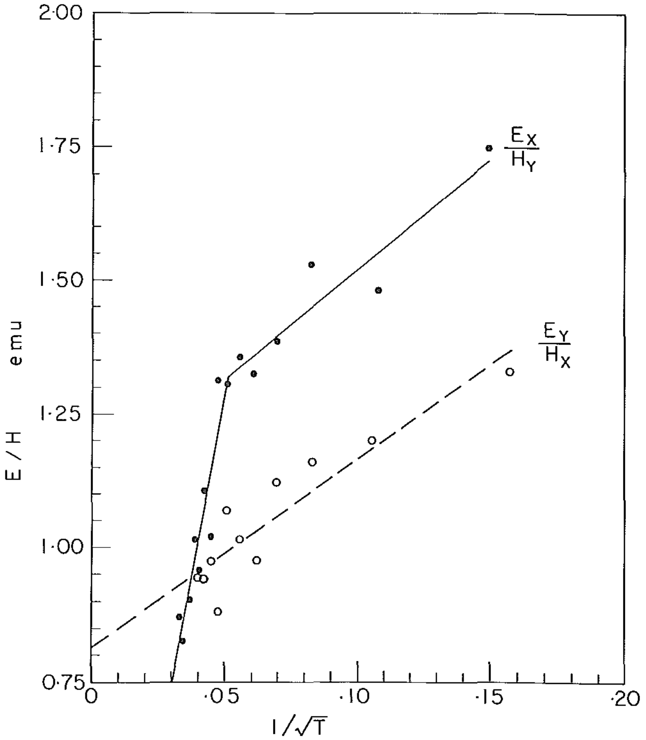
Conjugate Points

As already observed, the IGY stimulated a great deal of research in solar terrestrial phenomena. One such study related to "conjugate" points. It had been suspected for some time that two regions of the ionosphere linked by the same geomagnetic lines of force should show similarity in their electromagnetic disturbance. The observatory at Great Whale River was initially established, as a station conjugate to the antarctic station Byrd, to study their relationship. Similar magnetometers were used at both stations. The figure on the next page shows micropulsations recorded at the two stations; the lower part of the diagram shows the line of force linking the two stations and the spiral path taken by charged particles moving between the two points. These conjugate stations provided important information on the ionosphere.

Before the station was opened, an experiment was conducted to define the extent of the conjugate area[102]. Five variometer stations, all lying close to latitude 55°N, were operated during October 1963. The correlation with events at Byrd was examined for each station. Correlation was best for events with periods greater than 30 minutes, and reached a maximum as one approached the conjugate point in Hudson Bay, south of the Belcher Islands. Great Whale River, which







From Geophysics, 25, 1003, 1960.

was a station on the Mid-Canada Line, was selected as the nearest location to the conjugate point where an installation was logistically feasible.

MAGNETO-TELLURIC AND GEOMAGNETIC DEPTH SOUNDING

These two techniques have a common aim – to seek zones of high conductivity deep within the earth – and are frequently used to augment each other. We shall therefore treat them in a single section.

E.R. Niblett and C. Sayn-Wittgenstein[103] pioneered the use of magneto-telluric measurements in experiments at Meanook, in 1957. They measured earth potentials in a north-south and an east-west direction between electrodes buried in the ground 1.5 km apart. The potential differences E_X (N-S) and E_Y (E-W) were measured continuously and fluxgate magnetometers recorded the components Hx and Hy of the horizontal magnetic field.

Telluric currents and the magnetic fields resulting from them are not steady, but exhibit a quasi-sinusoidal form with periods covering a wide range. With the equipment then available Niblett and Sayn-Wittgenstein were limited to the range from 40 to 1000 seconds. They selected sections of the records of E and H exhibiting well-defined sinusoidal waves and measured the ratios of the potential difference E to the resulting magnetic field H for as many different periods, T, as possible. They measured two sets of ratios, E_X/H_Y and E_Y/H_X and plotted them against $1/\sqrt{T}$ as in the diagram below.

The data for E_X/H_Y defined two straight lines. The scatter in the observations for E_Y/H_X and their limited numbers at long periods prevent the definition of the first line in this case. Straight lines of the form

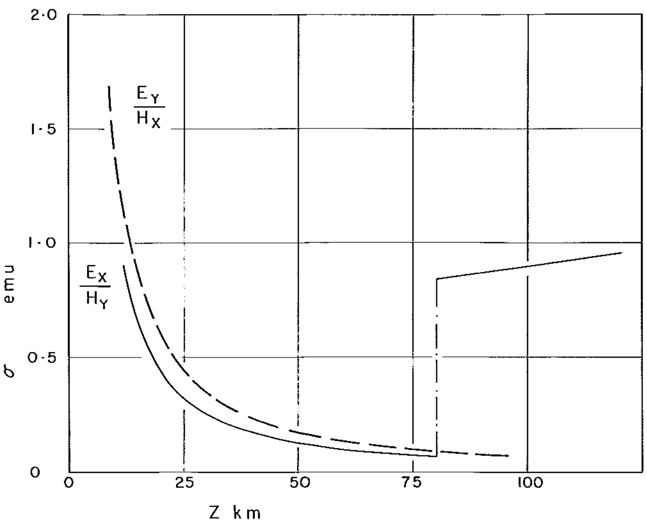
$$\frac{E}{H} = A + \frac{B}{\sqrt{T}}$$

were fitted to each set of data by least squares adjustment.

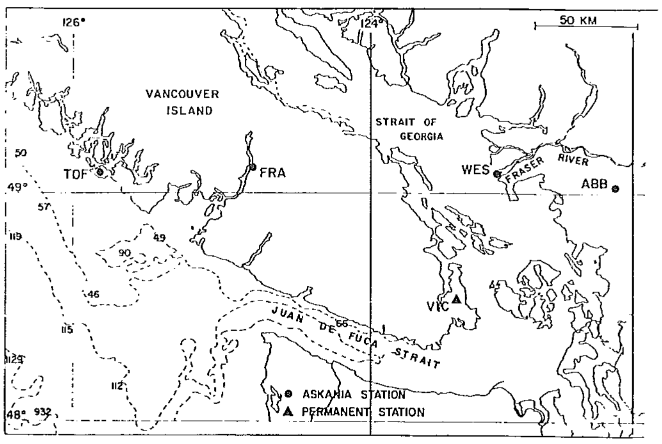
As one would expect, the depth of penetration of the currents is a function of their period. The relationship is complex, but with reasonable simplification the constants A and B are all that are needed to relate electrical conductivity σ and depth to period and E/H ratio. The curves on the following page give the relationship Niblett and Sayn-Wittgenstein found for Meanook.

The curve for E_X/H_Y shows a marked change in conductivity at a depth of 80 km. The discontinuity does not appear on the E_y/H_x, but the discrepancy is due to lack of reliable long period data, not to failure of the method. The pioneering work was a success, and it encouraged additional work in universities.

S.P. Srivistava, a graduate student at the University of British Columbia, made an elaborate magneto-telluric survey of south central Alberta, occupying six stations along a north-south line between Cardston and Meanook[104]. He augmented his Meanook records with those from the observatory and was able to obtain observations at periods as long as 4000 seconds. His plot of the data showed three straight-line sections. The



From Geophysics, 25, 1006, 1960.



From Canadian Journal of Earth Sciences, 2, 488, 1965.

first discontinuity agreed well with the depth to the Precambrian basement, the second at a depth of 90 km, was in reasonable agreement with the 80 km depth found by Niblett and Sayn-Wittgenstein. By comparing his observed resistivity/period curve to computed values he was able to make an estimate of the resistivities of the various layers.

In a later paper[105] in which he reconsidered his data and used additional observations from the Meanook Observatory he found that the method was limited in its resolution. "It is evident that it is not realistic to try to interpret magnetotelluric sounding curves with a large number of layers having small resistivity contrasts between them." Later however, as a postdoctoral fellow at the Observatory, he produced a large number of theoretical curves covering two- and three-layer cases[106]. We might in this connection mention papers by two other postdoctoral fellows in related fields: by T. Yukutake, who considered the problem of induction in an inclined conductor[107]; and by O. Praus who considered the problems posed by anisotropic layering[108].

As we saw earlier, Whitham and Andersen[83] used magneto-telluric measurements at Alert in 1963, to study the high-conductivity body suggested by magnetic variation studies. Their method was not the same as that used at Meanook. They combined the two components of E to produce a hodograph. It showed an electric field strongly polarized in a N-S direction rather than in the NE/SW direction determined earlier by the magnetic variation data. Sections of the records were digitized and subjected to a variety of sophisticated analyses without finding any acceptable explanation. It is now recognized that the instruments then available were inadequate and that the electrical data were poor, but there has been no opportunity to repeat the work with modern equipment[109].

The first geomagnetic depth sounding experiment in Canada[76], to map the Alert anomaly, has already been described. The anomaly was marked by high activity and strong polarization in the horizontal component and Whitham set out lines of three-component variometers at right angles to the anomaly to define its limits. B. Caner, of the Victoria Geophysical Observatory, continued the Divisional interest in the method, working with a succession of students at the University of British Columbia.

Geomagnetic depth sounding in other parts of the world had shown a conductivity discontinuity at the boundary between ocean and continent. Did this "coast effect", which was expected to show an increase in the relative amplitude of the Z component, exist in British Columbia? R.D. Hyndman, then a graduate student at the University of British Columbia, recorded a variometer profile from Lethbridge to Vancouver[110], and this was extended to the west coast of Vancouver Island by A. Lambert, also a graduate student at UBC, and Caner[111].

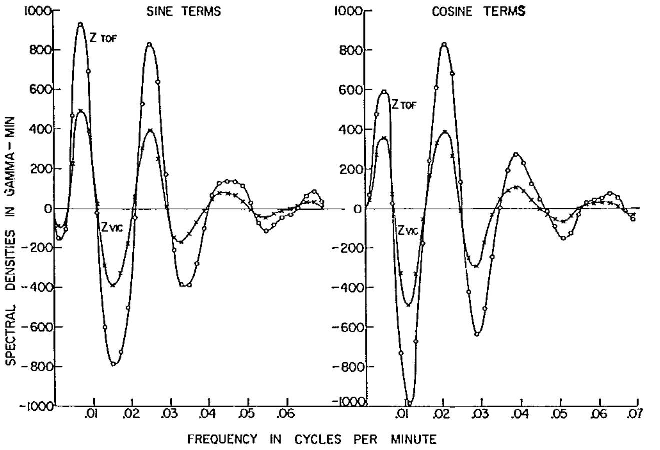
They set out four stations TOF, FRA, WES and ABB in a straight line as shown in the figure above. The stations ran from January 1 to July 15 1965. The magnetograms for Victoria were also available for the study.

Lambert and Caner distinguished three frequency ranges:

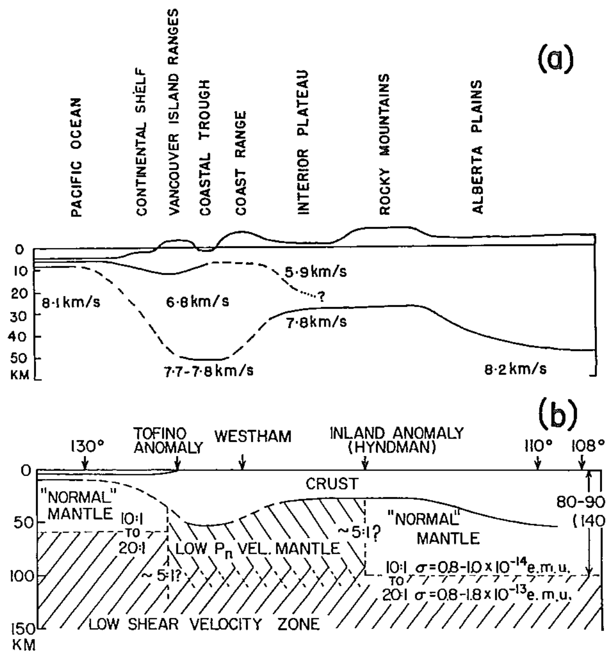
low frequency (periods 6 hours to 24 hours);
intermediate frequency (periods 20 minutes to 3 hours);
high frequency (periods 20 seconds to 600 seconds).

They broke down the observed disturbances into a series of harmonic terms by a Fourier analysis and computed the spectral density for the various periods. The figure on the following page shows their results for two stations exhibiting simultaneous intermediate frequency disturbances. The expected augmentation of the Z component between a coast station (TOF) and one only slightly removed (VIC) is clearly shown. When the study was extended to all stations and the complete frequency range, the results were further confirmed. The coast effect exists, and is demonstrated at all frequencies. Its cause is not clear; it may be due simply to the presence of the ocean, or to some change in the mantle associated with the ocean/ continent boundary.

It was not possible to make a unique solution of the underlying conductivities, but by combining their geomagnetic data with that of Hyndman, and making use of the seismic profile proposed by White and Savage[112], they were able



From Canadian Journal of Earth Sciences, 2, 490, 1965.



(a) Crustal structure in British Columbia, after White and Savage112.

(b) Proposed Upper Mantle structure and conductivities under British Columbia, at latitude 49°-50°N. The low-conductivity zones correlate with those of high seismic velocity Pn. From Canadian Journal of Earth Sciences, 2, 499, 1965.

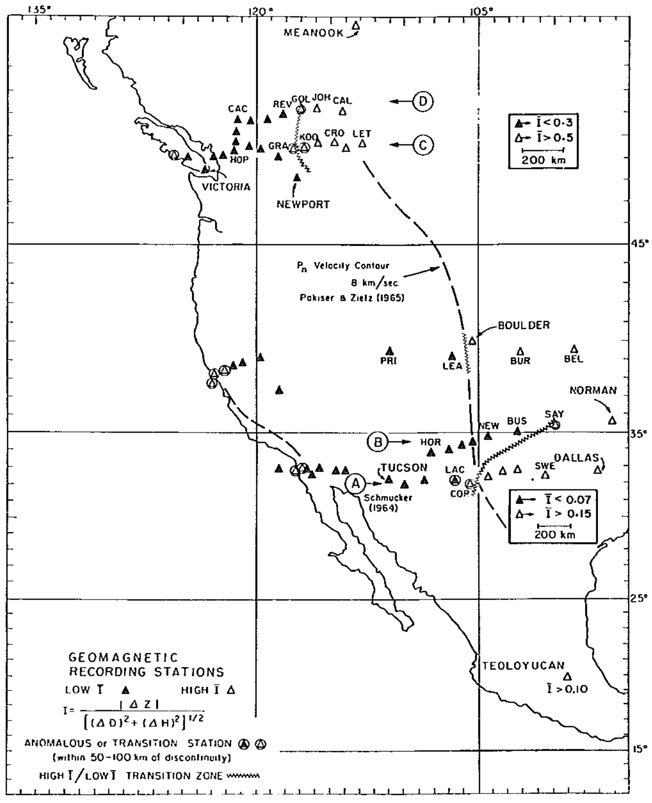
to propose the structure shown in the lower part of the diagram above, in which zones of decreased mantle conductivity correlate with those of increased Pn velocity. This correlation has been demonstrated in several east-west profiles in the United States.

Caner and W.H. Cannon[113] reconsidered the Canadian data and introduced a simple parameter which proved useful. This is the ratio, I, of the amplitudes of the vertical to the mean horizontal component in the intermediate frequency range. The ratio changes by a factor of 2 or 3 in crossing the boundary between low and high conductivity. Their paper summarized all data for western North America and provided a guide for future work. This was commenced immediately through continued cooperation of the Dominion Observatory and the University of British Columbia[114].

In 1965 and 1966 stations were occupied in the southern United States (profile B in the figure below), in British Columbia north of the Hyndman line (profile D in the figure), and along a short line connecting the two Canadian lines. Use was made of the ratio I, defined above, to separate zones of high and low conductivity; the boundaries of the two conductivity zones are shown by wiggly lines.

While the use of the I ratios was more than adequate to fix the position of the zone boundaries it was not sufficient to define the nature of the attenuation. For this purpose power spectra covering the period range from 5 to 60 minutes were calculated and were used to test various models of conductivity variation with depth. These studies suggest the rise of the high conductivity material to within 25-35 km of the surface. They also suggest that, with careful application, the method permits a location of the conductivity boundaries with a resolution of 25 to 50 km.

Niblett, Whitham and Caner provided a critical review of Canadian work on electrical conductivity anomalies up to this time[115] and conclude: "Much more field experimentation and theoretical development are required before we can understand their proper relation to the distribution of physical and chemical properties in the interior and to events in geological time."



Low-I and high-I areas in the Cordillera region. From Journal of Geophysical Research, 72, 6337, 1967.

Caner's attention now turned to magneto-telluric studies. He believed that one reason for the inconsistent results of Srivistava[105] was that, given the presence of a high-conductivity surface layer, observations at very long periods were necessary to permit the accurate application of curve-matching techniques. These long periods had not been observed. With D.R. Auld[116] he planned an experiment at the Victoria Magnetic Observatory to correct this limitation. They set out telluric electrodes to form an L-shaped array aligned in, and at right angles to, the magnetic meridian. A variety of magnetic detectors, including the standard station magnetographs, were deployed to supply coverage from periods of 1 second to direct current. The instruments were operated throughout 1966 and part of 1967.

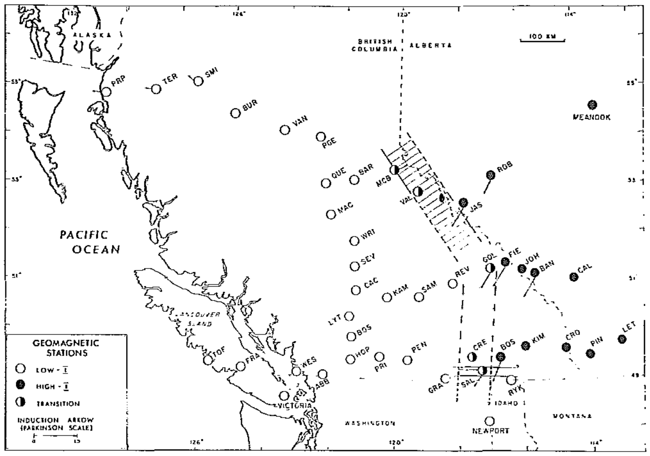
The telluric currents proved to be strongly polarized in a NS direction; tidal effects dominated the amplitudes at periods greater than 15,000 seconds, but they were able, by curve fitting, to obtain a well-defined model. A layer of high conductivity material 10 ± 3 km thick, lay at a depth of 65 ± 5 km. No further conductivity anomalies existed to depths of at least 750 km. In a separate paper[117] Auld published the hourly values of the telluric current in the magnetic north component.

The success of the experiment at Victoria led directly to the next effort. Geomagnetic deep sounding had established a discontinuity in central British Columbia between high-I and low-I stations[113] and it had been inferred that this involved a difference in conductivity in the lower crust and upper mantle. Could the magneto-telluric method be used to measure the conductivities in the two regions?

A cluster of three stations was set up near Penticton, in the low-I region, and another cluster to the east, near Fernie, in the high-I region. Telluric and magnetic recordings covering a period range of 20 to 7500 seconds were made at the various locations over a seven week period in 1967. The analysis confirmed that a high-resistivity crust, some 35 km thick, does exist in the eastern, high-I, region whereas a low resistivity at the base of the crust marks the western zone[118].

In the United States, aeromagnetic profiles filtered to remove short wave-length effects, are generally flatter and more featureless in the western, low-I, region than in the eastern, high-I, region. Caner[119] searched for a similar effect in Canada, using both total force surveys of the GSC and three-component surveys of the Dominion Observatory. He found that, as in the United States, long-period profiles were smoothed in the western, low-I, zone. Combining this fact with data on heat flow, seismic velocities and the measured values of resistivity, Caner considered the petrological implications[120]. He concluded that the high conductivity of the lower crust in the western zone was due to partial melting, probably caused by an increased water content.

At the same time geomagnetic deep sounding was defining the boundary between the two regions as far north as latitude 55°[121]. The results are shown in the figure at the top of the next column. Instead of a clear demarcation between the two zones, there is a transition zone indicated by the shading. Caner[122], reconsidering both the magneto-telluric and deep sounding data, was able to define appropriate conductivity models for the two regions as shown in the figure on the following page.



Low-I and high-I areas in western Canada, with the transition zone between them. The shaded anomalous zone close to the international border is discussed in the text. From Journal of Geophysical Research, 76, 7198, 1971.

One of the stations, SAL in the figure, gave indications of a major anomaly not related to the high-I/low-I discontinuity. During the summer of 1968 a detailed geomagnetic deep sounding survey of the area, involving 20 stations, defined a major sharp deep-seated discontinuity trending east-west at the south end of Kootenay Lake. Geological evidence suggested a compositional, rather than thermal source[123].

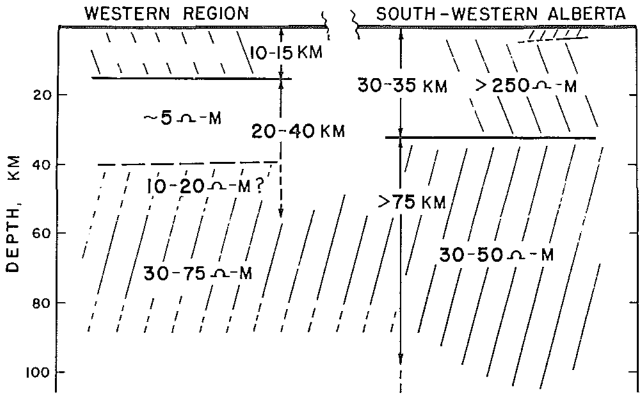
PALAEOMAGNETISM

Introduction

In earlier geologic times, when rocks were formed either by cooling from magmas or by deposition on land or in the deep ocean, any small magnetic particles present aligned their magnetism with the then existing magnetic field. Once the rock had consolidated it bore the imprint of that field. If a precisely oriented sample can be cut from the rock, and if the formation has not undergone any drastic heating or displacement, this imprint can be read. The study of ancient magnetic fields in this manner constitutes the science of palaeomagnetism.

The science has a history dating back more than 100 years to the study of the remanent magnetism in recent Italian lavas, but the modern phase of the work dates from the mid-1940s and 1950s. Very sensitive instruments were developed for measuring the minute "fossil" magnetism in rocks and many specimens were measured. Results often were confused and it was soon realized that the rocks were carrying more than one magnetic imprint. The original field was present, but it had been modified by the subsequent history of the rock. Magnetic minerals cover a wide range of retentivity, and the "softer" minerals would change their magnetic imprint with the changing field. It was necessary to "clean" the specimens of the younger imprint in order to find the original.

It will be worthwhile to consider in more detail how rocks may have acquired natural remanent magnetism[124]. Lavas are normally erupted at temperatures of about 1000°C, and as they cool through the Curie point they acquire a thermo-remanent magnetism which is commonly that of the



Proposed conductivity model for southern British Columbia and southwestern Alberta. The western region is marked by a layer of high conductivity (low resistivity 2M) starting at a depth of 10-15 km and extending to, or into, the mantle. At equivalent depths in southwestern Alberta the conductivity is low (high resistivity 2M). Beneath these surface layers the con- ductivity is of the same moderate order on both sides of the boundary. From Journal of Geophysical Research, 76, 7214, 1971.

then-existing field. Since lavas cool quickly the direction of the field should be sharply implanted; in sills or large dikes the magmas cool over a much longer period so that effects of secular variation may be present.

This statement is over-simplified. If there are anisotropies in the rock the thermoremanent magnetism may be deflected; the iron minerals may be redistributed, or new ones may be formed in late stages of cooling to produce a chemical remanent magnetism which may differ from the thermoremanent one.

Sediments are formed over a much longer time period than are igneous rocks and may therefore show the influence of a varying field in its depositional remanent magnetism. Sedimentary rocks can also show chemical remanent magnetism due to the deposition or enlargement of iron minerals during lithifaction.

Both igneous and sedimentary rocks are subject to metamorphism. Local thermal metamorphism, as at the contact between sedimentary rocks and intrusions, occurs frequently, and iron minerals may be reconstituted over a considerable region during metamorphism. Such changes are all potential sources of error.

In order to eliminate secondary effects and to retrieve the original field, the specimens must be cleaned. There are three principal methods of cleaning. The sample may be heated to a degree sufficient to destroy the secondary field without unduly reducing the original one, and then cooled in a null field. This is known as *thermal* cleaning. For other samples *magnetic* cleaning may be more appropriate. In this the sample is subjected to an alternating field of a strength sufficient to erase the secondary overprint. In both thermal and magnetic cleaning extensive experiments are needed to find the temperature or field appropriate to the particular specimen. A third method of cleaning, *chemical*, is sometimes used to remove from the sample the specific mineral that carries the secondary field.

As we have seen, the magnetic field at any point on the earth may be explained in terms of a dipole inclined at a small angle to the axis of the earth's rotation. The converse problem may be solved: given the direction of the magnetic field at any point on the earth, the orientation of the dipole can be determined. This is true for present fields or ancient ones. When many carefully oriented samples from a single formation are available for measurement, a mean direction of their residual magnetism can be determined and the corresponding position of the fossil dipole computed.

When this is done for rocks of a variety of geological ages from a single continent, it is found that the magnetic pole has traced out a *polar wandering* curve. The polar wandering curve from one continent has much the same shape as that from another, but sometimes is displaced from it, the displacement being greater the older the rocks. This is due to the fact that the two continents have moved relative to each other, and constitutes one of the most telling arguments in favour of continental drift.

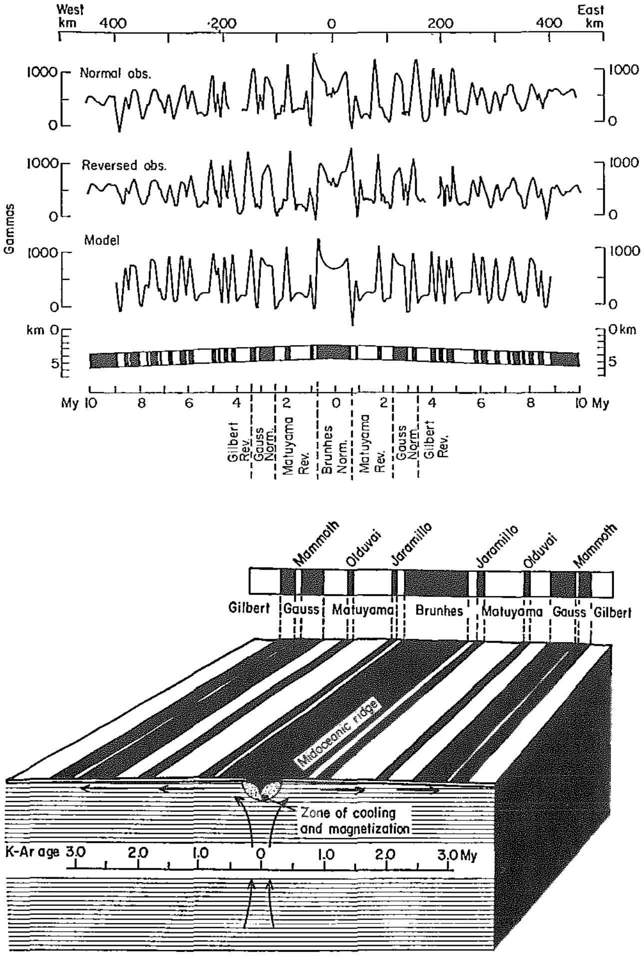
An even more interesting and puzzling fact about the magnetic field is that it not only drifts, it changes polarity frequently and relatively suddenly. "Normal" and "reversed" epochs have followed each other as far back in geological time as reliable measurements have been made. This was a great puzzle when it was discovered, and many people thought that the "reversed" observations were the result of mineralogical processes, which had been observed in the laboratory. As observations accumulated this thesis became untenable. Observations from all continents can be correlated to define the periods of reversal. It is now known that the reversals are the result of complex fluid motions in the earth's inner core[125].

There is an important consequence of changing polarity, initially observed in the mid-Atlantic rift. Many magnetic traverses had been made across the rift, which all showed a pattern symmetrical with the median valley as shown in the example below. Vine and Matthews[126] suggested that basaltic lavas, welling up in the central rift, absorbed the existing magnetic field. As they were displaced latterly by later flows they carried this remanent magnetism with them. The continuing upwelling lava would adopt the succession of polarities as they occurred. A sequence of symmetrical bands of normal and reversely magnetized lavas would spread out from the central rift, as shown in the figure on the following page. This pattern has since been observed in the other mid-ocean rifts.

The Observatory Program

The Observatory palaeomagnetic program began as an offshoot of the work on meteorite craters. When drill cores became available from the Holleford crater Beals insisted that the Geomagnetic Division build a magnetometer that could detect any magnetic material in them. A very sensitive astatic magnetometer had recently been designed by P.M.S. Blackett, of Manchester University, and an improved version had been produced by a team at Cambridge University.

S.K. Runcorn, a member of that team, was invited to come to Ottawa during the summer of 1955 to supervise the construction of a similar instrument here; Jean Roy, recently



The top figure shows in the upper trace the magnetic profile across a mid-ocean ridge; in the trace next below, this profile has been drafted in reverse direction to demonstrate the mirrored similarity on the two sides of the ridge; the third trace is an adopted model. The lower figure shows Vine and Matthews' explanation in terms of sea-floor spreading. Both diagrams indicate the periods of field reversals, black for normal field, white for reversed. These periods are recognized world-wide and have been given names, as indicated. From Earth Science Review, 5, 230, 1969.

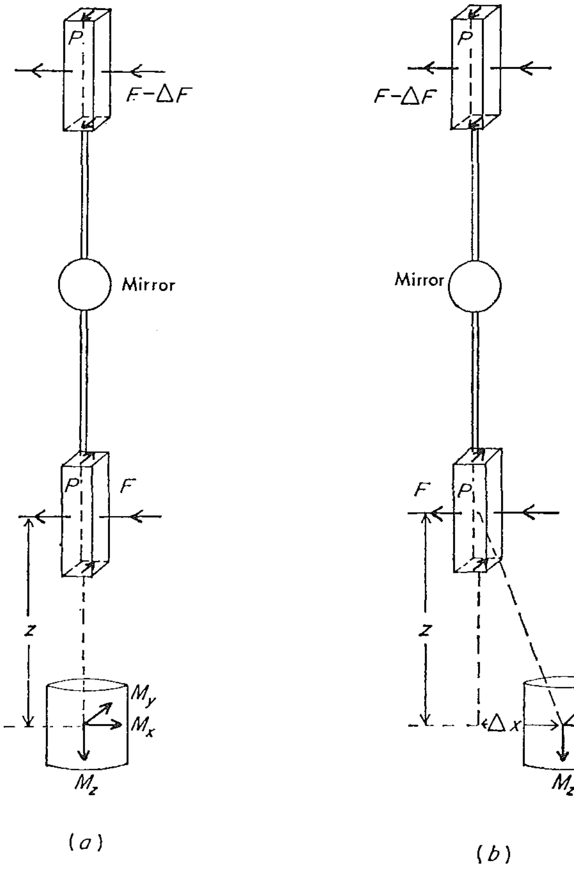
returned from a year as operator of the Baker Lake magnetic station, was assigned to work with him. The arrangement was not a happy one. Runcorn spent most of the summer collecting samples in various parts of the United States, and Roy was left very much on his own. With help from K. Whitham the instrument was completed with several design improvements. It was installed in a non-magnetic building at the Prescott highway site late in 1956.

Roy[127] has described the instrument, and the considerations that bore on its design. The principle is illustrated in the figure below[128]. The instrument consists of a rigid system made up of two highly magnetized rectangular bars, mounted on a rod so that the magnetization P is in opposite directions in the upper and lower bar. The assemblage is suspended on a torsion fibre of known constant.

A sample of the rock to be tested, in the form of a core, is brought up below the system as shown in the left-hand figure. Its magnetic effect is greater on the closer, lower, bar than on the more remote upper one, and the system will turn under the influence of the component M_X . Knowing the strength P of the magnetic field in the bars, and the torsion constant of the fibre, the strength of the component M_X may be calculated. By rotating the specimen through 90° M_Y becomes the active component and may be measured. Then, if the specimen is moved some distance out of the line of the suspended system, as shown in the right-hand figure, the component M_X and part of the component M_Z will contribute to the rotation. Since M_X is known, M_Z may be calculated.

The magnetometer is mounted inside a large system of Helmholtz coils designed to eliminate the earth's field at the magnetometer. Each of the coils has two windings. A steady current is passed through one winding to counteract the main field. In the second winding the currents are controlled by a nearby three component fluxgate magnetometer which senses the varying magnetic field and controls a current in the second winding to counter act it.

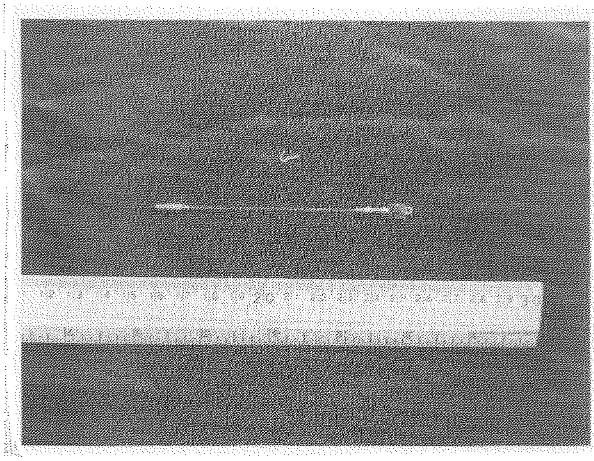
The sample to be tested is cut as a disk from a cylindrical core. Roy has shown[129] that the ratio of disk thickness to diameter has an optimum value of 1.25. It is mounted beneath the suspended system on a small turret which may be rotated, raised, or moved out of the central position by a system of



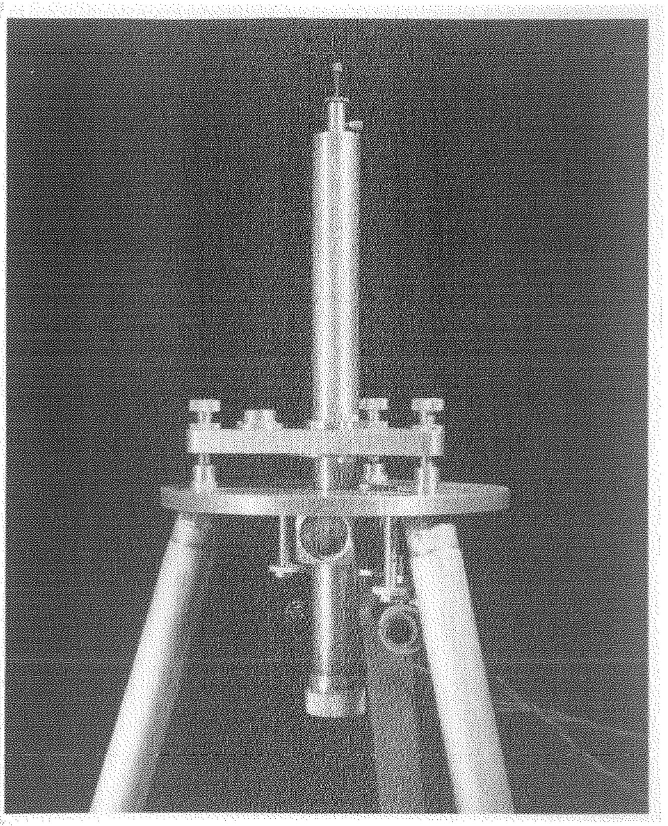
From M.W. McElhinny[128], 71, by permission of Cambridge University Press.

strings and pulleys that can be manipulated from a distance. Once the sample has been mounted, the operator need not approach the magnetometer until the measurements have been completed.

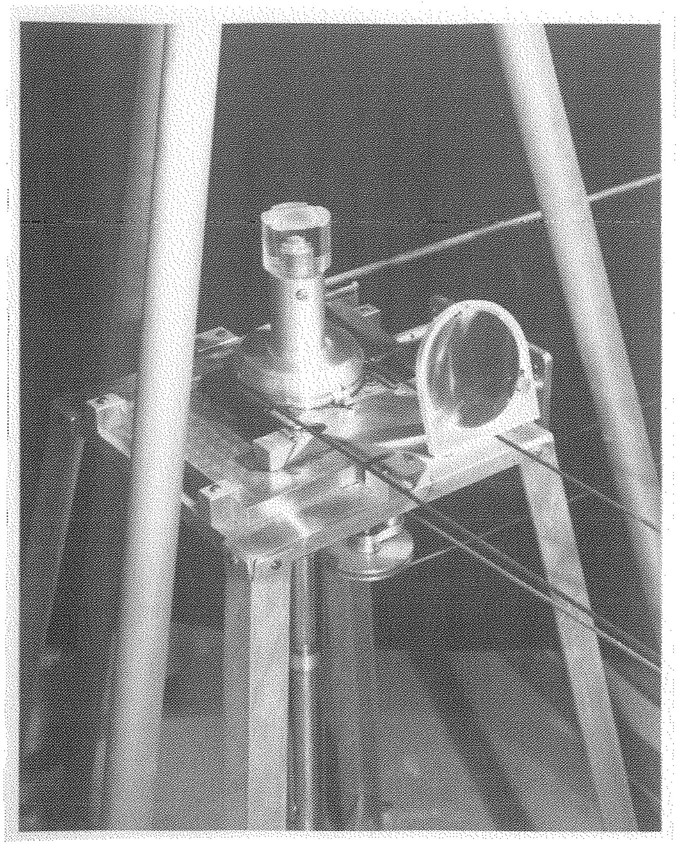
As we saw in an earlier chapter, the astatic magnetometer did not find any systematic alignment of magnetization in any of the crater cores tested. Roy was free to get on with more interesting matters. He began a study of the palaeomagnetism of Palaeozoic redbeds from Prince Edward Island, to be described presently, and collected a large number of samples, varying in age from Ordovician to Carboniferous, and including limestones from around Ottawa and red and grey sandstones and siltstones from the Maritimes. It was lonely work. A larger team was needed.



The magnet system of the astatic magnetometer.



The magnetometer case. The mirror can be seen through the window below the tripod head.



The specimen table, mounted immediately beneath the magnetometer. The several strings and pulleys which adjust the specimen position may be clearly seen.

Roy's isolation ended in 1964 when E. Irving joined the staff. Irving had been part of the pioneering team at Cambridge that had designed the improved magnetometer; from Cambridge he moved to the Australian National University where he was part of an active palaeomagnetic group. One of his students, W.A. Robertson, had come to Canada earlier, holding a postdoctoral fellowship at the Geological Survey from 1962 to 1964, at which time he transferred to the Observatory staff. While at the Survey he build an electric oven[130], modelled after one at the Australian National University, to be used in the thermal cleaning of samples. It was housed in one of the huts on the Prescott highway site.

The staffing of the section was completed in 1966 with the recruitment of J.K. Park.

Irving and Robertson had brought with them unpublished material from Australia. The publication of this was their first task[131]. Robertson next assisted in the study of the Manicouagan crater by studying the palaeomagnetism of the lavas found within it. This lava is supposed to have been formed at the time of the impact. When the average palaeomagnetic dipole was calculated and compared with the standard polar wandering curve its position was found to be in the Triassic, which is taken to date the impact[132].

Irving reviewed the samples that Roy had collected. He recognized that, at that time, there was little chance of getting accurate data from most older sedimentary rock other than redbeds. He helped Roy select suitable redbed samples from the Maritimes, mainly of Carboniferous age, and Roy prepared specimens, measured them, and did some trial demagnetization with Robertson's furnace. On the basis of the results, the team collected additional samples from a large number of rock units in the Maritimes during the summer of 1965. This collection, augmented as necessary, became the basis for Roy's research for the next several years.

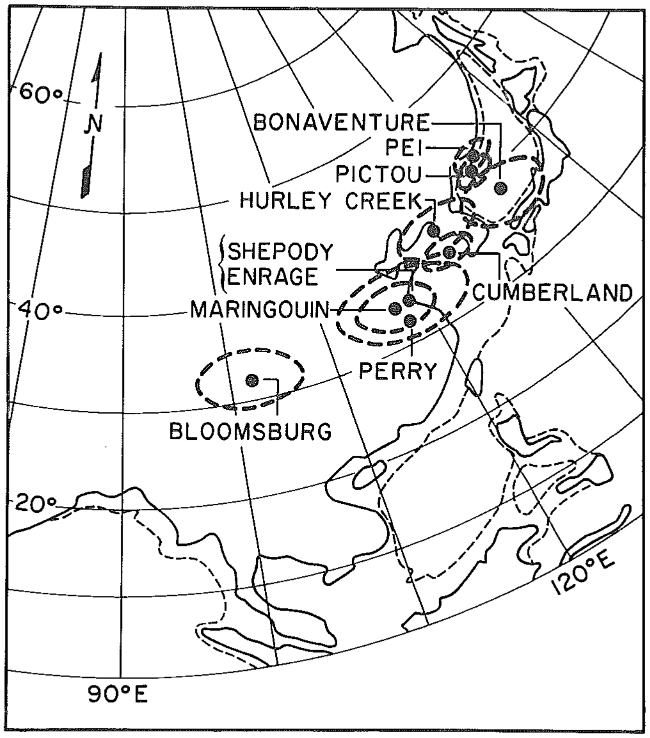
Palaeomagnetism of Upper Palaeozoic Maritime Rocks

This was a long and involved investigation. We shall consider the details of the first study to illustrate the techniques.

The first part of the work was completed before Irving's arrival. Roy sampled a number of flat-lying dark-red sandstones and soft red shales from the northern coast of Prince Edward Island. Geologists date the rocks as of late Carboniferous or early Permian[1] age. Samples were collected from seventeen sites· Because much of the rock was very friable, not all samples yielded usable specimens, but 60 survived from the 17 sites, as many as six from some sites, as few as two from others. The remanent magnetism was measured for each of them. By averaging the results of the several specimens a mean value of the palaeomagnetic field, expressed as the declination, inclination, and strength of the magnetic vector, was determined for each site. The inclinations ranged from 2° to 27° and covered a range of azimuth of 46°. The mean position of the palaeomagnetic dipole was 3.0°N, 123.2°E, a point in the Yellow Sea. A comparison of this pole with the standard polar wandering curve, confirmed the late Carboniferous or early Permian age of the source beds[133].

The scatter in the results suggested that the samples had been subjected to some secondary magnetic field, which might be removed by thermal cleaning. Additional samples were collected during the 1965 field program, from northern Prince Edward Island and from similar redbeds in two other locations, one on the Nova Scotia coast near Pugwash, the other on the Gaspé coast near the Baie des Chaleurs. When the cores had been prepared Roy had 109 specimens from 42 sites[134].

From these 109 specimen he chose 7 for thermal cleaning experiments with Robertson's electric oven. The oven operated inside a set of Helmholtz coils that maintained a null field; specimens could be heated to any desired temperature and then cooled in this null field to ensure that they did not acquire the present field. In Roy's experiments his seven samples were measured before heating, and after being heated to a series of increasing temperatures – 340°C, 425°, 505°, 535°, 600° – and cooled in the null field. The results varied somewhat from sample to sample, but in general the unstable components disappeared at about 400°C. Thermal cleaning to 450°C was accordingly applied to all specimens. There was not a marked improvement in consistency but the inclinations decreased systematically, and the mean declination changed by about 6°. Roy also noted that the cleaning had reduced the mean intensity[135].



Palaeozoic pole positions relative to eastern North America. The elliptical standard errors are derived from the standard errors of the site means. From Canadian Journal of Earth Sciences, 6, 667, 1969.

When the palaeomagnetic dipoles were computed for each group of samples they varied considerably, which Roy attributed to the fact that the pole had wandered a good deal over the long period, estimated as 100,000 years, during which the sediments were being deposited. Despite this, the poles established by Roy's work where more consistent then poles found earlier by other observers. Roy attributes this to the fact that in the earlier work the samples had not been thermally cleaned.

A series of papers followed[136], in which Roy was joined by Robertson and Park, in which the palaeomagnetic field was studied in rocks ranging in age from upper Devonian to late Pennsylvanian, a period of about 90 million years. The samples came from various parts of the Maritime Provinces. Each suite of samples presented special problems in separating the original field from later ones, but all required thermal cleaning.

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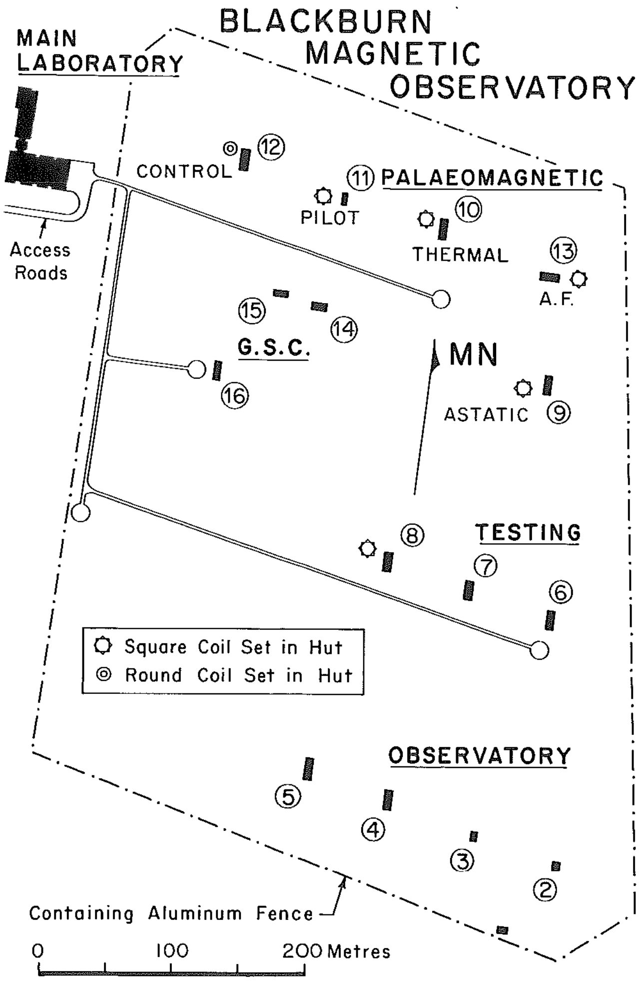
[1] See the Geological Time Scale on page 93.

When the study was completed it was possible to trace the movement of the magnetic pole during the upper Palaeozoic.

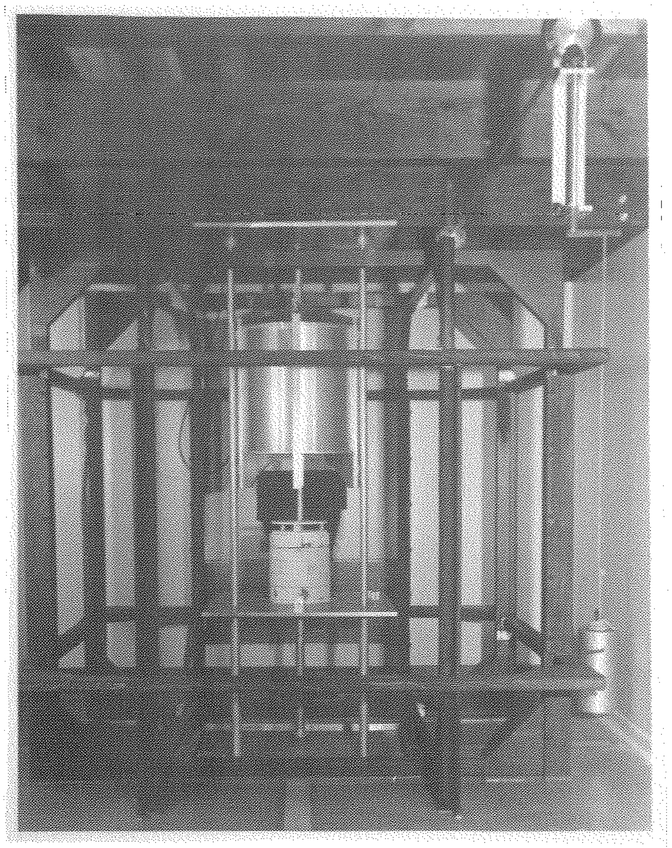
Before leaving Australia, Irving had cooperated in a study of Appalachian structure[137], that fitted in well with the Observatory's interest in the palaeomagnetism of the Palaeozoic. The study was extended after his arrival[138]. It provided a position of the palaeomagnetic pole in the Silurian, thereby extending the range of observations to some 135 million years. The migration of the pole, based on the Ottawa studies, is shown in the figure on the previous page. It amounts to 27°.

Park subjected some of the redbed rocks to chemical cleaning, a technique we have not previously met[139]. The beds are red because of the presence of haematite as a very fine-grained cement. Haematite also occurs as much larger black grains. What is the relevant influence of these two forms of haematite?

The specimens were subjected to a high magnetizing field and were then placed in a leaching solution of HCl and subjected to a continuing, small, demagnetizing field. The field remaining from the initial magnetization was measured at regular intervals. It was found that the red colour was leached from the rocks in about 18 days, and that the induced field was systematically reduced to a low level as the red haematite leached out. This low level represented the contribution of the black specularite grains of haematite. The process was true for a variety of formations although the more solid, well-cemented, specimens took longer to leach out and to lose their induced magnetism. The method clearly provides an effective method for separating the effects of the two forms of haematite. If the remanent magnetism of the leached material is now measured, that of the red haematite can be estimated as the differences in the unleached and leached specimen.



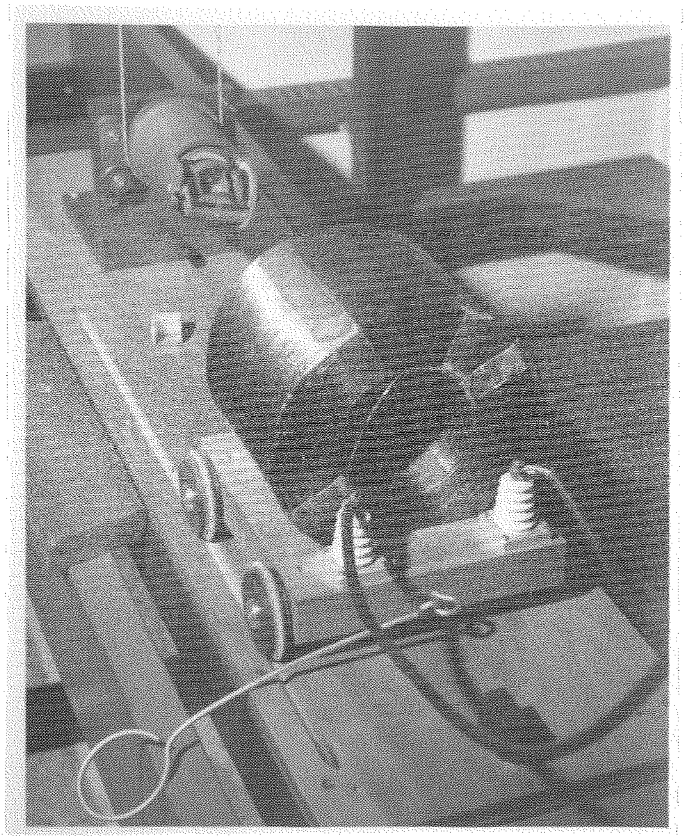
Layout of the various laboratories at the Blackburn Magnetic Observatory.



The electric oven used for the thermal demagnetization of samples. The samples are placed on shelves in the lower body and the electric oven is lowered over them. The surrounding Helmholtz coils provide the field-free environment in which the samples are cooled.

The Ottawa Magnetic Laboratory

In 1968 the Ottawa Magnetic Laboratory was ready for occupancy, and all work in palaeomagnetism was transferred to that site. It occupied five different buildings with a minimum separation of 60 m[140]. Each of the buildings was equipped with a set of Helmholtz coils to provide a field-free space for the instrument – magnetometer, oven, alternating field demagnetizer – housed therein. These coils had three sets of windings, the current in which was established in a control centre. The first windings, connected in series between the several huts, carried a current to compensate for the main field, the second windings, again connected in series, carried currents controlled by a three-component fluxgate



The tumbler and solenoid of the alternating field demagnetizer.

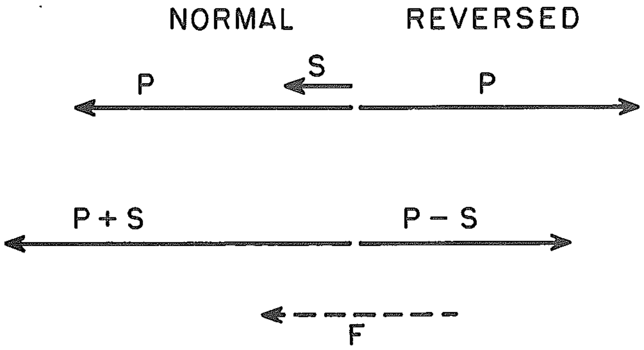
magnetometer mounted in the control hut. The third windings were independent to each building and were used to adjust for any small change from one building to another. A new astatic magnetometer was constructed[141]. Its design profited by the experience gained in operating the original and by the availability of new magnetic materials. By the use of feedback circuits in the electronic controls a variety of sensitivities could be obtained. A new electric oven for the thermal demagnetization of samples was also built[142]; it could take up to 120 standard samples in an essentially field-free environment.

To round out the new equipment an alternating field demagnetizer was constructed[143]. The sample to be tested is placed inside a non-metallic container that can be rotated by a system of gears in three orthogonal directions. This "tumbler" operates inside a solenoid that can produce fields of 2800 oersteds.

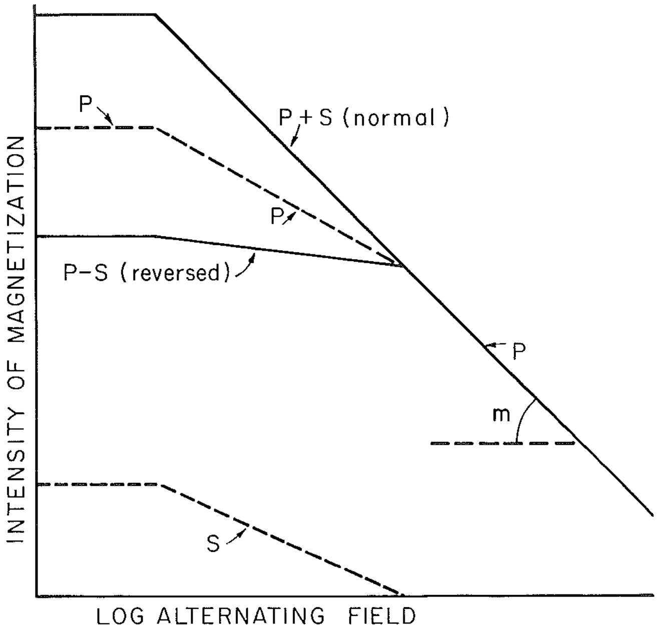
Magnetic Reversals and Seafloor Spreading

Irving and Roy[144] studied the remanent magnetism of a series of basalts of Upper Tertiary age found in Oregon and Washington, in which normal and reversed palaeomagnetism are both represented. An earlier study[145] had determined the intensities of magnetization of specimens of 75 lava flows from 23 localities, both before demagnetization and after demagnetization in an alternating current field of 200 Oersteds, and had established the before to after ratio. Irving and Roy considered the two determinations separately and found that, before cleaning, normally magnetized specimens had a field one third greater than the reversely magnetized specimens. However, after cleaning, the two were the same. They argued that this showed the influence of a viscous component due to the current, normal, field, on the stable primary component. The effect of this viscous component, S, is illustrated in the figure below; it would add to the strength of the primary field P for normally magnetized specimens, subtract from it for reversely magnetized ones, thus accounting for the difference. The cleaning of the viscous component S would have been gradual. The effect of an increasing alternating field is shown in the bottom figure for the cases of normal and reversed magnetization. The demagnification curves are quite different.

In interpreting the magnetic anomaly pattern associated with mid-ocean rifts, it had never been established that a surface pattern assigned to a zone of, say, normal magnetization,



From Canadian Journal of Earth Sciences, 5, 910, 1968.



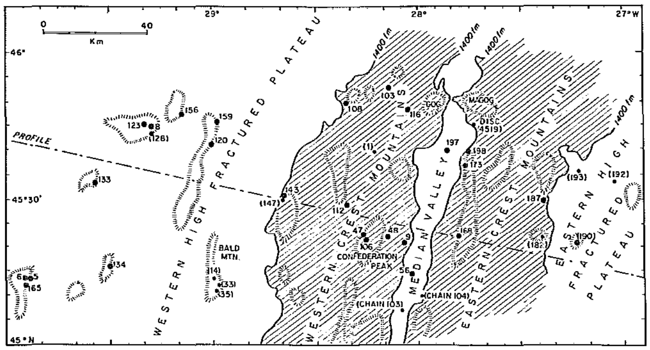
Demagnetization curves for normally and reversely magnetized specimens. Modified from Canadian Journal of Earth Sciences, 5, 910, 1968.

did in fact correlate with normally magnetized rocks. When the theory of seafloor spreading was being developed, only dredged samples were available; these were of course not oriented and their polarization could not be determined. Irving and Roy suggest that these lavas should be influenced by the viscous magnetism in the same manner as in the Oregon basalt and that it should be possible to deduce the nature of the primary magnetism – direct or reversed – by studying the demagnetization curve in comparison with that given in the figure above.

Irving tested the possibility[146] by "blind-fold" tests on five samples of known polarity supplied by the Geological Survey. He correctly determined the polarity in all five specimens. He points out however that the method is subject to error; ocean bottom cobbles may have changed position since their formation and so acquired a variable viscous component neither parallel nor antiparallel to the primary magnetization.

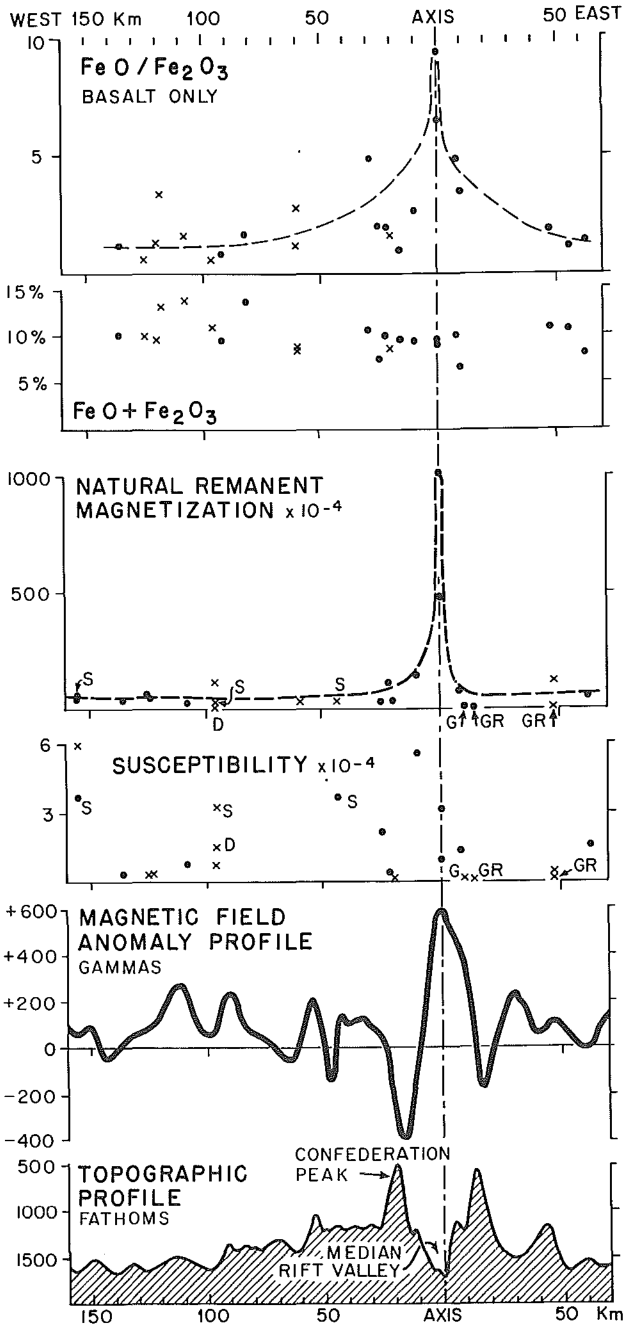
An opportunity soon arose to test the method. In 1966 the Bedford Institute began a detailed study of the Mid-Atlantic Ridge on a traverse along the 45th parallel. As part of this survey 27 dredge hauls were made covering a traverse from 150 km west of the Ridge to 70 km east. A total of 75 samples was obtained, most of them basalt, but 18 were erratics dropped from icebergs during the Pleistocene glaciation. These samples were studied jointly by Irving and Robertson of the Observatory, and Aumento of the Geological Survey[147].

We must understand something about the nature of the Ridge; the figure below will assist. The pattern consists of a very narrow median valley flanked on either side by crest mountains of an average width of 50 km, the peaks of which rise 1800 m above the floor of the median valley. Beyond the mountains lie high fractured plateaus. It is generally agreed that there are three crustal layers present: Layer 1, thin and often absent, consisting of sediments; Layer 2 consisting chiefly of basalt, a few hundred metres thick, in which the magnetic signatures characteristic of the ridge occur; and an underlying Layer 3 representing the normal, oceanic crust, essentially non-magnetic. Layer 2 is supposed to have its origin by outpouring along the rift, and the rift is taken to mark the boundary between the two plates.



From Canadian Journal of Earth Sciences, 7, 227, 1970.

The samples were analyzed magnetically and chemically; the results are illustrated in the figure below. The bottom diagrams show the topographic and magnetic profiles across the feature. The next gives the magnetic susceptibilities, which is a measurement of how likely the samples are to have been contaminated by the existing field. They appear to be random and in any event are small. The diagram next above shows that the remanent magnetism in the central rift is nearly 20 times that immediately away from it. How is this to be accounted for? It might be due to a large increase in the magnetic field in recent times, but it is known from other



From Canadian Journal of Earth Sciences, 7, 230, 1970.

evidence that this has not occurred. It may be due to chemical changes that have destroyed the magnetization of the older, more remote, samples. A clue may be given in the two top diagrams. The total percentage of iron minerals is reasonably constant across the profile, but the ratio of FeO to Fe_2O_3 increases remarkably at the median. These facts suggest the low values of remanent magnetism away from the central rift may be due to the oxidation of the iron minerals.

The paper does not address the question of normal or reversed polarization, but this is taken up in a second paper[148] in which the method, proposed earlier, of studying the demagnetization curves, was applied and extended. The authors were able to infer the direction, normal or reversed, of the primary component. These proved to be all normal in the median valley, whereas both normal and reversed polarities were found in samples from the adjacent mountains and plateaus. This supported the accepted picture.

J. Brooke and R.L.G. Gilbert of the Bedford Institute developed a remarkable deep-sea drill[149] which was used during the 1969 cruise. "The drill is powered by releasing sea-water at bottom pressures through turbines into reservoirs at atmospheric pressure. Drilling continues until the reservoirs are filled, the power cycle being limited by the size of the reservoirs". The drill takes a core 2 cm in diameter and, depending on the hardness of the formation, up to 1.7 m in length. During the 1969 cruise it produced three cores from the crest mountains, one to the west, two to the east[150]. The demagnetization method was used to determine the nature, normal or reversed, of the magnetizing field. Since the cores were oriented, these results could be checked against direct measurements; they were correct 11 out of 12 times.

The results of all the measurements, from dredged samples and drilled cores, were summarized in a short paper in Nature[151], and an interpretation of the results in terms of the magnetic properties of submarine basalt was provided by Irving[152]. The magnetic properties of submarine basalts are quite different from those of continental ones in that they exhibit much greater remanent magnetism. Irving attributes this to the quenching of the lavas at sea bottom temperatures which leads to small grain size as compared with those in slow cooling continental lavas. But the rapid quenching makes for instability of mineralization. With time, the chemical ratio of FeO/Fe_2O_3 decreases and so does the remanent magnetism, confirming the earlier conclusions.

By the close of our period of interest the state of knowledge of earth processes was in a somewhat confused state. Polar wandering curves had been established for most continents; these diverged increasingly with the geological age of the rocks studied, and a pattern and rate of continental drift could be determined in this way. On the other hand, magnetic reversals had led to the hypothesis of ocean floor spreading, the direction and rate of which could be determined. These did not agree with the pattern shown by polar wandering nor of continental movement. Irving and Robertson examined this problem[153]. Considering both sets of data, and admitting the possibility that the mid-ocean rifts had shifted position, they concluded that rapid polar wandering was limited to widely separated periods, between which there were periods of low drift in which the direction of the field remained constant but might undergo frequent reversals.

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Dr. E.A. Hodgson, Chief of the Seismology Division, 1919-1952.

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XIII – SEISMOLOGY

CANADIAN SEISMICITY AND THE CANADIAN SEISMOGRAPH NETWORK

The Seismograph Network

A Note on Instruments

Seismic waves cover a broad range of periods, from a small fraction of a second to several hundred seconds. The shorter periods are produced by explosions and near earthquakes and by the initial, longitudinal, waves from distant earthquakes. The transverse waves from distant earthquakes have periods of the order of seconds; surface waves from distant earthquakes have periods of tens or even hundreds of seconds. A well-equipped station must have a complement of instruments to cover the complete spectrum.

During the period covered by Part I of this history most instruments – the Bosch, Mainka, Milne-Shaw – were designed to cover the middle range. The magnification peaked at something like six seconds, and dropped off in both directions to become near unity below one second or above twenty. Most of these early instruments had mechanical or optical recording, and their magnification was limited, usually not more than a few hundred times.

These instruments were successful in recording large distant earthquakes. Epicentres could be located reliably and the records permitted a detailed study of the earth's interior. They did not provide adequate information on near earthquakes or on surface waves; both of these studies were necessary for an understanding of the earth's surface layers.

Two seismographs were developed to meet the short-period problem, both of them at the California Institute of Technology. The Wood-Anderson seismograph, which involved a vibrating string, appeared in the mid-1920s. It had a fundamental period of about two seconds with a peak magnification of 2800, much better than its predecessors, but still lacking high magnifications at periods less than one second, necessary for recording the short-period range. The stations at Shawinigan Falls and Seven Falls, established in 1927 after the 1925 St. Lawrence earthquake, were equipped with an early version these instruments.

In the mid-1930s Hugo Benioff produced an instrument that revolutionized earthquake recording. It consisted of a heavy mass with electromagnetic transducers, which drove two galvanometers, of short and long period. As a short-period seismograph it covered the spectrum from a few tenths of a second to about two seconds with a peak magnification, at 0.8s, as high as 100,000; as a long-period seismograph it covered periods from a few seconds to as much as 100 seconds. The instruments were available in both vertical and horizontal components. They became world standards.

In 1948 the Observatory began a study of the earth's crust, using rockbursts at Kirkland Lake as an energy source[36]. The short-period Benioff seismograph had an ideal period range for this work, but was much too heavy to be used as a field instrument. We attempted to use Sprengnether short-period seismographs for the purpose. This instrument had been designed as an intermediate-period instrument for the study of microseisms. Later both short- and long-period versions were manufactured. The short-period instrument had a peak magnification of about 3000 at a period of one second. This was much inferior to the Benioff, but the instrument was light enough to be used in the field. In the immediate post-war period several sets, both short- and long-period, were purchased by the Observatory.

The short-period Sprengnether proved quite unsatisfactory for recording rockbursts, since its period range did not extend to very short periods. P.L. Willmore, a graduate student at Cambridge University, had developed an instrument of a radically new design, small enough to be carried in one hand, but having, for short periods, nearly the same magnification curve as the Benioff. It could be used as either a vertical or horizontal seismometer. One of his instruments was made available to us, was copied, and used throughout the rockburst project. The Willmore seismometer later became available commercially and was widely used, both as a field instrument and as a short-period station one; it became our standard instrument for both purposes.

The development of the Canadian network will now be traced with the foregoing information in mind.

The Developing Network, 1948-1958[1]

At the close of the Stewart Administration the Canadian seismological network consisted of six stations – Victoria, Saskatoon, Ottawa, Shawinigan Falls, Seven Falls and Halifax – all in the settled areas of southern Canada and, except for Ottawa and the Quebec stations, inadequately instrumented. The installation of the Benioff at Ottawa and the Wood-Andersons at the Quebec stations had been the result of a sequence of major earthquakes. There had been no equivalent impetus on the West Coast, despite the fact that Canada's coast was the only part of the Pacific rim that had had no serious earthquakes since at least the 1850s. The Vancouver Island earthquake of June 1946 (magnitude 7.2) showed that a hazard did exist and steps were immediately taken to obtain a Benioff seismograph for Victoria. The instrument was installed in June 1948[2].

The seismograph stations installed as part of the rockburst project provided some additional short-period stations during the years 1948 to 1951, initially with Sprengnether seismographs, later with Willmores. The Sprengnether short-periods were then available for other purposes.

One Sprengnether vertical was operated at Kirkland Lake, initially to time the rockbursts at their source, subsequently as a regular station of the Canadian network. It was of limited use for the study of local earthquakes, but it was an excellent instrument for recording teleseisms. I always read the records from Kirkland Lake myself and a large number of teleseisms were recorded which did not appear at any other Canadian station. C.F. Richter, an authority on world seismicity, told me much later that not only were "my" teleseisms not recorded elsewhere in Canada, they were usually not recorded elsewhere in North America. However, they did agree with observations in other parts of the world and with known earthquakes.

The problem on the West Coast became more acute once the Benioff seismograph was installed, because it indicated a relatively high level of local, mid-magnitude, earthquakes but without the ability to locate the sources[3]. Worse, large earthquakes continued to occur; in April 1949 an earthquake of magnitude 7.1 did more than a million dollars damage in the nearby state of Washington, and a few months later, in August 1949, the Queen Charlotte Islands were struck with an earthquake of magnitude 8.1. Clearly something needed to be done to improve our coverage of West Coast earthquakes. W.G. Milne was transferred from Ottawa to the Dominion Astrophysical Observatory to take charge of an expanded program. This was the beginning of a geophysical presence at the DAO. Milne was joined by W.R.H. White in 1954 and Sandra S. Meidler in 1960. We saw in the last Chapter that geomagneticians Caner and Auld joined the group in 1957 and 1960 respectively.

Since the rockburst project had been completed, the instruments, and the portable huts that housed them, were available. They were transferred to British Columbia during the summer of 1951 and installed at Alberni and Horseshoe Bay. With the existing station at Victoria they provided a triangle of high-sensitivity stations in the most populated area. The stations were operated by local residents.

The three stations allowed the accurate location of earthquakes within the triangle, with increasingly less accurate location of epicentres with distance outside it. For earthquakes south of the network, data from United States stations were of assistance. Annual papers were published listing the earthquakes, numbered chronologically, giving the recorded data for each of the stations and, where possible, the epicentre and any felt reports. Changes in the details of instrumentation, usually involving adjustment of periods to meet local noise conditions, were also given.

In 1953 a new office building was completed at the Dominion Astrophysical Observatory and a new vault, dark room and office space were made available for seismology. A complete set of Benioff seismographs, vertical and horizontal, short- and long-period, was purchased and the existing Benioff vertical was transferred to Seven Falls, Quebec, where it was installed in October 1953[1]. Unfortunately, due to delay in delivery, there were no high-magnification instruments at Victoria for a period of about three months.

In late 1953, in cooperation with the Mines Branch, Department of Mines and Technical Surveys, three stations were installed to record "bumps", small rockbursts, originating in the coal mines of the Crowsnest Pass area of southeastern British Columbia. These stations, at Fernie, Coleman and Turner Valley, operated in a way that marked a new departure for the Division. P.L. Willmore had joined the staff in the summer of 1952 and brought with him new ideas about field equipment. The Crowsnest Pass stations were equipped with Willmore seismometers and portable recorders that did not require dark recording rooms. This equipment, manufactured by Hilger and Watts of Great Britain, was a great advance over the cumbersome stations used in the earlier rockburst project.

The stations were operated from late 1953 until mid-1955 and located some local earthquakes, thus extending coverage to the southern interior of British Columbia. The project failed in its aim of locating the rockbursts; the separation of the stations was such that rockbursts never recorded on more than one of them at a time[4]. It was therefore decided to concentrate on the Fernie area and to use a much smaller triangle of stations. Two of these were linked to the third, central, station by short-wave radio; this was the first use of radio-linked sensors by the Division, a technique which was to become commonplace.

A number of disturbances were recorded on the Fernie network and, although the wave velocities for the area were not known, it was possible to prove that the events did originate within the workings. The stations were then moved to the Coleman area with equal, limited, success. The hope of the Mines Branch personnel that the "bumps" could be accurately located in three dimensions was not realized.

It was not just in the Crowsnest Pass area that the velocities were unknown; the situation was the same in the main earthquake program. Travel-time curves had been developed for eastern Canada in the rockburst program[36] and while there was no reason to suppose that these would apply to the Pacific coast, they were certainly much better than the Jeffreys-Bullen near-earthquake tables, which were based on values of crust and upper mantle velocities by then known to be incorrect. The Canadian Shield P velocities were therefore used, and sets of curves were computed giving the loci of epicentres corresponding to P arrival-time differences of 1, 2, 3, etc. seconds between pairs of stations. The three sets of curves permitted a reasonably accurate location of the epicentre. This location could then be refined by the use of S-P intervals if the S wave was well recorded, and the most consistent time of origin determined. For the larger earthquakes, data from neighbouring United States stations were added.

It was not only in the west that the network was advancing. It had been recognized for many years that the vast Arctic regions of Canada represented a major gap in the world network of seismograph stations, but the logistics of the situation were beyond the resources of the Observatory. The establishment of a Joint Weather Station at Resolute Bay on Cornwallis Island in 1947, with all the associated air and sea transport and the provision of housing and power, made it possible to consider seriously the installation of a seismograph station. P.C. Bremner, who had worked as a summer student in the rockburst program, was sent to Resolute during the summer of 1948 to investigate the possibilities. He found a rock outcrop at a distance of 1400 feet from the power station and base camp; it was not considered a feasible location because of the impossibility of servicing the station during winter storms. Bremner suggested that the seismometers be located at the bedrock location, the recorders close to the base camp, the two to be connected by a buried cable.

Bremner joined the Observatory staff during the summer of 1949 and during the following winter ran field tests of the proposed system, using a 1600-foot cable to connect seismometers on a concrete slab south-east of Observatory House to a recorder in a "rockburst project" shelter on the Observatory grounds. Temperatures as low as -20°C did not cause any problems. Various instruments were tested.

By July 1950 Bremner and a summer assistant, R.E. Andrews, were in Resolute Bay with all their equipment. The instruments were installed on the outcrop, housed in a small shelter buried in gravel to reduce the effects of wind. A buried multi-conductor cable connected the instruments to the recording hut, conveniently located near the camp. It was a double-walled, heavily insulated building, prefabricated in Ottawa. The recorder and galvanometers were set on rigid steel tables, the legs of which passed through the floor to rest on a heavy framework of cribbing, frozen into the permafrost.

A variety of instruments were tested, but the installation eventually consisted of a short-period Sprengnether vertical and a pair of long-period Sprengnether horizontals. A power cable, buried with the instrument cable, supplied two 500-watt heaters, controlled by a thermostat set at 50°F[5].

The station was in operation by August 20, 1950. Bremner remained in Resolute Bay as station operator during the winter of 1950-51, being replaced by Andrews during the following year.

Some seismic activity had been anticipated within the recording range of the station since earthquakes had occasionally been felt at other Arctic stations. During the first year of operation however only five local earthquakes were recorded, and of course they could not be located. Subsequent experience showed considerable local activity; the low numbers recorded during the first year were undoubtedly due to the relatively long period and low magnification of the Sprengnether vertical seismograph.

During the same period improvements were being made in the stations of southern Canada. A pen-recording seismograph designed by Leet and Blumberg of Harvard University was purchased in 1951 and continued in operation for about three years[1]. It produced interesting records but was difficult to maintain. The seismometer was in the vault, the recorder in the office, and one could not observe the effects of any adjustment without trudging up the stairs. The three-component inertial element involved a system of servo-mechanisms that sensed and corrected for any tilt; since these mechanisms had a tendency to hunt the trace was not very stable. The recording was done on reels of papers, with no drum translation, so that the record of a major earthquake was very long, and consequently difficult to read and to store. It was however exciting to see what was going on as it happened. One could even hear large earthquakes, for the pen screeched as it moved rapidly across the paper.

Some time in 1951 discussions were begun with Dalhousie University about improvement of the Halifax station. We wanted a new vault, separate from the Physics Building; the bedrock was so close to the surface that a surface building would be required. The University would have nothing that would conflict with its architectural plans for campus development. As a consequence the building had to be made of stone to match the other buildings and to be finished in accordance with the general plan, right down to a false lantern on the roof. Operation began in 1952, with Benioff short- and long-period verticals and Sprengnether long-period horizontals.

W.E.T. Smith and John O'Connor with the Leet-Blumberg pen-recording seismograph.

The station at Kirkland Lake was closed in June 1957, that at Saskatoon in March 1960. Neither location was suitable for the modern stations then being planned.

The National Network, 1958-1970

The year 1958 may be taken as a watershed in seismology, marking the beginning of a tremendous expansion in all branches of the science. There were perhaps three factors responsible for this. The IGY was the first of these. While it was designed primarily to study the earth's fluid envelope, seismology was not excluded. One important project, involving the study of surface waves, was carried out by the Lamont Geological Laboratory of Columbia University, New York, using funds supplied by the United States government. Surface waves are influenced by the nature of the crust, and must be studied over purely oceanic or purely continental paths. The existing world network of stations did not provide such paths. Lamont placed long-period instruments, especially designed for the purpose, at key stations appropriately chosen. The experiment established the fact that seismologists, given adequate funds, need not be content with the haphazard arrangement of stations and equipment, but could set out international networks for special purposes.

A second influence, which began to become important at about the same time, was the general availability and increased capacity of high-speed computers.

From the point of view of our present interest, the most important influence was the search for a means of monitoring a nuclear test ban. In 1958 military and political leaders of the USA, the USSR, and Great Britain met in Geneva to arrange a ban on the underground testing of nuclear devices, but the vexed question was how such a ban was to be policed. The delegates turned to seismologists for an answer. After much discussion it had to be admitted that they didn't have an answer, and couldn't provide one unless they were given vastly increased resources to develop their techniques.

P.L. Willmore attended these meetings as an observer and he returned to Canada convinced that we must fill up the gap in the world's seismic network represented by our large land mass, the second largest in the world. He drew up a proposal for a network of uniformly equipped stations spaced 500 miles apart. I was in Europe in 1958-1959 on a sabbatical year and Willmore was acting Division Chief. He sold his plan to the Director and to Director-General W.E. van Steenburgh and, with their help, to the Treasury Board. It was agreed that we could proceed with the network, installing two stations a year. Considering the logistical difficulties involved, two stations per year were as much as we could manage; the approval was all we could hope for.

The same impulse that led to the Canadian initiative resulted in the establishment in the United States of the Vela Uniform program, which supported a great variety of research projects aimed at improving the contribution of seismology to the problem. Among other things, it funded a program under which 125 sets of identical instruments were given to stations throughout the world. The resulting network was known as the World Wide Standard Seismograph Network (WWSSN). Canada was offered instruments but, since our plans were already well advanced, we declined.

However an effort was made to obtain uniformity of instrumentation. The Vela Uniform program had selected as standard long-period instruments seismographs developed at Lamont, an improvement of the instruments, already mentioned, used during the IGY. The same instruments were adopted for the Canadian network. For short-period instruments we had selected the Willmore system; the Vela Uniform program used Benioffs. This was largely based on considerations of logistics; given our distances and the problems of transportation the small size of the Willmore was a great advantage. The magnification curves of the two instruments were similar, the Willmore having a somewhat broader peak, with larger magnifications at the shorter periods.

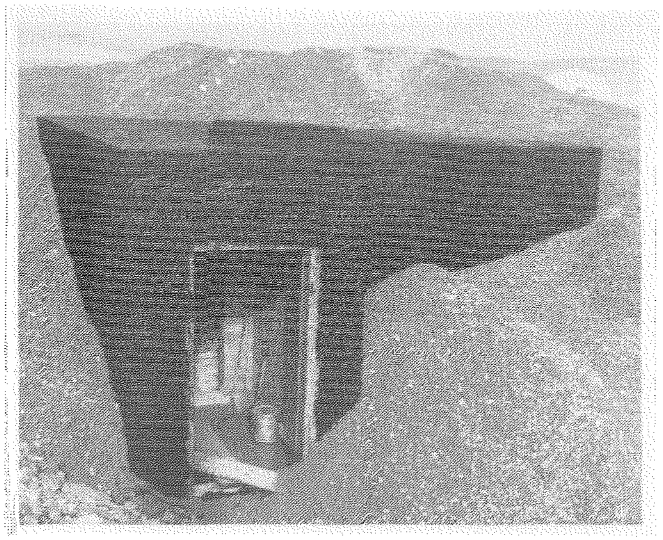
If a network of some twenty stations was to be installed across Canada, it was evident that we should have to have some help in its operation. We approached the Meteorological Service for permission to place the stations on their existing sites and to have them serviced by meteorological observers. They agreed, but insisted that they must have freedom to move their personnel around. This meant that all their meteorological observers must be trained in seismology. Of this more later.

Since observers might be moved from one station to another it was obviously desirable to have a standard station. The recording room might be on the surface or buried underground, the operator might approach it from the surface or by a cellarway, but once he entered the recording room he would find things as he knew them. The design was carefully considered and then adopted as standard. Nearly all the new stations conformed to it, and a standard training vault was built at Scarborough, Ontario, on the site of the Meteorological Training Centre.

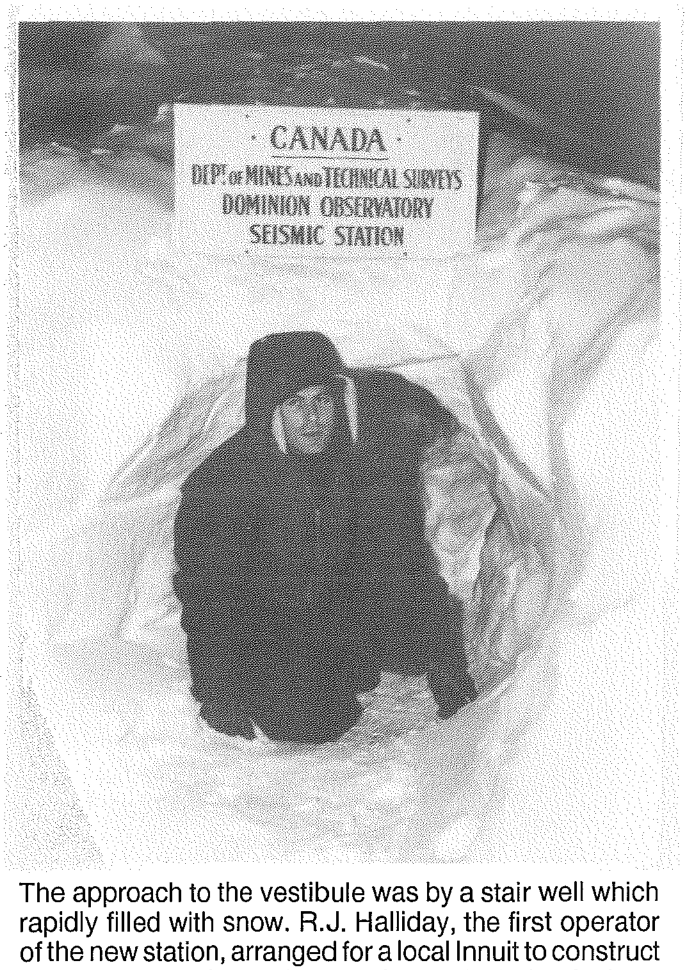
The design of the standard station was based on experience gained at Resolute[6], where a new station was installed in 1957. In building the station, permafrost was both a problem and an asset. The vault was built on bedrock, which lay at a depth of eight feet. The excavation was scraped out over a period of two years. Each August a bulldozer would remove soil every week or ten days as the permafrost melted. Bedrock was reached in August 1957. The vault was then constructed, and the excavated material was returned to surround and cover it; this quickly froze, leaving the station permanently enclosed in permafrost. A vault temperature of 30°F was maintained to ensure that the permafrost would not melt.

Willmore installed the station; he experimented with various instruments, in particular the three short-period verticals – Willmore, Benioff, and Sprengnether – and it was on the basis of these tests that the Willmore was chosen as the Canadian standard. The final instrumentation at Resolute consisted of Willmore short-period vertical and horizontals, Sprengnether long-period horizontals, and the very long period vertical and horizontals installed by the Lamont Geological Laboratory as part of its IGY program.

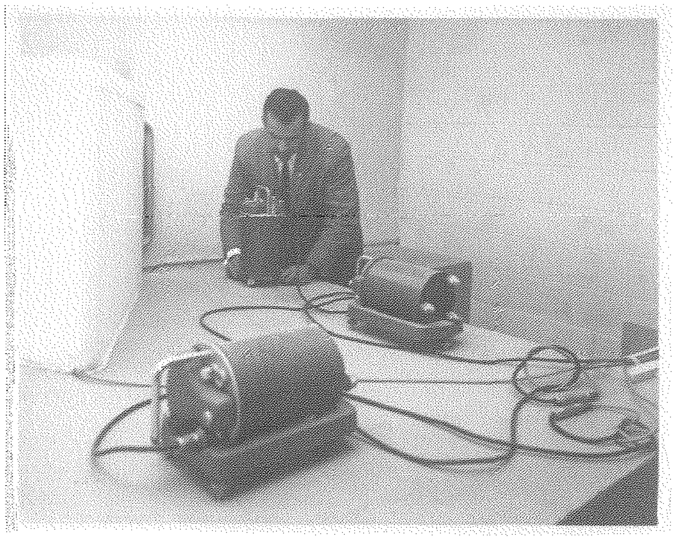
The tedious excavation for the Resolute vault.



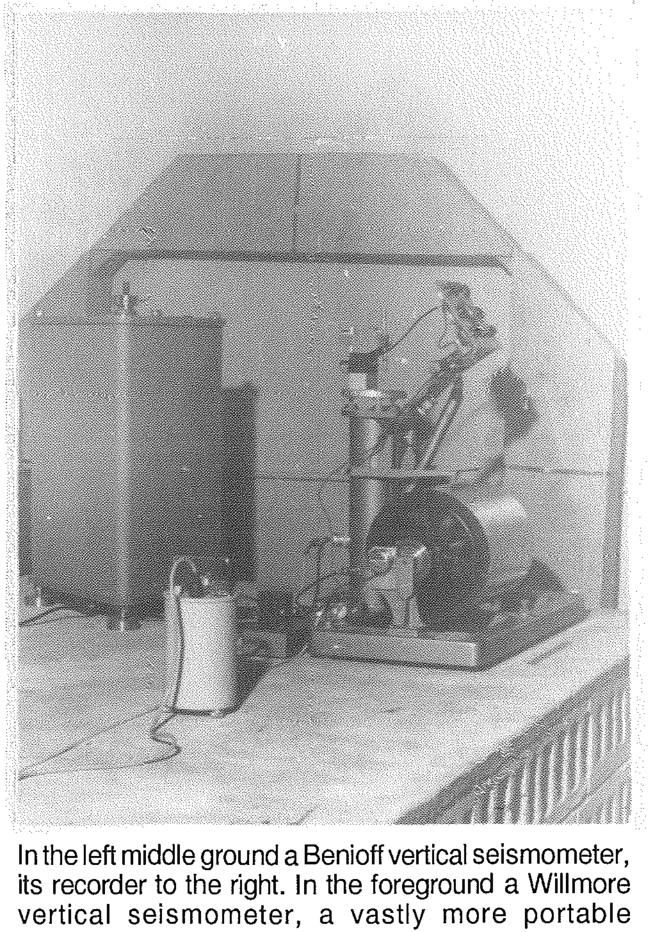
The vault completed, and the back-fill begun. The entrance is through a vestibule which acts as a light trap. An escape hatch in its roof protected the operator from being trapped by heavy snow.



The approach to the vestibule was by a stair well which rapidly filled with snow. R.J. Halliday, the first operator of the new station, arranged for a local Innuit to construct an igloo over the staircase, from which he is here emerging. A permanent shelter was build the following summer.



The interior of the Resolute vault. Frank Lombardo adjusts the vertical Willmore seismometer; the two horizontal components are seen in the foreground. The styrofoam case encloses one of the long period seismographs, which were very sensitive to temperature effects. Lombardo and Halliday became the heart of the trouble-shooting team that managed the completed network.



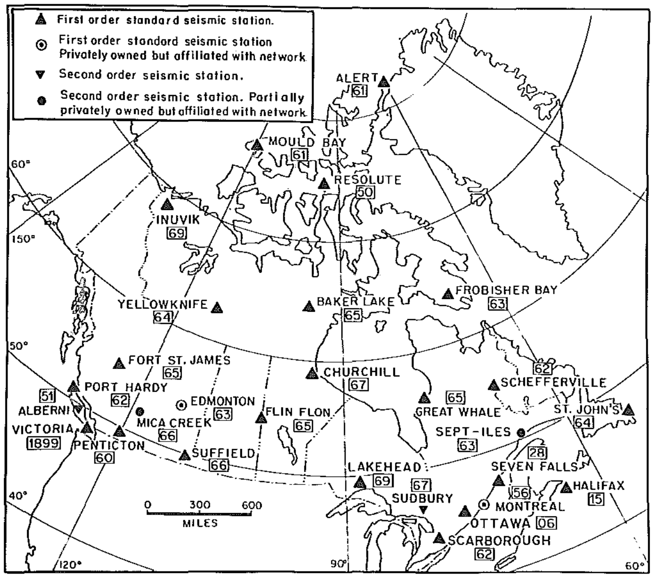
In the left middle ground a Benioff vertical seismometer, its recorder to the right. In the foreground a Willmore vertical seismometer, a vastly more portable instrument. The octagonal case in the background contains the coil of a magnetometer being operated for the California Institute of Technology.

At about this time Willmore developed a portable device for the rapid and accurate calibration of electromagnetic seismographs[7]. It involved the use of a Maxwell impedance bridge; the seismometer was connected across one arm of the bridge, the galvanometer across the output. A signal generator drove the seismometer, through its coil, at a sequence of frequencies, and the output was measured by the galvanometer. After the prototype had been thoroughly tested a licence to manufacture the package was given to Hilger and Watts, who also manufactured the Willmore seismometer.

The bridge provided an accurate calibration for the instruments of all the new stations. It was portable; a technician had only to carry it and a signal generator to the station in order to provide detailed calibration curves covering the entire seismic spectrum. It was used to calibrate the instruments of both the old and new stations at Resolute. It was found that the constants of the instruments at the old station had drifted badly.

With the experience gained at Resolute, and with the long-term commitment of Treasury Board, the development of the network went ahead apace. Combined magnetic and seismograph stations were installed at Alert and Mould Bay in the high Arctic in 1961 and stations were established farther south at the rate of two or three a year. The figure below shows the state of the network in 1970.

This network was not achieved without some frustrations. A standard station was installed at Coppermine in 1963. In 1969 the Meteorological Service decided to close its station there; without their logistical support it was not possible to continue to operate the seismograph station. It was closed, and a station at Inuvik, 750 km to the west, was opened in the same year with an overlap of some six months. Elsewhere a number of locations proved unsatisfactory and plans for permanent stations had to be cancelled. A proposed station at Meanook, for which plans were well advanced, was abandoned when the University of Alberta announced plans for a station at Leduc.



The Canadian Seismograph Network, 1970.

An interesting experiment was conducted at the Coppermine station. The configurations of the Canadian standard instruments and those of the Vela Uniform program were not identical and it was arranged to run instruments of the two sets, side by side in the same vault. Except for the fact that the Willmore short-period instruments showed slightly lower amplitudes for P waves, the outputs were identical. The experiment lasted during the life of the station. An attempt was made to purchase a USSR standard set of instruments to be included in the experiment. The manufacturer quoted an exorbitant cost for instruments, something like four times the cost of the Vela Uniform systems, and the matter was dropped.

The stations were managed under a variety of different systems. Those at Edmonton and Montreal were independent, owned respectively by the University of Alberta and the Collège Jean-de-Brébeuf. The latter station had cooperated closely with Ottawa from its inception in 1956. A payment was made to the University of Alberta sufficient to pay the salary of a technician, who maintained and reported on the standard instruments as well as performing other duties for the University. A similar arrangement was made with four other universities: Memorial at St. John's, Dalhousie at Halifax, McGill at Schefferville, and Lakehead at Thunder Bay. Suffield was the site of a Defence Research Board experimental station and the vault there was built, and the station maintained, at its expense. Five stations – Port Hardy, Inuvik, Churchill, Frobisher and the training vault at Scarborough – were operated by the Meteorological Service. Observatory personnel staffed the remaining stations.

The map shows a number of "second order" stations, later called "regional" stations to better indicate their function. They were installed to monitor local earthquake activity and were operated by a variety of interested organizations, Sept-Iles by the Iron Ore Company of Canada, Mica Creek by the British Columbia Hydro Authority, Sudbury by Laurentian University. The regional stations all used Willmore seismometers recording, with electronic amplification, on hot-wire Helicorders.

One other change should be mentioned. The Ottawa vaults, built during the King Administration, were too small for the increased complement of instruments they were expected to hold, they were damp, and there were ominous hollow reverberations when the walls were tapped. New vaults were needed. By this time the Seismological Division was preparing to occupy the old Geodetic Survey building. A tunnel was built to connect it to the main building and two vaults were constructed on bedrock, one off either side of this tunnel. Work commenced in November 1961 and immediately ran into trouble. The tunnel crossed the fault scarp on which the Observatory is built, the rock was badly fractured, and apparently unlimited water under pressure gushed into the vaults as they were excavated. It took more than a year to complete the vaults, and since the old vaults had been destroyed in the construction, the Ottawa station was shut down for 13 months[1].

The old vaults, whose soundness had been questioned, were very resistant to the wreckers' hammer. They clearly would have lasted for another fifty years.

New methods had to be found to handle the vastly increased flow of data produced by this expanding network. Under the pre-expansion system all records came to Ottawa and everyone was responsible for reading the records from one or more stations. Because there were more interesting things to do, the readings and the resulting bulletins were usually much in arrears.

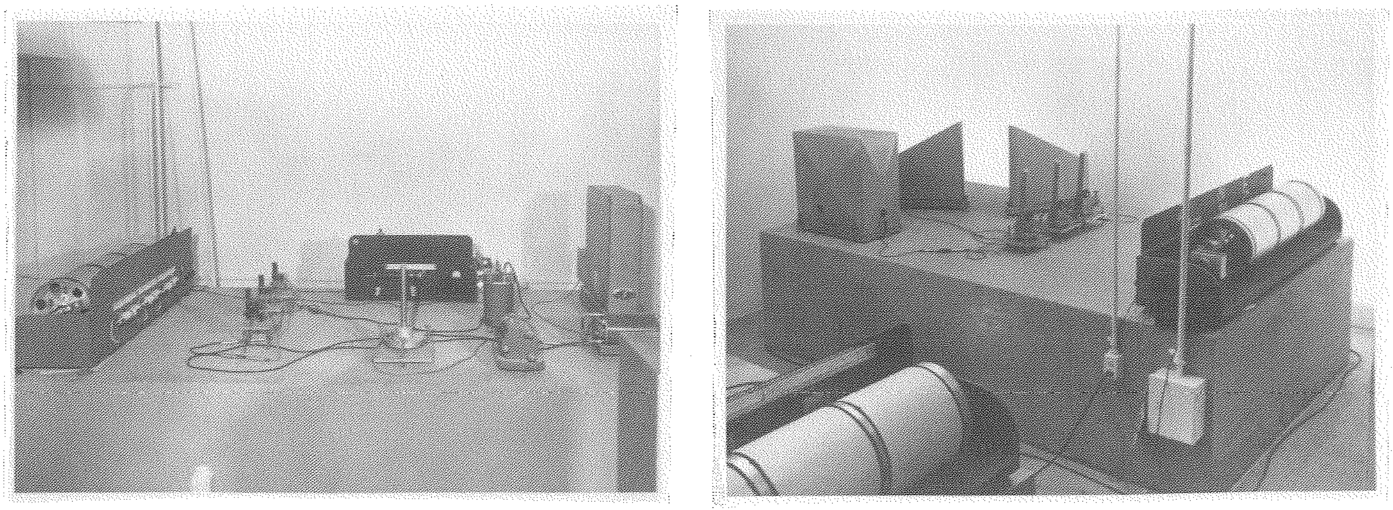
Construction of the new Ottawa vaults. The tunnel connecting the Seismology building to the main Observatory building, the staircases to the two vaults, and the vaults themselves are seen as the backfilling commences.

Successive operators at Resolute Bay had read the records and submitted their readings by radio; these were checked in Ottawa when the records arrived in late summer. The readings always proved reliable and eventually the bulletin was prepared using the radio-transmitted data. Could the same thing be done with the new network?

It could and was. Except for the second order stations, the records were read at the stations. This meant that payment had to be made to the universities and other cooperating agencies, as outlined above, and that the technicians at all stations had to be trained to read the records as well as to maintain and operate the instruments. Since the Meteorological Service insisted that all its observers must be trained in the work, it proposed that the Observatory participate in the twice-a-year courses that they ran at their training centre in Scarborough. This was agreed to, and eventually our own technicians from all the stations were also trained there, remaining just for the seismological part of the course.

The Meteorological Department had lots of experience in training, and we had little; our initial efforts were not good, but we learned. A large set of typical seismograms was reproduced in the Map Printing Section of the Surveys and Mapping Branch, a standard vault was built at Scarborough, and the students all had at least one week of hands-on experience. The graduates of the course were very competent; the rotation of observers at the stations managed by the Meteorological Service had no adverse effect on the seismograph station operation.

A "quality control" centre was set up in Ottawa to monitor the operators, and to identify and react to developing technical problems. R.J. Halliday was responsible for this monitoring, and he was ideal in the job; he had an infinite capacity for taking pains. As the records arrived he reviewed their quality and checked the recorded earthquakes against the radio messages received earlier. If there was anything questionable he wrote to the operator, pointing out the trouble, with copies of the seismogram if necessary. Usually the operators quickly ran through and corrected a number of breaking-in difficulties, after which the close checking could be relaxed. In the few cases when this didn't happen the operators were discharged.



The "standard" Ottawa vault: to the left the short-period Willmore seismographs, to the right the long-period Columbia seismographs.

A calibration pulse was placed on each record, and it was possible by examining the shape and amplitude of the pulse to detect any change in the seismometer's operating characteristics. When this was noted, the other half of the team went into action. F. Lombardo would advise the operator how to correct the fault if that was possible. If not he would visit the station and correct the fault as soon as transportation could be arranged. Initially he made a routine visit to each station every year to calibrate the instruments with Willmore's Maxwell bridge and to check over the complete operation. Later it was found that the instruments were so stable that visits every second year sufficed.

Some university geophysicists criticized us for installing standard paper recorders in the network instead of introducing an FM tape system which would allow a much more sophisticated analysis of the records. Twenty years of first-class operation has proved us right. However, the need to digitize these records for specific studies was soon recognized. Using an old Benioff recorder, A.J. Wickens and F. Kollar developed a digitizer with the variable speeds and sampling rates demanded by the standard records, both short- and long-period[8].

The preparation and publication of a bulletin for the expanding network was a tedious job. Was it necessary? We thought not and many other seismologists felt the same[9]. At the time of the 1960 Helsinki meetings of the IUGG there was general dissatisfaction with the workings of international seismology. Three agencies collected earthquake data and computed epicentres: the USC&GS produced lists of epicentres relatively quickly but with data from a limited number of stations; the Bureau Central International Séismologique (BCIS) published more extensive lists of epicentres, based on more data, with a delay of about a year; the International Seismological Summary (ISS) included data from all the world's stations but operated very much in arrears. Surely, with modern means of communication and computation, the collection of world-wide data at one central bureau, the prompt computation and publication of epicentres and of the complete data on which they were based, was possible. The matter was thoroughly discussed and action was called for.

This call was answered by UNESCO; it convened a meeting of experts in July 1961[10]. The Meeting recommended the establishment of an international agency, to be known as the International Seismological Centre (ISC) to supersede both the ISS and the BCIS. It would define world standards in such matters as microfilming and data submission and would aim to publish earthquake epicentres and data in a complete and timely manner; a publication delay of 18 months was envisaged.

The recommendations were followed. Financial support for an initial period was assured by grants from the United States National Science Foundation, the Royal Society of London and the Canadian Department of Mines and Technical Surveys. The Centre was set up initially in Edinburgh, with P.L. Willmore as Director. Its first bulletin covered the earthquakes of 1964. From this small beginning it grew, not without many vicissitudes, to an established organization, issuing monthly bulletins and catalogues. It took over from the Observatory the publication of the Bibliography of Seismology and, as we shall see later, the routine determination of earthquake mechanisms.

As the ISC developed there was less and less need for us to issue detailed national bulletins. Beginning in 1962 only P phases were listed, although each earthquake was listed separately, with the USC&GS epicentre heading the listing. Thereafter only the initial P-wave arrivals were reported in a computer-listing format, without any epicentre being given; beginning in 1972 even this was stopped. An annual network bulletin, issued in the Seismological Series, listed each station location and operator and gave the magnification curves for all the instruments, with notes on any changes that had occurred during the year.

It was recognized that the expanding network would present an impossible problem in the storage of records. A decision was made to experiment with microfilming. The first contract was let to a private firm and all available records back to 1906 were copied. It had been the practice to write the pertinent data about the seismograms on the back of the records. At the beginning this information had to be transferred to the record face; then the company developed a system to copy both sides of the record, and this was used to complete the copying to the end of the 1959 records. Once the problem was recognized, routine operations were changed so that all data appeared on the front of the record.

A report on this program was made to the UNESCO working group, convened in 1961 to consider the handling of seismological data on an international basis. Soviet seismologists also presented examples of their efforts in this direction and representatives of the USC&GS, speaking on behalf of the Vela Uniform project, discussed the plans for the microfilming of all WWSSN records. They were thinking in terms of 70-mm chips, a separate chip for each record, whereas the Soviet and Canadian experiments were based on reels of 35-mm film. What about international standards? The Vela Uniform group agreed that the WWSSN records would be copied on 35-mm reels as well as on 70-mm chips.

The Canadian program continued, except that it was transferred from a private contractor to the Central Microfilm Unit of the Public Archives of Canada. The Archives dedicated a "roll-through" copier for the purpose and Halliday personally supervised the work. The quality was high, but not as high as that of the 70-mm chips of the WWSSN system. Since Canada was filling a gap in the world's area which otherwise would have had to be covered by the WWSSN, and since the records would be necessary to any researcher wanting a world set of records, it seemed reasonable that the WWSSN centre should microfilm Canadian records, possibly with a subsidy from the Canadian government. Despite the logic of the position it was mid-1966 before the Americans would accept it. A year and a half later, at the end of 1967, budget constraints led them to cancel the agreement, and microfilming reverted to the Public Archives.

When the microfilming project began it had been intended to destroy the original seismograms, and the Public Records Committee had agreed to this. When Professor Maurice Ewing and his colleagues at the Lamont Geological Laboratory heard of this, they asked that the records be given to them. They had no old records to refer to, and promised to care for our records and make them available upon request.

This was most fortunate. Renewed interest worldwide in earlier instrumentally-recorded earthquakes has led researchers back to the original seismograms to read details not clearly reproduced on the microfilm copies. To have destroyed the originals would have been an irreparable loss.

In 1985 the records were returned to Ottawa for permanent storage at the Public Archives of Canada and a computerized inventory was prepared.

Canadian Seismicity

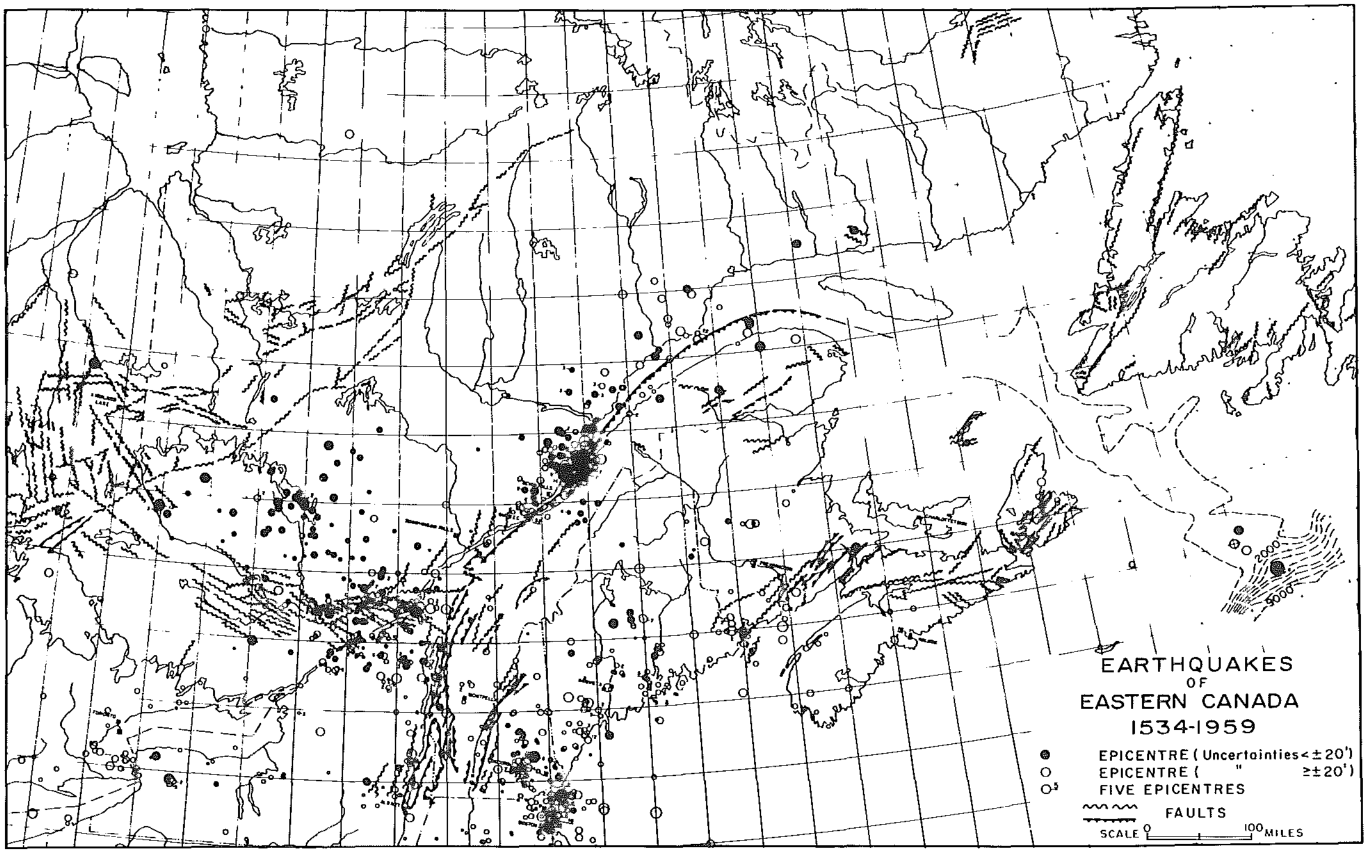
In 1953 the Division of Building Research, NRC, published a revised edition of the National Building Code. We were asked to provide a map defining zones of earthquake risk. A map was provided, as will be described later. The effort to construct it brought home the fact that we had no systematic listing of Canadian earthquakes. We began the long process of compiling such lists.

From the time of his arrival in Victoria, Milne had produced annual publications[11] listing all earthquakes felt or located by his small triangle of stations. By the time the earthquake lists were prepared for the 1955-1959 shocks, explosion-based crustal studies had provided travel-times appropriate to the area and the quality of the records had improved through revised instrumentation. Locations were based on measurements of the S-P interval, not only from the Canadian records, but from those of an increasing number of United States stations.

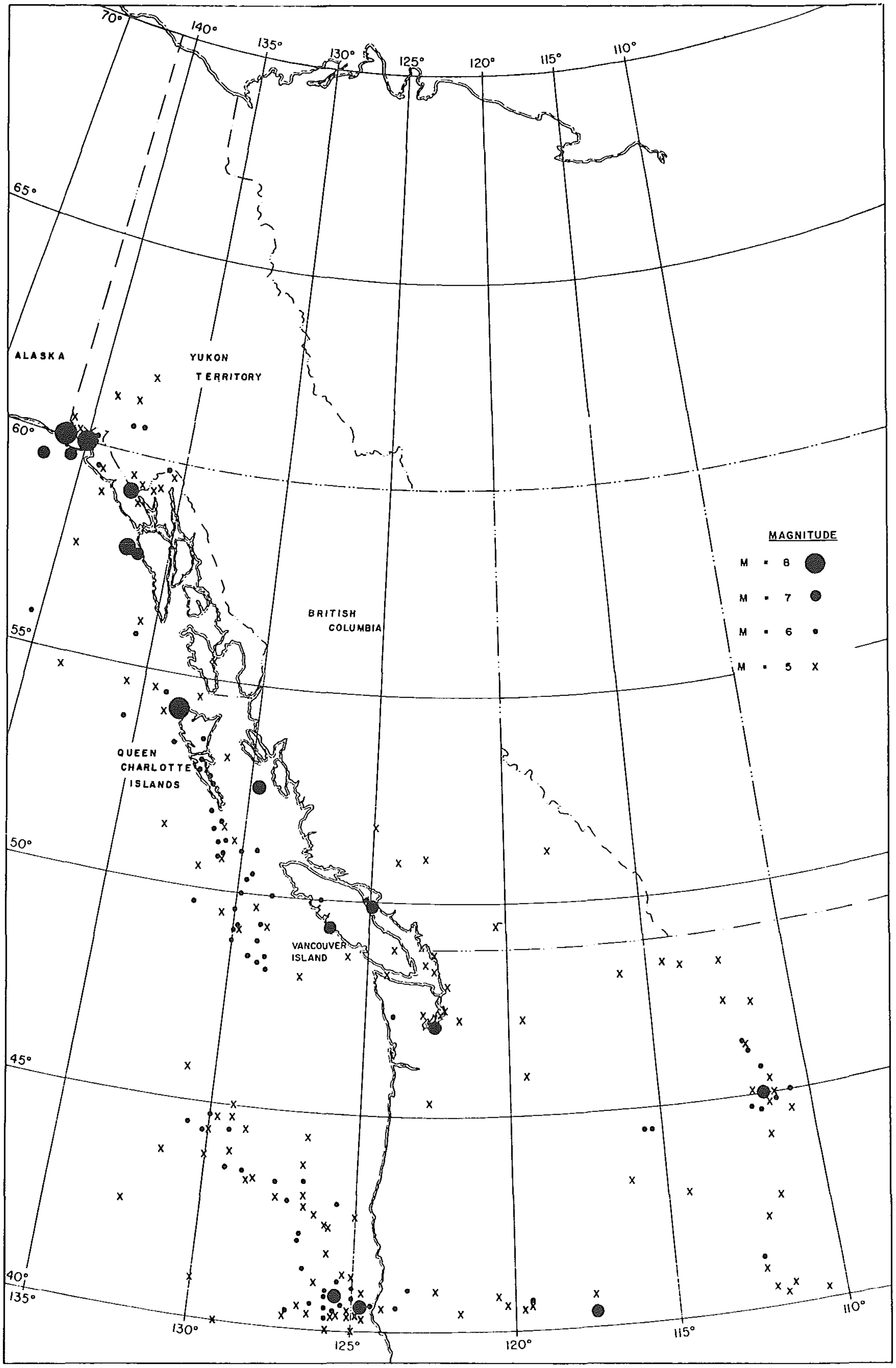
In addition to recording the current earthquakes, Milne studied the seismic history of British Columbia. Using various catalogues of Pacific earthquakes and the index of the International Seismological Summary, he compiled a list of earthquakes located offshore or on the islands and mainland. He then searched the newspapers in the Provincial Archives to find if the earthquakes were felt and over how large an area. He ended with a list of 242 earthquakes[12]. Most of these were offshore, but there were several in the Straits of Georgia and of Juan de Fuca, and one, with magnitude of about 7.5, inland from Vancouver, probably in west-central Washington. This earthquake occurred in 1872 at which time there was only scattered settlement in the Province.

By 1960 the seismic history of western Canada was as complete as it was likely to become, and up to date. The annual listing of western earthquakes gave way to lists for all of Canada, issued in Ottawa.

Milne's efforts to prepare a catalogue of western Canadian earthquakes was paralleled in Ottawa by W.E.T. Smith. Here the work was much more difficult. All the available seismograms, dating back to 1906, had to be re-examined. Parts of eastern Canada had been settled for nearly 300 years, making for a lengthy search of historical documents, old newspapers, and the like. Most of the material for this search was already in the files, collected principally by E.A. Hodgson in the form of a bibliography and excerpts of pertinent passages. Smith added references to some later publications but did not re-examine the original documents. He did however re-read



Earthquakes of Eastern Canada and adjacent parts of the United States, 1534-1959, as located by W.E.T. Smith.



Earthquakes of Western Canada and adjacent parts of the United States, 1841-1965, as located by W.G. Milne. From Canadian Journal of Earth Sciences, 4, 802, 1967.

the pertinent seismograms. The results were published in two papers. The first[13], cataloguing the earthquakes up to 1927, at which time the first short-period seismographs were installed in Canada, depended almost exclusively on historical data; the second[14], covering the period up to 1959, was based more completely on instrumental data.

In 1960 the effort to document the seismicity of Canada was placed on a more formal basis. The country was divided into four zones: (1) the Western region, lying west of the 113th meridian and south of the 60th parallel; (2) the Eastern region lying east of the 85th meridian and south of the 60th parallel; (3) the Arctic, covering all of Canada north of the 60th parallel; (4) the Central region, lying between the 85th and 113th meridians and south of the 60th parallel. The seismic histories of each region would be published as soon as possible, and the listings would be kept up-to-date by annual publications.

The history for the Western region had already been published[11,12]. Because Smith's catalogues for the Eastern region were not expected for some time, a preliminary list covering 1954-1959 was issued[15]. Studies based on a search of standard catalogues and the limited data from the Resolute station brought the history of the Arctic region up to 1959[16]. No listing for the Central region was attempted; the known earthquakes were too few to justify it.

The listing of earthquakes having been brought up to the end of 1959, joint papers, covering all regions of Canada for each calendar year were published regularly in the Seismological Series[17].

The expanding seismograph network led to the location of many more earthquakes than had previously been possible, particularly in the Arctic. One interesting result of this was the recording, during a thirty-day period in 1965, of a swarm of small earthquakes, more than two thousand in number, close to the Mould Bay seismograph station. A swarm is a series of earthquakes without an identifiable principal earthquake; it is not a foreshock – main shock – aftershock sequence.

It will be recalled that magnetic studies had suggested abnormally high upper mantle temperatures[18] in the area. Could the swarm be related to this? By good fortune a small seismic array was being operated by the United States Air Force just to the north of the Canadian station. By pooling data from the two sources it was possible to locate about 10 percent of the shocks in three dimensions. They were found to define a volume of about 8 km^3, with its centre at a depth of about 7 km, near to the strike of a known fault. Magnitudes as low as 1.1 were identified, and by studying the distribution of the numbers in different magnitude ranges and the nature of first motions it was inferred that the source of the energy was tectonic[19], although the possibility of magma movement could not be ruled out.

An earthquake swarm occurred in the highly seismic area west of Vancouver Island in a six-day period late in August 1967. By this time the network in Western Canada had been completed and five stations were within a "regional" distance of the swarm. R.J. Wetmiller studied the events as part of a thesis for a Master's Degree from the University of British Columbia[20]. By treating the five stations as a "spread" he deduced the travel-time curves for P and S waves in the mantle and was able to estimate a minimum crustal thickness of 40 km under Vancouver Island.

The experience with earthquake swarms suggested that a study of small, "micro" earthquakes, might cast light on incipient energy release in potential earthquake areas. Microearthquake research was conducted in two areas in Canada.

The Geodetic Survey had rerun lines of precise levels between Quebec City and Lac Saint-Jean and found remarkable changes, indicating a vertical uplift at a rate of some 0.4 m/century[21]. It extended its lines of investigation so that the uplift could be contoured, and the bulge fell in the Laurentide Park, somewhat to the west of the epicentre of the 1925 earthquake. Could the two phenomena be related? Smith installed three sensitive seismographs in the area in the summer of 1968. They were operated only at night when lowered noise level permitted the highest magnification. The results were not conclusive, a few dozen microearthquakes were recorded, but there was no clear correlation with the uplift. However, the feasibility of the method was established[22].

During this experiment Smith and W.T. Piché, with the collaboration of F. Kollar, refined the instrumentation used at regional stations[19,23,24]. The resulting portable Helicorder field system provided high-magnification, high-resolution, short-period records written directly on paper by hot stylus. The seismologist could judge immediately whether a site was likely to prove suitable. The band-pass was adjustable by plug-in filters to further enhance the signal-to-noise ratio. The entire system was powered by 12-volt automobile batteries, permitting continuous operation for 24 hours, after which the batteries were replaced by recharged ones. Since components of the system could be carried by a single individual, the seismograph could be installed at sites as remote from access roads as the individual was prepared to walk. While portable short-period seismographs were further refined in the 1970s, this 1968 experiment marked the beginning of Canadian field surveys that permitted high-quality monitoring of local seismicity and aftershock sequences.

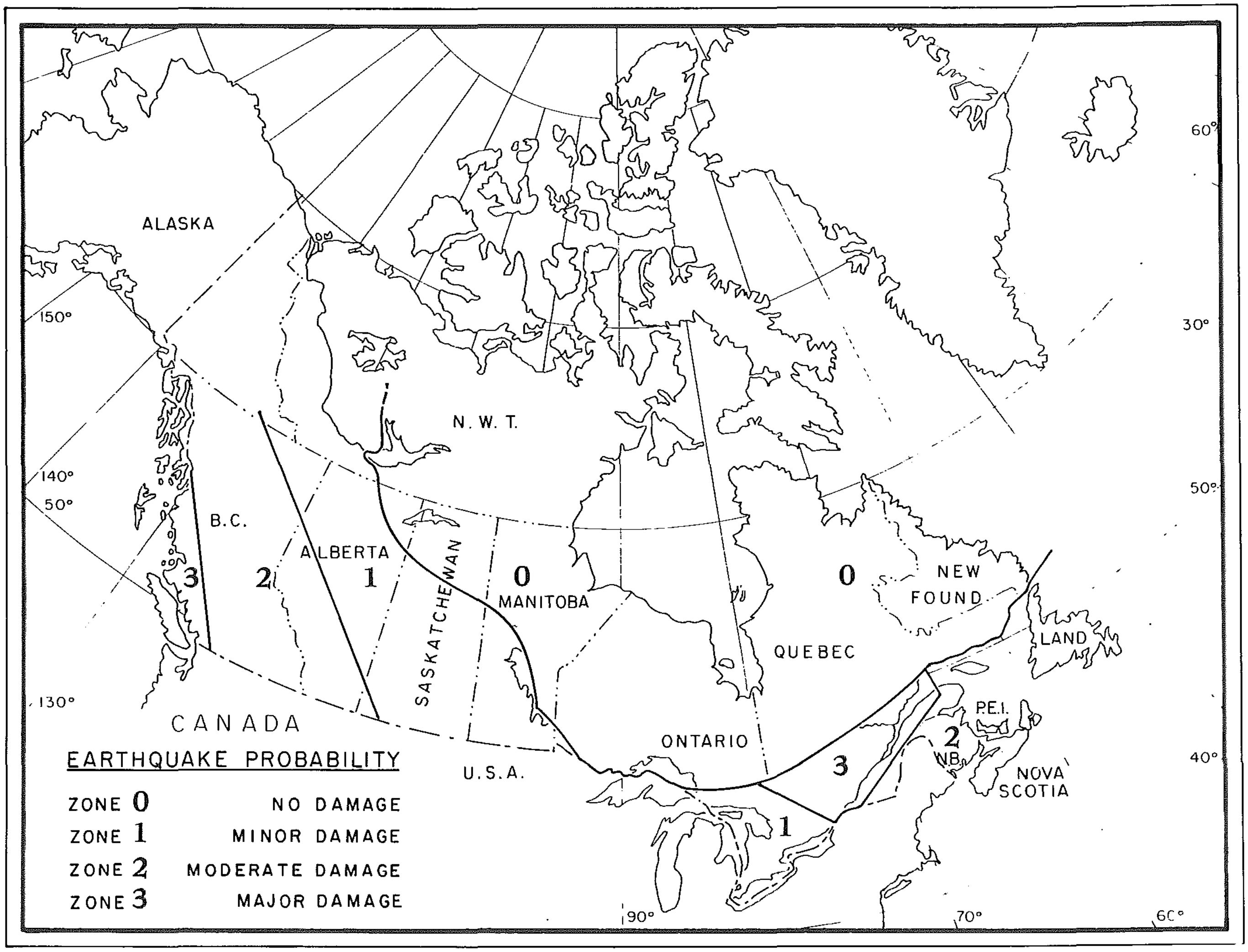
Later in the summer of 1968 G.C. Rogers took the equipment to north-central British Columbia, to the vicinity of Mt. Edziza, a large dormant volcano. J.G. Souther of the Geological Survey had suggested that there might be magma movement beneath the mountain. The recorders were operated for three months at various locations in the vicinity. No unusual activity was found beneath the volcanoes, but a large number of more distant, low-frequency, events were recorded from about 150 km to the west, near the Alaska-British Columbia border.

Microearthquake studies were renewed in northwestern British Columbia during the summers of 1969 and 1970. Rogers analyzed the data for an MSc thesis at the University of Hawaii. The unusual low-frequency events turned out to be generated by large glaciers flowing to the sea in southeast Alaska. None of the geologically recent volcanoes in the region exhibited any seismic activity[23].

In 1970 interest returned to eastern Canada and the area of major seismicity in the St. Lawrence river valley, south- west of the mouth of the Saguenay river. It was a joint effort between the Ottawa group and the Department of Geology of Laval University. By combining equipment they were able to field seven portable stations. These were located not at the bulge in the Laurentide Park shown by precise levelling, but in the area to the northeast where the known earthquake epicentres were located. Analysis was largely limited to events recorded during the night, in order to eliminate possible blasts[24].

Approximately one microearthquake per day was recorded over a period of 53 days; of these, 28 were located in three dimensions and they were found to lie within the "Charlevoix structure", an impact crater described in an earlier chapter. The clustering of both the large historical earthquakes and the current microearthquakes within this circular area suggested a correlation. The implication was not that the earthquakes were caused by crustal readjustments to the enormous energy of the impact, but that they were concentrated in this localized area because the crust had been weakened by the impact. Later studies, after 1970, showed that some microearthquake activity extended beyond the crater boundaries. The impact may have been a contributing factor, but not the entire explanation of this earthquake zone.

The increased awareness of earthquake hazard in Canada led to a realization that many branches of the Department could have an input to the problem. A Departmental committee was set up in 1962, with representatives from the Geological, Hydrographic and Geodetic Surveys, the Gravity and Seismology Divisions of the Observatory, and with observers from the Division of Building Research, NRC. The committee met at infrequent intervals under my chairmanship; W.E.T. Smith acted as secretary. In 1965 the scope of the committee was enlarged to include the general field of recent crustal movements. In 1969 it sponsored a "Symposium on Recent Crustal Movements", arranged by a sub-committee chaired by J.G. Fyles of the Geological Survey. The papers presented were published in a dedicated volume of the Canadian Journal of Earth Sciences[25]. A preface to the volume contains an interesting comment:



Earthquake Probability Map for Canada. Early 1950s.

> "Since the Committee was set up, the seismic history of Canada has been completed and much evidence has been collected about recent crustal movements in Canada. It must be admitted that, at present, the latter throws very little light on the former."

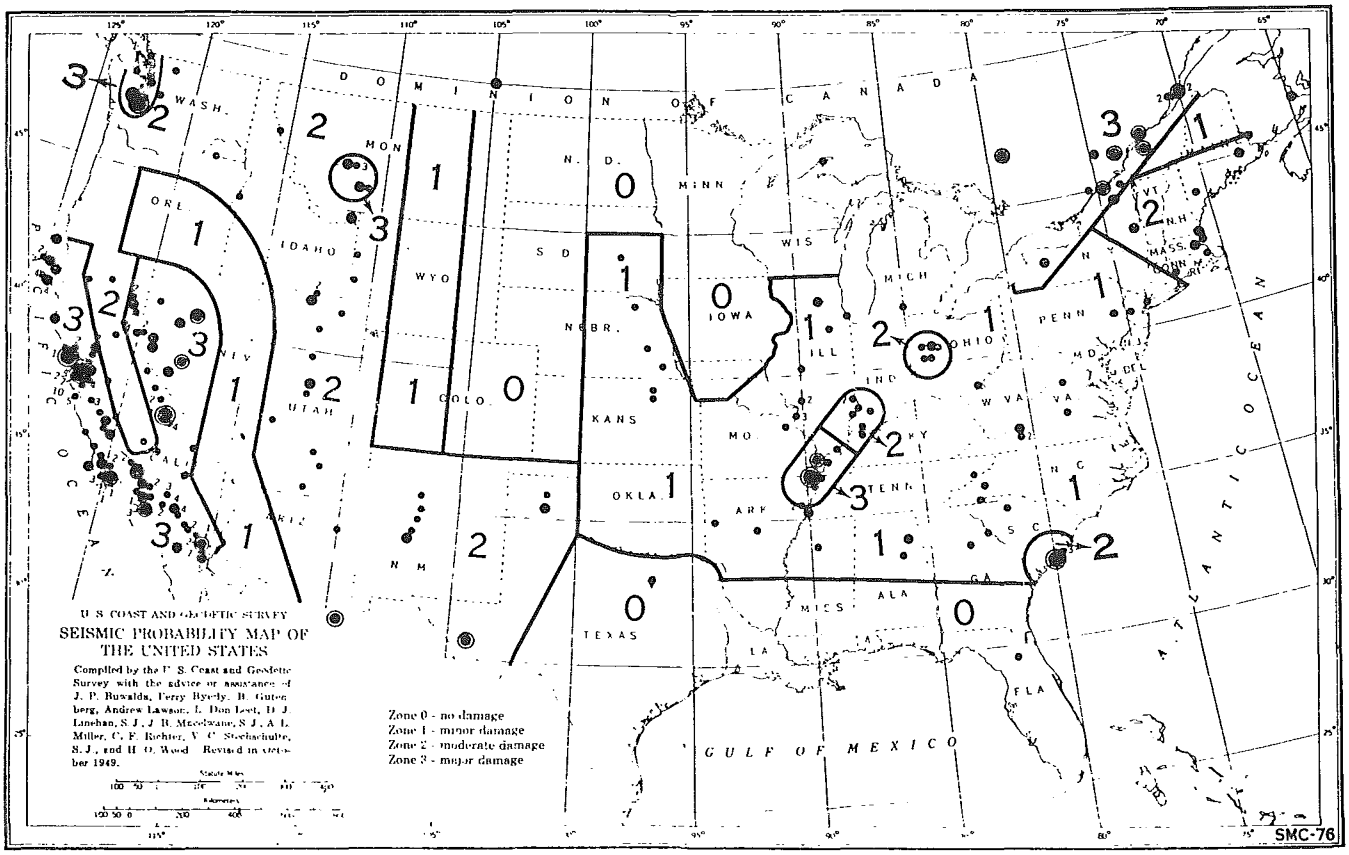
The increased knowledge about Canadian seismicity also meant that Observatory personnel were called upon to take part in symposia. The Royal Society of Canada held a symposium on Appalachian Tectonics to which Smith contributed a paper on the implication of eastern Canadian earthquakes[26]. As another example, the Division of Building Research, NRC, and McGill University sponsored a two-day symposium in September 1966 on "Design for Earthquake Loading". Observatory staff provided two papers giving background information on seismology[27] and on Canadian seismicity[28].

Seismic Regionalization and Seismic Zoning Maps

The continuing efforts of Observatory seismologists, particularly of E.A. Hodgson, to stress the existence of severe seismic hazards in Canada finally bore fruit. In 1953 the Division of Building Research, NRC, was publishing a revised edition of the National Building Code and decided to provide specifications for earthquake-resistant construction suitable for Canadian conditions. They asked the Seismological Division to provide a map defining zones of earthquake hazard. Our contribution is shown in the figure on the facing page.

An equivalent zoning map already existed in the United States; the Canadian map was patterned on it and made to match as much as possible at the international border. Both maps defined zones of four degrees of possible damage, 0 to 3, ranging from "no damage" to "major damage". The zones of major damage consisted of the St. Lawrence and Ottawa valleys and the Pacific coast, areas in which major earthquakes were known to have occurred. Zone 0 was assigned to the Canadian Shield, which was believed to be geologically stable, except that the line was drawn north of the Shield boundary in Quebec, in recognition of the fact that a serious earthquake had occurred at Temiskaming, in the upper Ottawa River valley, in 1935. The Maritimes were placed in Zone 2 since a magnitude 7.2 earthquake had occurred on the Grand Banks in 1929 and since there were frequent earthquakes throughout New Brunswick and Maine. There was no attempt to extend the boundaries into the Arctic; except for a cluster of earthquakes in Baffin Bay, nothing was then known about its seismicity.

The map was published as part of a "Climatological Atlas" but no opportunity was provided for a discussion of its limitations. These should have been obvious; the fact that the



Earthquake Probability Map for the United States. Early 1950s.

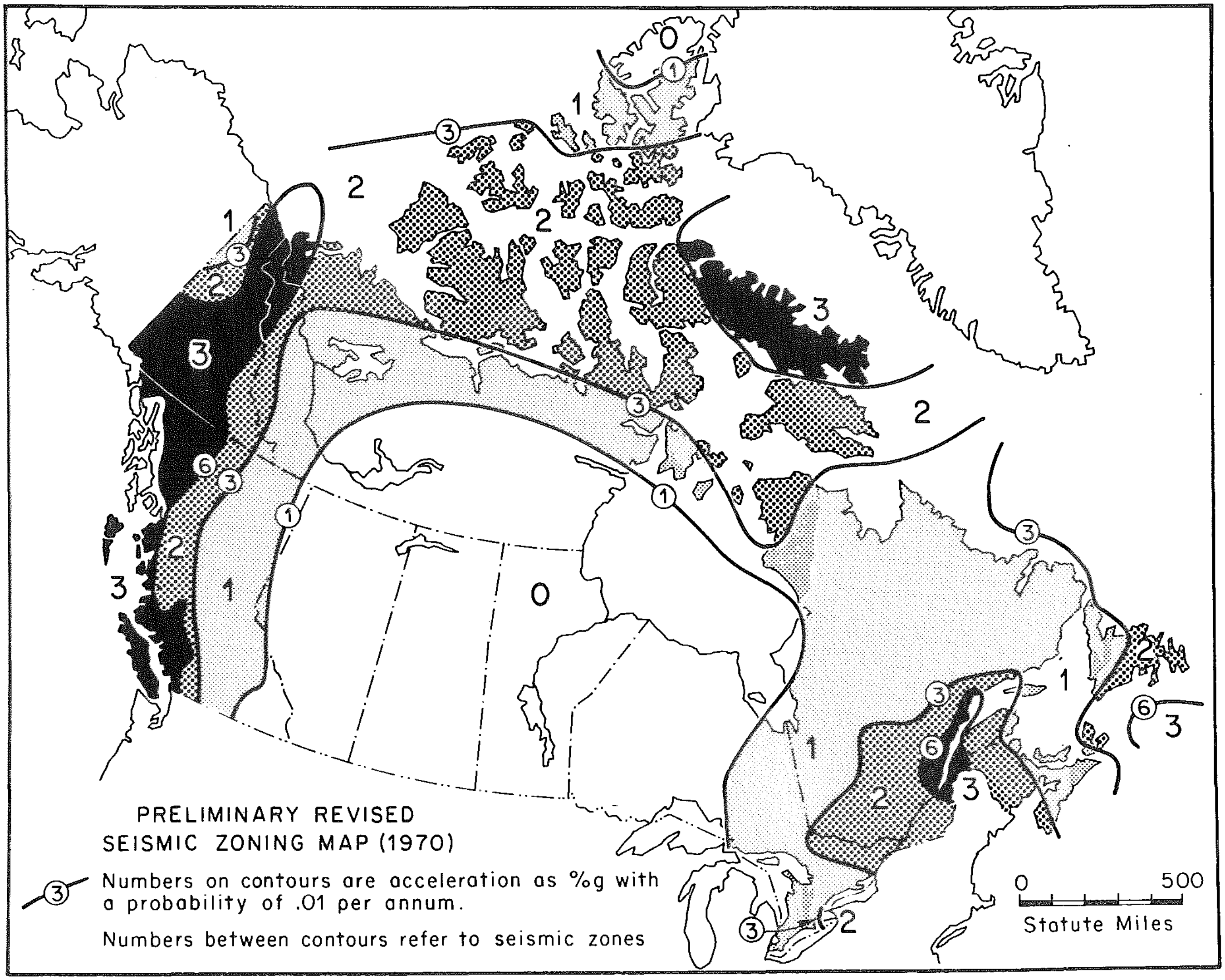
boundaries were drawn as straight lines and simple curves should have warned the users that they were imprecise. A paper was published in an insurance underwriting journal[29] setting out the limitations, but engineers were unhappy about the map's vagueness. What were they to do at a triple point where, depending on which way one stepped, one could be in Zone 3, or 2, or 0?

The program for updating our knowledge of Canadian seismicity, as described in the foregoing section, was instituted immediately and by the late 1960s all available data had been collected. The time had come to prepare a much more detailed and sophisticated seismic regionalization map.

As a beginning Milne produced an interesting paper[30] in which the seismicity of western Canada was illustrated in a variety of different ways: by plotting the epicentres without regard to magnitude; by plotting epicentres with symbols indicating magnitude; by computing the strain energy release per square degree and contouring the result. He studied the frequency distribution by year and the relationship of magnitudes and numbers of earthquakes. An extension of the paper, applying the methods to all of Canada was published a few years later[31]. The work was not, strictly speaking, regionalization, although it did define the zones of hazard quite clearly.

The larger problem of seismic regionalization was undertaken by Milne for his doctoral thesis at the University of Western Ontario, where he worked with Professor A.G. Davenport. They developed an approach based on extreme-value statistical methods applied to the catalogues produced during the previous decade, but limited to the years 1899 to 1963. This was done in order to have a uniform time period across Canada, because the western data were considered adequate only from 1899, and because Smith's catalogue for eastern Canada became less reliable the more remote the period.

Milne and Davenport[32] produced equations for determining at any point the peak horizontal ground acceleration, stated as a percentage of gravity, g, that had a 1% chance of being exceeded in any year; to put it more positively, it had a



Seismic Zoning Map for Canada, 1970. From Canadian Underwriter, 15, 6, 1970.

99% chance of not being exceeded in any given year. By computing these horizontal accelerations at a grid of positions it was possible to produce a seismic zoning map, where the zones 0, 1, 2, and 3 corresponded to peak acceleration ranges of < 1, 1-3, 3-6 and > 6% g respectively. The map[33], shown in the figure on this page, was published in the 1970 edition of the National Building Code of Canada (NBC). A set of commentaries to the NBC permitted a discussion of the derivation of the zoning map and of its limitations.

The use of horizontal ground accelerations provided the basic information design engineers wanted, since it is horizontal forces against which they must protect their structures. Subsequent engineering advances have added other equally important design parameters.

The new map included the Arctic for the first time, and portions of all four zones were found there. In eastern Canada the area of the high-hazard Zone 3 was reduced to a small section of the lower St. Lawrence, thereby removing Montreal and Ottawa to a Zone 2. In the Atlantic provinces, as in British Columbia, portions of Zones 1, 2 and 3 were found. No attempt was made to match zones across the International Boundary.

The method by which the map was computed allowed for the calculation of the value for any point, including those in Zone 3 where the map gave only the lower limit of 6% g. A service was set up whereby engineer and others could be given a calculation for any point for a nominal fee.

The new map, like the earlier one, was published by the Division of Building Research, NRC, as part of the National Building Code. The liaison between the Observatory and the NRC was through an Associate Committee which had been established to advise the Council on all aspects of earthquake engineering, zoning maps, codes, etc. With the establishment of an International Association of Earthquake Engineering in 1964, this Committee also served as the Canadian National Committee of the Association.

The map was a major contribution to Canadian engineering and lasted for nearly 15 years. It was finally superseded in 1985 by still more detailed maps based on new techniques and profiting from another fifteen years of earthquake data.

Strong-Motion Seismology

While a knowledge of the probable maximum ground acceleration to be expected in an earthquake zone is useful, a knowledge of soil and building response to actual earthquakes is also necessary. This requires a "strong-motion" seismograph, an instrument of very low gain but high recording speed, whose record stays on scale even in a violent earthquake and in which each movement of the ground can be resolved. If such instruments were to run continuously at the required speed, they would spew out mountains of paper as they waited for an earthquake. Some sort of triggering mechanism is necessary to start the recorder.

In 1961 it was decided to begin the installation of strong-motion seismographs in Canada, but such instruments were not commercially available. The USC&GS had developed an instrument and had deployed many of them throughout seismic areas in the western United States, but the instruments were no longer being manufactured. The solution was to borrow one and have it copied. The contract for this was let to the Fairey Aviation Company of Victoria.

The seismograph measured ground acceleration in three mutually perpendicular directions, and two horizontal components of displacement. It was set in motion by a trigger mechanism, the sensitivity of which could be adjusted, and it then recorded on film at a rate of 2 cm/s. It operated from a 12-volt storage battery, trickle-charged from a continuous a.c. source.

The installation of the strong-motion network was a joint effort of the Dominion Observatory and the Division of Building Research, NRC. Beginning in 1963 Milne deployed eight instruments in coastal British Columbia, the NRC a similar number in the St. Lawrence valley. Most were installed in the basements of buildings but a pair of instruments, one in the basement and one in the 23rd floor penthouse, was installed in the B.C. Hydro building in Vancouver.

G.C. Rogers joined the staff in 1967, and was placed in charge of the strong motion program. By that time a commercial accelerograph was available, manufactured by the United Electro Dynamics Corporation. While it did not measure displacement, but only three components of acceleration, it was much easier to maintain, and the program switched to this instrument. By 1970 six had been installed, making a west coast strong-motion network of 14 stations.

The accelerographs were expensive to purchase and to install. For this reason they were augmented by large numbers of "seismoscopes". In these, a pendulum of carefully selected period scribes a line on a smoked watch glass. The gyrations of this line give a measure of the amplitude and direction of the motion. The instruments were inexpensive and easy to maintain; by 1970, 48 had been installed in the west.

In a paper detailing the state of the network in 1970[34] plans were outlined to expand the networks of both accelerographs and seismoscopes, and to interest engineering firms and builders to help with the funding of this program. This expansion did take place, although not quite as forecast. In the early 1970s the cost of accelerographs dropped dramatically. The reduced cost made the seismoscopes not good value for the information received, and their use was discontinued.

The network is still awaiting a major earthquake. Low levels of accelerations had been recorded only from three western U.S. earthquakes up until 1970[35]. This is the problem with strong-motion seismology; one must be prepared to wait for a long time, maintaining the instruments always in a state of readiness. In the 1980s a number of important records were obtained from the permanent strong-motion network in eastern Canada and from instruments deployed temporarily in aftershock zones in New Brunswick and the Northwest Territories.

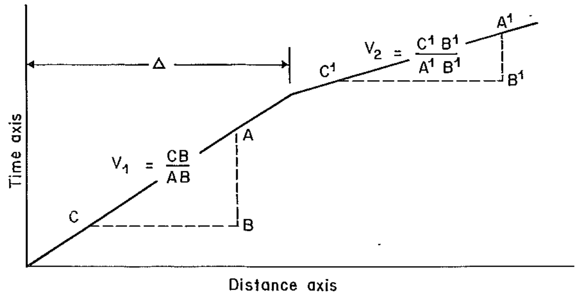
STUDIES OF THE CRUST AND UPPER MANTLE

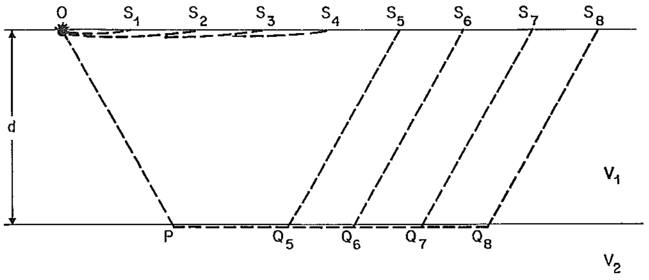
The fact that the continents are covered by a crust was discovered by Mohorovicic, in Europe, in the early days of seismology; later Conrad found evidence, again in Europe, for a second layer. The "classical" crust which they defined consisted of two layers, a "granitic" layer above and a "basaltic" layer below. Beneath them the mantle extended, without major discontinuities, to the earth's core, at a depth of some 2900 km. Subsequent work has shown that all continents have a crust, and that it may be layered. The terms "granitic" and "basaltic", gross over-simplifications, are no longer used, but the interface between the crust and mantle is still called the Mohorovicic discontinuity, or "the M", for short.

The continental crust has a thickness measured in tens of kilometres, with a median value of about 35 km. The oceans also have a crust, only a few kilometres thick. We saw in the previous chapter that the oceanic crust has derived from the mantle by the upwelling of basaltic magma along mid-ocean ridges, and that this process has led to the drifting of the continents. The study of the crust, oceanic and continental, and of the mantle, is important to the understanding of the geologic processes that have shaped the earth. Seismology provides several of the important tools for this study. Two of the approaches are by refraction seismology and through the analysis of surface waves. Reflection seismology, so important in the search for petroleum, was not applied to crustal studies until some ten years after our period of interest.

Refraction Studies

The methods of refraction seismology are easily understood. As an example, suppose that a single uniform layer in which the velocity of seismic P waves is V_1, overlies an infinite substratum in which the velocity, V_2, is greater than V_1. We propose to measure its thickness, d. We set out a "spread" of seismographs S_1, S_2 --- S_n, equally spaced, as shown in the figure below, and fire an explosion at the point O. How will the first energy reach successive seismographs? Clearly at the nearest points, at S_1 and S_2, and perhaps at S_3 and S_4, it will travel the direct path. The slope of the line relating distance to time, shown in the upper part of the figure, is $1/V_1$.





Now consider a distant station, say S_7. The wave critically refracted at the bottom of the V_1 layer will save enough time travelling the long horizontal distance at the velocity V_2 to make up for the time it loses in travelling down to and back from the lower layer. Since the down and back time is the same for each station, the arrival-time differences at the stations depend only on the distance travelled; again the slope of the travel-time curve, $1/V_2$, gives the value of V_2.

Where do the direct and refracted waves arrive at the same time? At the point where the two lines cross on the time-distance graph. The crossover distance, Δ in the figure, obviously depends on the thickness d of the upper layer, and the relative values of the velocities. The relation is simple:

$$d = \frac{\Delta}{2}\sqrt{\frac{V_2 - V_1}{V_2 + V_1}}$$

All the unknowns in this equation are given by the travel-time curves; we simply plot travel times, measure velocities and crossover point, and compute depth.

In an earlier chapter we discussed the use of refraction seismology in the study of meteorite craters. The method was of limited use in determining the structure at depth because of the size of the craters. We can now understand this; the crater diameters were less than the crossover distance and the refracted wave could not be observed.

If there is a third layer, with still higher velocity, the travel-time curve will have three segments; again all the factors needed to determine the thickness of the layers will be given by the travel-time curves.

The above account is considerably over-simplified. If an interface is dipping, the down and back times are different for each station, and the slope of the travel-time curve does not give a true velocity. This can be overcome by firing an explosion at each end of the spread; from the two apparent velocities the true velocity can be determined. There are other complications. If the second layer has a lower velocity than the one above, the refracted ray will be bent toward the normal rather than toward the interface; no refracted wave can be recorded. What is less obvious is that the wave through a second layer need not appear as a first arrival, even though it has a higher velocity. If the layer is too thin, or if the velocity is not sufficiently different from the one above, a wave will be transmitted through the layer, but it will never be recorded as a first arrival. Since secondary arrivals are not easy to identify, this presents a serious problem, which was not effectively met until shortly before the close of our period of interest.

Physicists reading this section will object that the critically refracted wave carries no energy. The wave observed is actually a head wave, but the geometry of the problem is met by our simpler assumptions.

To determine crustal structure by refraction seismology, one sets out an array of seismographs covering a distance of several hundred kilometres and fires shots off each end of the spread to detect dipping layers; this is called a "reversed profile". The project may be accomplished by having many seismographs and one explosion at each end of the array; it is usually done with a limited number of instruments, moved along the spread, with repeated explosions at each end. It may also be done with two widely-separated spreads with a sequence of shots along the line between them. If the separation between the shots and the stations becomes large the refracted ray will penetrate the upper mantle, the longer the distance the deeper the penetration. The upper mantle may be studied in this way.

The work to be described covers more than twenty years, a period that includes many changes of personnel and problems of administration. In order that the science and the administration may be followed in chronological order without confusion, the field programs will be signalled by an identifying title, and separated from the administrative details by a series of dots, thus:

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The Rockburst Travel-Time Project

The beginnings of this project were introduced in Part I. Late in 1938 a rockburst in Lake Shore Mines, Kirkland Lake, Ontario, recorded on the Benioff seismographs at Ottawa and at Weston, Massachusetts. Recognizing the potential of rockbursts as an energy source for crustal studies, E.A. Hodgson arranged to have a seismograph set up in the mine to time any future bursts at their source. Thirteen bursts recorded at Ottawa were timed in this way; two of these recorded at Shawinigan Falls, Quebec, one at Weston, Massachusetts. I analyzed the records, as a Master's thesis at the University of Toronto, and proposed that a project should be set up to utilize bursts in a more systematic study. A pair of stations could occupy a succession of points along a line from Kirkland Lake, bearing south-southeast in the direction of Ottawa; the recordings already obtained at Ottawa and Weston would extend the line far beyond the crossover point.

The use of rockbursts as an energy source presented many disadvantages, as we shall see, but it had one big advantage that now appears rather foolish, but which was important at the time. Timed explosions had been used in crustal studies in a limited way in New England and had produced velocity values substantially higher than the accepted values suggested by earthquake studies in California and Europe. The pundits would not accept these results; there was something wrong with blasts as an energy source! Rockbursts wouldn't face this problem. They are really small earthquakes. During mining operations voids are created in the ore body and in the country rock and, no matter how carefully they are backfilled, the pressure on the remaining rock builds up. Weak rock simply crumbles under this pressure, but more competent rock, such as that at Kirkland Lake, accept the pressure, which builds up until a breaking point is reached, releasing the energy in a sharp violent burst.

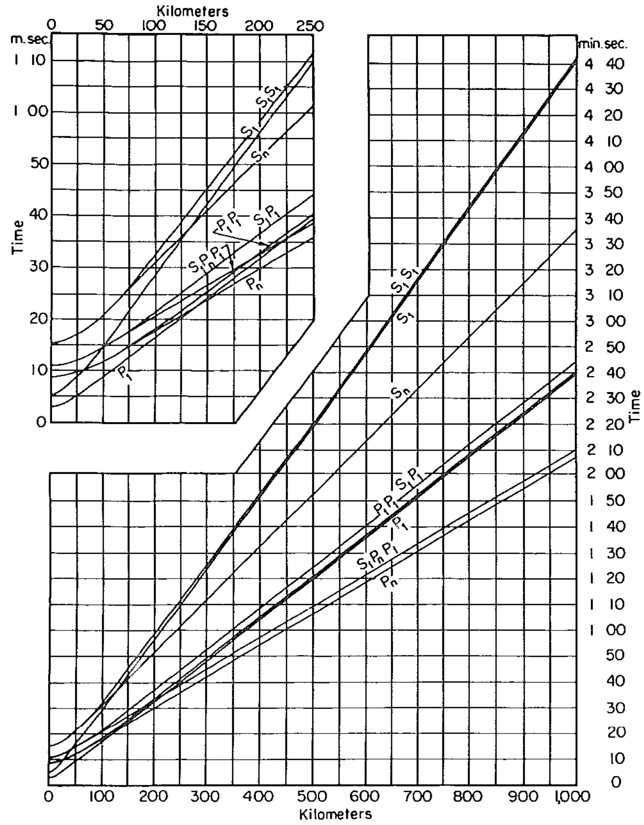
The main problem in using rockbursts lay in their unpredictability. Recording would have to be continuous, and might have to be maintained for a long time. Regular seismograph stations would have to be used, one at Kirkland Lake to time the bursts at their source, another pair to be moved along the traverse as adequate records were obtained.

Our dilemma in selecting instruments has already been described. The Benioff seismometer was by far the better instrument for our purposes but it was very heavy, too heavy for a field project such as that proposed. We chose the Sprengnether seismometers. These quickly proved inadequate to detect the

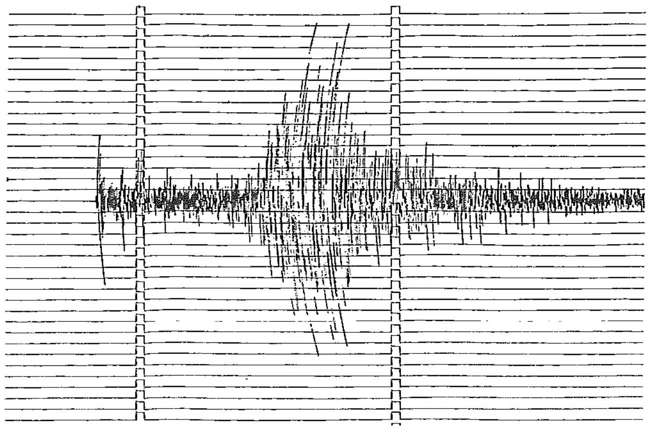
One of the portable seismic huts of the rockburst travel-time project. The hut is set up at La Cave, Quebec, where mains power was available. Normally operation was powered by storage batteries.

The interior of a recording hut. The operators, from left, W.G. Milne and Graham Alvey.

short-period energy of a rockburst, and the project was eventually completed with modified Willmore seismometers. We were more successful with recorders. Sprengnether supplied recorders with variable drum speed – 60, 112, 150 and 281 mm/min. These were ideal for our purpose, and three were purchased, a single component recorder for the base station, three-component recorders for the moving stations.



Travel-time curves for earthquakes in Eastern Canada. They are based on the velocities and crustal dimensions found in the rockburst work and include travel times for multiply reflected waves. It was hoped to account for the many phases found in the record of a near earthquake, as shown in the figure below, and to provide a means of determining the earthquake's depth.



A typical near-earthquake record, the Ottawa record of an earthquake near La Tuque, Quebec.

Dr. P.L. Willmore in his laboratory.

To house these instruments a small concrete observing station was built on bedrock in Kirkland Lake, and the Observatory carpenter built insulated wooden huts, 8′x8′x6′ to house the field stations, with a small vestibule to act as a light-trap. These huts could be dismantled into seven easily-transportable sections. A small concrete foundation to support the huts, and piers to support the recorder and galvanometers, had to be prepared at each site. A large stake-bodied truck was provided to carry buildings and recorders.

By modern standards the equipment was very heavy but we became adept in its use. The piers and foundations were built, as time permitted, before they were needed. When the time came to move, a station could be taken out of operation after breakfast, moved as much as 50 km, and be back in operation at the new site by supper time.

Stations close to Kirkland Lake were operated during the summer field seasons at a high paper speed, the records being changed three times a day. This program could not be maintained during the winter months; then the instruments were moved to the more distant locations and operated at 60 mm/min by local operators. The project required four years, from July 1947 to May 1951, and provided the material for my PhD thesis at the University of Toronto. I was assisted by W.G. Milne and by a succession of student assistants two of whom, F. Lombardo and P.C. Bremner, joined the Division upon graduation.

No clear evidence was found for an intermediate layer in the crust[36]. The travel-time curves broke from a crustal velocity of 6.25 km/s to a mantle velocity of 8.18 km/s. Equivalent S velocities were 3.54 and 4.85 km/s. The mean crustal thickness was 36 km. This was the first determination of crustal thickness and velocities in Canada.

During the last field season blasts were recorded, originating in the open-pit mines of the International Nickel Company at Sudbury, and at the dam construction sites of the Ontario

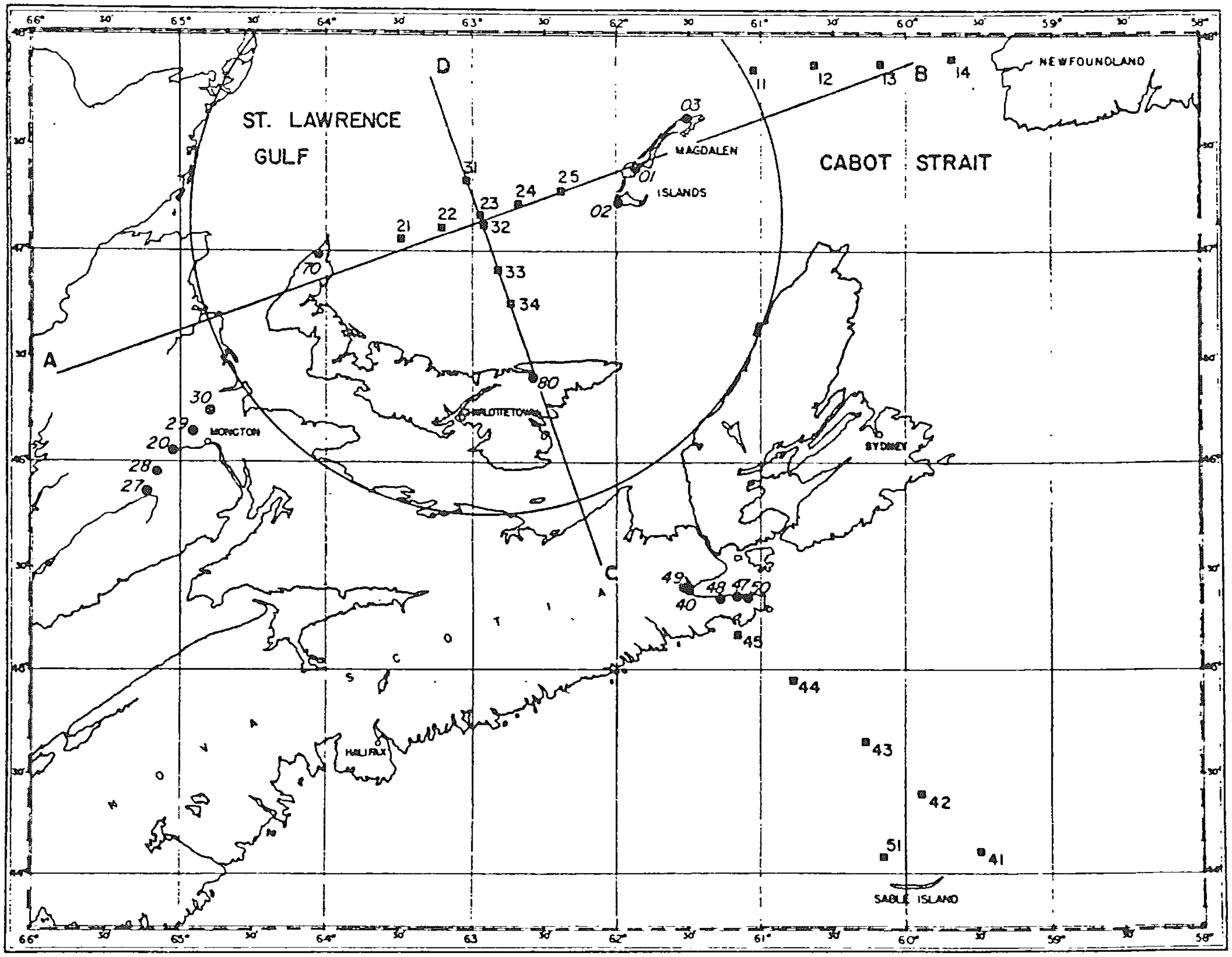
Hydro Electric Commission at La Cave and Rolphton. Arrangements were made to time a number of blasts at each location. The velocities of first arrival P and S phases were determined for several different travel paths over the intervening area and mean velocities could be determined. These proved to be 6.25 ± 0.01 and 3.54 ± 0.01 km/s respectively, in excellent agreement with the velocities obtained from the rockburst refraction profile[37]. So much for the criticism of blasts as an energy source.

Travel-time curves, similar to those shown below, were drawn on the basis of the rockburst results. Dozens of local earthquake records were re-examined in the hope of identifying secondary phases and so determining the focal depths of the earthquakes and refining the curves. The effort was a complete failure; no system to explain secondary arrivals in the P- and S-groups could be found. However the travel-time curves were used successfully for many years in eastern Canada; they were also used in western Canada until local values could be determined.

Southern British Columbia

We saw earlier that W.G. Milne transferred from Ottawa to the Dominion Astrophysical Observatory in 1951 and set up stations at Horseshoe Bay and Port Alberni. These stations, with the existing one at the Observatory, provided coverage of southwestern British Columbia. Late in 1953 a series of depth charges fired by the Royal Canadian Navy in their routine operations, recorded at all three stations. It was apparent that depth charges could provide an energy source for crustal studies. Milne approached Naval officials and the Director of the Pacific Naval Laboratory (PNL) and from these contacts an extensive program developed. Milne was not alone in this work; W.R.H. White joined the staff in 1954 and played a major role.

Several series of depth charges were detonated over the next four years with all the explosions carefully located and timed by PNL scientists. The triangular arrangements of the stations did not provide an accurate "spread", but by extending very long lines of explosions along both coasts of Vancouver Island, conventional techniques were approximated. Analysis of the



From Transactions, Royal Society of Canada, 50, Ser. 3, 21, 1956.

data[38] showed a crustal layer with a P velocity of 6.28 km/s, similar to that found in the Canadian Shield. This layer outcropped under all three stations. Within the distance range available, less than 200 km, the crossover point was not reached. It was concluded that the crust must be much thicker than the 36 km found in the Shield in eastern Canada.

After the data had been analyzed, seismic prospecting refraction equipment became available. It was used to measure the velocities of the surface rocks in various localities. The fact that the crustal layer outcropped under each station was confirmed in this way. Lower velocity volcanics covered the granitic layer south of Victoria and in the vicinity of Ripple Rock, and the Strait of Georgia showed a considerable thickness of sediments, particularly off the mouth of the Fraser river.

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As noted earlier, P.L. Willmore joined the staff of the Seismological Division in the summer of 1952, bringing a wealth of experience in crustal studies. He had been part of the Cambridge team that recorded the post-war explosion that destroyed the German fortress at Helgoland. He then studied the crustal structure of South Africa, using rockbursts in the deep gold mines as energy sources. In connection with this work he had developed the seismometer that bears his name, as well as a cassette recorder that could record up to three components on a single drum carried in a light-proof locker trunk. These instruments were now available commercially. Several sets were purchased, a far cry from the equipment used in our rockburst work, which required a large truck for its transport. Still more simplification was at hand.

Willmore immediately began the design and construction of radio transmitters that could link seismometers as distant as 30 km to a central station where their output was registered on a 12-channel seismic-prospecting recorder. This equipment was first used in the study of the Brent, Ontario, crater, described in an earlier chapter.

Gulf of St. Lawrence

In the search for meteorite craters, attention focused on the Gulf of St. Lawrence; a large circle could be fitted to the shoreline of New Brunswick and Nova Scotia, from the Bay of Chaleur to Cape Breton. If this feature were a meteorite crater, the impact must have shattered the crust and upper mantle, a condition which should be detectable by refraction seismology. Willmore prepared an elaborate field survey in cooperation with the Royal Canadian Navy, which supplied and fired the depth charges used as explosive sources.

The plan of the operation[39] is illustrated in the figure on the preceding page. The project fell into three parts. Instrument stations, indicated by circular symbols, were set up on the Magdalen Islands, on the northwestern tip of Prince Edward Island and along a profile running south-southwest from the vicinity of Moncton. Depth charges were fired within the Gulf along two lines at right angles, as indicated by square symbols. The instruments were then moved to eastern Prince Edward Island, to Sable Island and to a profile near the Canso Strait, and depth charges were exploded along a line between Canso and Sable Island. Radio links and the 12-channel recorder were used with the profiles near Moncton and Canso, the light-proof Willmore recorders at the other positions.

Because of wave-generated noise, no useful data were recorded on Sable Island, but good records were obtained elsewhere. However, they were of such complexity that only first arrivals could be used. These first arrivals defined velocities that agreed reasonably well with the crustal and sub-crustal velocities found in the Canadian Shield, but variations from the line indicated an erratic structure. Furthermore, the failure of the stations on Sable Island to record meant that no reversed profile was obtained in the Atlantic section of the program. A more penetrating analysis than the usual least-square fitting of points to straight lines had to be found.

The method developed to handle the data within the Gulf[40] was due to the seismological intuition of Willmore and the mathematical competence of A.E. Scheidegger, who assisted him in the field work. It was called the "time term" method, and it became an important tool of crustal seismology. The travel-time between a shot-point A and a recording point B was expressed in the form

$$t_{AB} = a + b + \Delta_{ab}/v.$$

where v is the velocity of the refracted wave, Δ_{ab} the distance between the two points and a and b "time terms" under the shot and recorder respectively. The time terms are proportional to the thickness of the layers penetrated in reaching the refracting layer. Since each explosion was recorded at more than one recording site there was an excess of equations to solve for all the time terms involved. The solution became a matter of matrix inversion.

Applying the method to the Gulf data showed a great thickness of sediments in the centre and under Prince Edward Island. However the depth determined, about six km, was ambiguous in answering the question about a meteoritic origin for the Gulf.

The failure of the experiment on Sable Island called for other methods. Two short arrays of seismic-prospecting geophones, with shots covering distances out to 2000 feet, failed to show any evidence of a basement rock. Radio-linked stations extended this range to about 18,000 feet, without finding evidence of a basement. It was concluded that no rock existed within 1800 feet of the surface and that the Island was a result of winds and currents[41].

The survey involved the coordination of many groups, the Royal Canadian Navy, the Eastern Oceanographic Group and the Nova Scotia Research Foundation. This coordination was arranged by Willmore who then, as in subsequent experiments, showed a great ability in getting people to work together effectively.

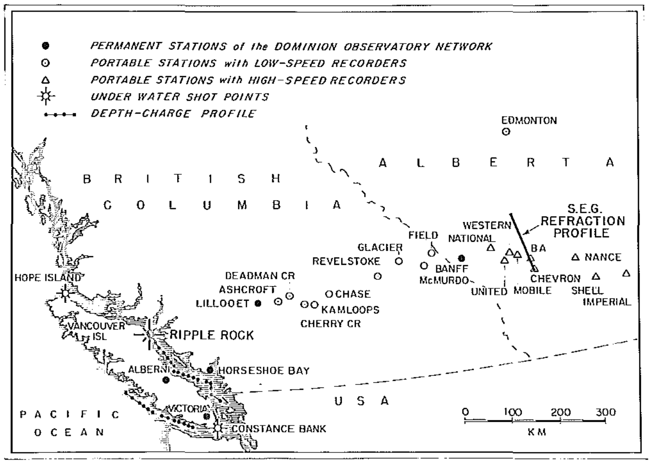
Ripple Rock

The next major effort, also directed by Willmore, required all his abilities as a coordinator. It involved recording the explosion of Ripple Rock. This large twin pinnacle of rock in

Seymour Narrows, between Vancouver Island and the mainland, reached close to the surface at low tide and so constituted a hazard to shipping. It was decided to destroy it by a large blast. A tunnel was driven from the Island to the pinnacles, they were hollowed out, filled with 1500 tons of explosive, and exploded in a single shot on April 5, 1958. Willmore selected a series of nine recording points across the Cordillera at distances from 300 to 700 km, and pressed all Division members, except the secretary, into service to staff these stations. One radio-linked array of three stations was deployed, based on Cherry Creek; the other stations used the Willmore field recorders. In addition to the Observatory-operated stations, Willmore arranged for commercial seismograph crews to record the event at a number of points in southwestern Alberta.

Accurate timing was of the utmost importance, particularly for the commercial parties which were not equipped to work in absolute time. Willmore arranged for the CBC to carry time signals as part of its radio broadcast of the event, convincing them that these would add to the interest. The commentator describing the explosion talked for some thirty-five seconds after the event and then announced that, since the seismic wave was now about to reach Cache Creek, the nearest station, the time signals would take over. We all recorded the signals, without voice interruption, and after about four minutes, when the waves had passed southwestern Alberta, the announcer went on with his commentary. It was dramatic, and provided excellent timing.

The Ripple Rock explosion.



The recording network set up for the explosion.

Willmore made a preliminary analysis of the results but, unfortunately, never published them nor any description of the project. It remained for White to make the analysis as part of a larger study we shall now consider.

Southern British Columbia, ctd.

This work was part of White's doctoral thesis[42] at the University of British Columbia, where he was supervised by Professor J.C. Savage. He made use of the data already analyzed[38] and arranged with the Navy and the Pacific Naval Laboratory for five additional series of explosions. All shots were recorded at the three permanent stations of the southern British Columbia network; in addition, the standard station at Port Hardy, on northern Vancouver Island, was in operation for the later shots. Several portable stations were deployed, moved about to make the best use of each series of shots.

A number of large explosions, made up of ten depth charges, were fired at the north and south ends of Vancouver Island. These and the Ripple Rock explosion provided a profile parallel to the coast extending as far south as northern California, well beyond the crossover point, and so revealed the existence of a second crustal layer. The depth to this layer varied from 11 km on the west coast of Vancouver Island to 5 km under the Strait of Georgia. A thickness of more than 40 km was indicated, leading to a total crustal thickness of more than 50 km.

This thickened crust did not extend to the interior of the province. The Ripple Rock observations indicated a crust 30 km thick under the Interior Plateau and the Rocky Mountains with a mantle velocity of 7.8 km/s. The crust thickened again under the Alberta plains.

Milne and White were joined by M.N. Bone in 1964. This enhanced group extended its crustal studies to the interior of the province. Between 1965 and 1966 a number of profiles, some of them reversed, were shot along the length of the Interior Plateau. The Ripple Rock records were available to provide a traverse across the Cordillera, and one line was shot westward toward Victoria from a point near Revelstoke. Explosions were fired in lakes and use was made of mine explosions where they were suitably located.

Two models could be used to explain the observations along the length of the Plateau. In one, the Mohorovicic discontinuity dipped from north to south at a slight angle, with a mean depth of about 30 km; in the other it dipped from both north and south to a maximum depth of 30 km under 100 Mile House. The profile, from east to west across the strike of the Cordillera, partially reversing the Ripple Rock profile, confirmed the earlier Ripple Rock analysis[43].

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Willmore left the staff in 1960. A period of administrative confusion followed, marked by controversy between the Director and myself. He wanted to continue the crustal work; I was opposed, our staff was too small. We had embarked on an expansion of the seismological network, with all the problems of installation, training and management. We would be faced with a proliferation of records, from stations covering half a continent; if we didn't mine at least some of the research possibilities they provided we would become simply a service organization. I couldn't accept this. Beals, on the other hand, wouldn't countenance abandoning field studies of the crust.

A compromise of sorts was found in the Polar Continental Shelf Project, which provided the logistical support for major projects in the Arctic. The Geological Survey was already conducting seismological studies within the Project, aimed at defining sedimentary basins with petroleum potential. In 1962 the scientific direction was transferred to the Observatory, and the emphasis of the work changed to the investigation of the crust and upper mantle. A. Overton and W. Tyrlik were transferred from the Geological Survey, and G.W. Sanders was recruited as leader. The organization of this party marks the advance to complete instrumental sophistication. Twelve-channel recorders, of a type designed for commercial prospecting, became standard. They could record the output from all three components at four stations, or a single component from twelve different stations. The seismic investigations of the craters at Deep Bay and Clearwater Lake, made by this team, have been described in an earlier chapter.

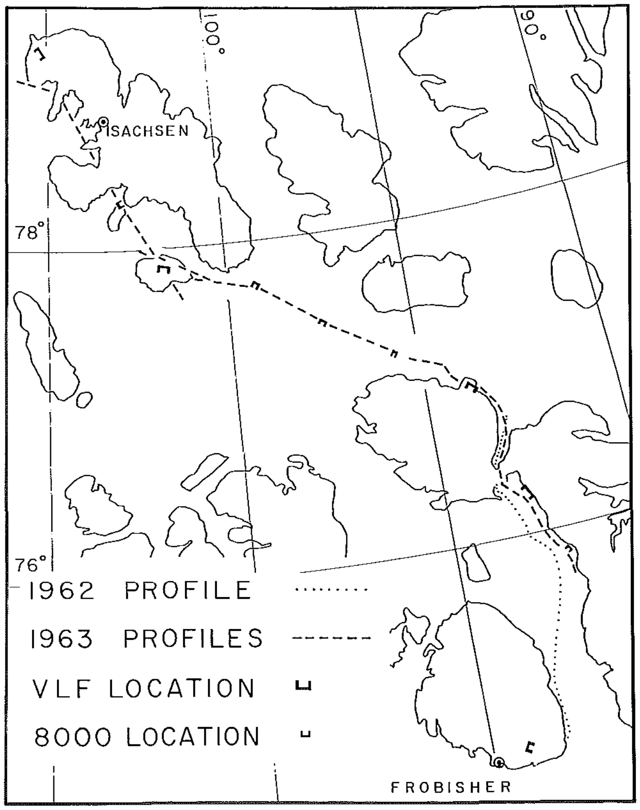
Arctic Islands

The group completed three seismic profiles during the 1962 and 1963 field seasons, covering a traverse from Cornwallis Island to the Arctic Ocean at Cape Isachsen. Each recording station consisted of an array of 12 seismometers, 600 m apart, oriented in the direction of the shot line. They were connected to the recording station by cable. The recorder, housed in a double-walled heated tent, also registered the time of the explosion, transmitted by radio. In addition, absolute time was recorded from crystal-controlled chronometers; this permitted recordings from the standard stations to be used in the analysis.

The recording stations and their spread of seismometers were placed on land, about 250 km apart, sufficient to ensure the first arrival of the mantle P phase. A shooting party traversed between them on the ice, drilling holes and setting off shots at intervals of approximately 10 km. This provided continuous, closely-spaced, reverse profiling. When the end of the profile was reached the lower recording station was leap-frogged over the upper one to become the upper stations of a new profile.

The dimensions of the operation were much larger than usual in the Polar Continental Shelf Project. The shooting party could not be supplied on a day-to-day basis by air. It carried its supplies, including large quantities of explosives, in a tractor-train drawn by a tracked vehicle, known as a Nodwell. Temperatures of -30°F were common and equipment failures, resulting from these temperatures, were frequent. On the other hand the deep-freeze conditions made for a low noise level.

The survey[44] showed a two-layer crust with a total thickness of about 37 km, with upper crust and mantle velocities similar to those found in the Shield. At the southern end of the traverse it was impossible to distinguish the Palaeozoic sediments from the upper crustal layer, but on the northern traverse the sedimentary layers were defined by additional, short-range, arrays at intermediate points along the traverse.



The Arctic islands seismic profile, 1962-1963.

The Nodwell tows supplies over the sea ice. From Geophysics, 30, 91, 1965.

Cooperative Projects

A number of major, "combined operation", crustal projects, marking a new beginning in crustal studies, took place in the period 1963-1966. Modern instruments were not very expensive and many university departments, both in the United States and Canada, had acquired them; in the case of Canadian universities this was largely a legacy of the Upper Mantle Project. The necessary explosives, and the techniques of handling and firing them, were another matter. Much more effective use could be made of the explosions if several groups worked together.

The first of these operations, in 1963, was the "Lake Superior Experiment" in which lines of shots were fired in the lake and their output recorded on land by parties fielded by fourteen organizations, five of them Canadian. J.S. Steinhart, of the University of Wisconsin was the overall organizer of the project[45]. G.F. West, of the University of Toronto, acted as Canadian coordinator, and, with M.J. Berry, provided a time-term analysis of the results[46]. The Observatory contributed a line of stations to the main project, but did not take part in the analysis. We did field a small party led by A.M. Bancroft, to study the frequency spectrum of waves from underwater explosions. Bancroft recorded 24 of the explosions on broad-band magnetic tape[113] and showed that their velocity amplitude spectra varied with the depth of the explosion; he was thus able to define an optimum shot depth[47].

In 1965 the Department mounted a major geophysical program in Hudson Bay. Again the Observatory contributed recording stations to the project[48] but did not take part in the analysis. In 1966 another US-organized project in Lake Superior, "Project Early Rise", involved large explosions intended to provide recordings at mantle distances. It had Canadian participation but none whatever from the Observatory.

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Why was the Observatory playing such a small part in these important projects? Partly it was a result of my reluctance to become involved myself or to assign other staff to it. Indeed there was no one to assign. A.M. Bancroft transferred to Seismology from Gravity in 1958; he contributed to the planning of crustal work including, in collaboration with Willmore[49], a more sophisticated analysis of the time term method. However, for medical reasons, he was not able to take responsibility for a major party. The small crustal group of Sander, Overton and Tyrlik was over-extended with its work in the Arctic and the demands made on it for the seismic investigation of meteorite craters. Dissension gradually built up within the group and in 1965 it broke up, with Sander going into private practice and Overton returning to the Geological Survey. Bancroft resigned at about the same time. A complete reorganization was necessary.

This instrument, used by Bancroft in the Lake Superior project, records the output of three seismometers on FM magnetic tape. It is a far cry from the cumbersome equipment used in the rockburst work!.

These changes coincided with others. Beals retired in 1964, I succeeded him as Director, and K. Whitham became Chief of Seismology. New positions were made available and Whitham began immediately to recruit an experienced team. K.G. Barr, who joined the staff in 1965, had been in charge of the British seismograph network in the Caribbean. M.J. Berry joined us in 1967. He had assisted West in coordinating the Lake Superior experiment, and had analyzed the results as part of a PhD thesis. D.A. Forsyth joined in 1967, and M.N. Bone, transferred from Victoria, to complete the team.

Bone's transfer marked the end of the DAO crustal group. White had resigned in 1966 and, while he had been replaced by G.C. Rogers, it was felt that the staff was too small to handle major crustal projects on its own.

Upper Mantle Structure

The large explosions of the Lake Superior and Hudson Bay projects had recorded on most of the standard stations of the Canadian network. One of Barr's first contributions was to study these observations and to deduce from them the structure of the upper mantle in Canada. All the prominent phases in the first twenty seconds of the records were read and secondary arrivals were used to define details in the travel-time curves which in turn defined the mantle structure. Two discontinuities were identified in the upper mantle[50]. The structure was believed to be uniform throughout the Shield and the Canadian Arctic.

In 1963, in cooperation with the British Ministry of Defence, the Department had constructed a seismic array at Yellowknife, NWT. This array, to be described in a later section, was designed to assist in the detection and identification of nuclear explosions. The site of the array had been selected because of the low seismic noise level in the area and because it was logistically possible. Once the array was in operation variations in local structure were found to be influencing the accuracy with which distant events could be located. This might have been foreseen since the array was close to the boundary between two major Precambrian provinces, the Slave and the Churchill provinces. Two studies contributed to an understanding of the structure.

D.H. Weichert, analyzing the array recordings from the same series of explosions used by Barr, defined the mantle structure in the neighbourhood of the array in the directions of the various explosive sources[51]. His results differed in detail from the average values found in Barr's wider area of study. They suggested significant P-velocity differences in the upper mantle under the two different Shield provinces.

Yellowknife, N. W. T.

The first project of the newly-organized crustal group, in the summer of 1966, also aimed at defining the local structure in the area of the array. It marked a new departure in organization. One member of the group would take responsibility for the project – plan it, arrange the logistics, supervise the field work and publish the results; the other members would assist in the field but return to their own interests when the field work was done. In this case the responsible officer was Barr[52].

Ten portable recording stations were available. Four consisted of three orthogonal components recording on a twelve-channel magnetic tape. The remaining six, two of which were provided and manned by the University of Alberta, were small spreads consisting of six vertical components spaced 500 m apart and recording both on paper and magnetic tape. Recordings from the array itself were of course part of the output.

There were two stages. In the first the stations were widely dispersed in an areal pattern to provide data for a time-term analysis. In the second they were arranged in a line, through Yellowknife, to give a reversed profile. Explosions were fired in Great Slave Lake and in other lakes selected to give the most effective data for the two types of analysis.

The structure proved to be simple, a shallow surface layer overlying a uniform crust of standard velocity; the velocities below the Mohorovicic discontinuity were reasonably standard but showed a significant difference for the Slave and Churchill provinces. Amplitudes were consistent with this basic interpretation. The crust had an average thickness of about 34 km, but there were significant local variations.

Weichert and Whitham[53] analyzed the array recordings of the explosions to define the fine structure in its immediate vicinity. Using all the tricks of analysis made possible by the broad-band magnetic tapes, they found the crust to be uniform and the M-discontinuity to be horizontal within a few tens of kilometres of the array. Beyond that distance the crustal uniformity breaks down, but for most teleseismic arrivals these inhomogeneities would have negligible effect.

Arctic Continental Shelf

In the spring of 1967 Berry and Barr attempted to measure a crustal profile across the continental shelf of the Arctic Ocean off Prince Patrick Island[54]. They had intended to fire a series of shots at 20-km intervals along a line at right angles to the coast. Recorders were to be operated at the land end of the line and, on the ice, at distances of 200 and 400 km from shore. The experiment was plagued with difficulties: temperatures constantly below -40°C, and the consequent failure of aircraft, made it necessary to limit the experiment to a 200-km line; the Decca navigational system on which they were depending for positioning was erratic; the ice, under both the shots and the 200-km station was in constant, erratic, motion; there were discrepancies between the recordings on their land-based station and those at the nearby standard station at Mould Bay. Despite these difficulties they were able to deduce a thinning of the crust, from approximately 30 km under Prince Patrick Island to 11 km 200 km offshore. There was evidence that this thinning must continue seaward, which strongly suggested an "oceanic" crust for the Arctic Ocean.

Grenville Front

The next major crustal project was conducted during the summer of 1968 with Berry in charge. It was concentrated along the Grenville Front, the boundary between the younger Grenville province to the south and the older Superior province to the north (see page 124 for a map of these provinces). The Front was associated with a major gravity anomaly. The shooting pattern was elaborate. It was designed to permit three reversed profiles, one along the Front, one parallel to it in each of the two provinces, with good ties between the lines and to the region to the south. The University of Western Ontario (UWO) occupied a series of stations to the west, Dalhousie University a series of stations to the southeast and a group from the Geological Survey stations on both sides of the St. Lawrence River in the vicinity of Baie-Comeau.

The data proved to be intractable; addition of the UWO observations to the west provided some assistance and some additional problems. Anisotropy in upper mantle velocities as well as considerable topographic relief on the Mohorovicic discontinuity seemed to be indicated.

Berry, joined by K. Fuchs, a visitor from the Fridericiana University of Karlsruhe, made use of the technique of "synthetic seismograms". Starting with models suggested by the initial analysis, they computed the form of secondary arrivals and compared these with those observed. Then, gradually modifying the model, they sought agreement between the computed and calculated phases. A complex picture emerged. In addition to possible anisotropy in the upper mantle, it showed a two-layered crust with a low-velocity channel in the upper layer, and a crust-mantle contact marked by a rapid change in velocity rather than by a discontinuity[55].

Yellowknife, ctd.

The technique of synthetic seismograms had proved to be a powerful one, revealing structural details unsuspected from conventional analysis. Barr and Berry returned to Yellowknife in 1969 to determine whether the new method would show a more complex structure there as well. Observations were made in the vicinity of Yellowknife, with station separation of only 250 m. By unexplained good fortune, recording frequencies peaked at 10 Hz, which, with the aid of ray tracing and synthetic seismograms, defined a fine structure much more complex than that suggested by the earlier work. There was good correlation with observed geological boundaries, a well-defined low-velocity layer was found in the crust, and there were suggestions of lateral variations in the lower crust and in the topography of the M-discontinuity[56]. The ray tracing was done using a program recently developed by T.E. Clee, a graduate student at the University of Toronto; he was listed as first author in the paper, in recognition of this important contribution.

Central British Columbia

In 1969 the Observatory mounted Project Edzoe in which explosions, set off in Greenbush Lake, near Revelstoke, were recorded by eleven different groups in western Canada and in the northwestern United States. The explosions were used by the Dominion Observatory to extend the north-south line along the Interior Plateau considerably to the north. A team from the University of British Columbia occupied stations along the highway from Prince George to Prince Rupert; in 1970 this profile was reversed by shots in Bird Lake on Graham Island in the Queen Charlotte Islands.

Both the Observatory and UBC teams recorded on analogue magnetic tape and the records were digitized to permit processing by computer. As in the recent projects in Eastern Canada, attention was paid not only to first arrivals but to secondary ones, and not only to arrival times but to amplitudes. Again the analysis proved to be powerful and produced detailed pictures of the crust. A time-term analysis was also made[57]. The results were incorporated in the review of Cordilleran geophysical studies[58] described in earlier chapters.

The use of secondary arrivals through the techniques of synthetic seismograms had proved to be a powerful tool, defining detail in the crust and mantle as well as unsuspected variations in velocity. How accurate were the earlier studies based simply on first arrivals? Berry[59] found that the method could lead to errors of up to 20%, probably substantially larger than the uncertainties in arrival times and least-square fits would suggest.

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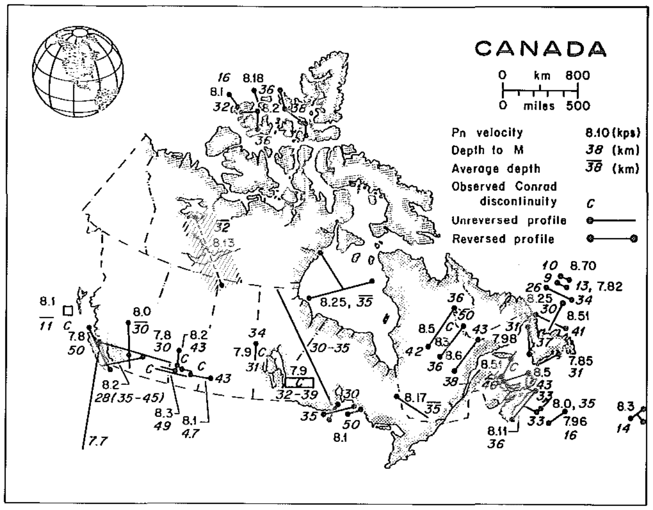
In the two decades between the rockburst travel-time project and the close of our period of interest, crustal studies had evolved remarkably: from the use of clumsy and inadequate equipment to the use of sophisticated and highly portable instruments specifically designed for the work; from the determination of layer velocities and thickness by travel-time graphs to the deduction of detailed crust and upper mantle structures by comparisons of travel-time and amplitude data with elaborate theoretical models generated by powerful computers. Much of the work had been done by Observatory personnel. Other studies involved University groups from both Canada and the United States; the development of a seismological competence within the Canadian universities owed much to our leadership. This development reinforces the statements made in the opening chapters: the period was marked by the development of sophisticated and readily-portable instruments, by the availability of increasingly powerful computers, and by the expansion, with Observatory help, of geophysical competence within the universities.



Measurements of crustal thickness and upper mantle velocities in Canada to the end of 1970. After M.J. Berry, Tectonophysics, 20, 184, 1973.

Surface Wave Studies

The seismic waves we have been discussing in the section above are "body" waves, so-called because they penetrate into the body of the earth. Much of what we know about the earth's crust and upper mantle, and most of what is known about the deep interior, comes from the study of body waves.

Seismic energy also travels to great distances along the surface of the earth. There are two types of surface waves, each named after the mathematicians who discovered them. The first are Rayleigh waves, in which the particles move in the plane of propagation; there is no particle motion at right angles to this plane. The second class of waves, Love waves, require a layer, such as the crust, in which to travel. Their particle motion is always at right angles to the plane of propagation. Thus, if an earthquake occurred directly south of a station, the Rayleigh waves would record on the north-south and vertical components but not on the east-west; the Love waves on the east-west component, not at all on the vertical or north-south.

Both waves show "dispersion": their velocity depends on their wave-length, the longer wave-lengths travelling faster than the shorter ones because they penetrate to deeper and faster rocks. At great distance from the source they therefore appear as a wave train of constantly decreasing wave-length. Since the distance to the source is known the average velocity of each wavelength can be computed; when these velocities are plotted against period, the "group velocity" curve is obtained. Since it reflects the average structure between source and recording point, it is important that the path be either all-ocean or all-continent.

If two seismograph stations happen to lie on the same wave path of a surface wave, it may be possible to correlate the records and so determine the velocity with which each frequency has travelled over this shorter distance. When velocity is plotted against frequency in this case we obtain the "phase velocity" curve. It is diagnostic of the structure over the distance between the two stations.

The velocities with which the surface waves travel depend on the number and thickness of layers in the crust, the velocities and densities in each, and on the layering, velocities and densities in the upper mantle. Early studies showed different group velocity curves for oceanic and continental paths; these were the first indications of crustal differences. With the advent of high speed computers it became possible to design models that fitted the observed curves closely.

Professor J. Coulomb was the first of a series of "distinguished visitors" to spend the summer months at the Observatory; he was there during the summer of 1950. Milne had assembled the seismograms for the Queen Charlotte earthquake of August 22, 1949, and for two of its aftershocks and had already computed the epicentre. Because the earthquake was on a continental border, Coulomb recognized that it provided an opportunity to study the propagation of surface waves along all-continental and all-Pacific paths.

This study[60] was made before the great explosion of surface wave studies that was soon to begin, and it is interesting to see the problems that Coulomb faced with inadequate instrumentation and station distribution and with uncertain theory. He was however able to produce dispersion curves for Love waves over both continental and Pacific paths, and for Rayleigh waves over Pacific ones.

It was more than ten years before Observatory seismologist returned to the study of surface waves, although the expanding Canadian standard network contributed data to such studies. We have mentioned elsewhere that the Observatory cooperated in the surface-wave studies of the Columbia group by operating special instruments for them in the new Resolute vaults during the IGY. Brune and Dorman[61], of the Lamont Geological Laboratory, used data from this and other Canadian stations to define the crust and mantle structure of the Canadian Shield. They compiled both Love and Rayleigh phase velocity curves between several pairs of stations and deduced an average crustal structure that could account for these curves. It involved a three-layer crust, 35 km thick, and a low-velocity layer in the upper mantle.

During 1961 and 1962 the Soviets were conducting atmospheric tests of nuclear devices at Novaya Zemlya, an island in the Soviet Arctic. These explosions produced Rayleigh waves on the long-period instruments of the Canadian standard network, particularly at the Arctic stations. G.G.R. Buchbinder was able to compute the group velocity curves from Novaya Zemlya to Mould Bay, Alert and Resolute in the Arctic and to London, Ottawa, Penticton and Victoria. By good fortune the azimuths from Novaya Zemlya to Alert and to Resolute were almost the same so that the phase velocity curve between Alert and Resolute could be determined. Using a program developed at Columbia University he then searched for a crustal model that would fit this curve; a three-layer crust with a total thickness of 38 km produced the best fit[62].

Mould Bay and Penticton were also nearly in the same azimuth from the source, and a phase velocity curve for this pair was calculated. Because of their large separation and the many geological provinces traversed, no attempt was made to fit a curve to these data.

Dr. K. Pec, of Charles University, Prague, Czechoslovakia, came to the Observatory as a postdoctoral fellow in 1966. His first work was the completion of a project begun at home, the preparation of dispersion tables for Love waves in a wedge and in a layer with a linear velocity gradient[63]. He then joined A.J. Wickens in an interesting study in which his tables were necessary.

Wickens had noted that a number of Canadian standard stations – Mould Bay, Coppermine, Yellowknife and Edmonton – and WWSSN stations at Dugway and Tucson, lay along a single great-circle path. By selecting appropriate earthquakes north and south of this line, the stations provided a reversed spread over which the propagation of Love waves could be studied. The group and phase velocities of the waves were determined by a Fourier analysis and dispersion curves were prepared for each section of the traverse. A crust and upper mantle model was fitted to these curves. It showed a low velocity zone in the upper mantle which dipped to the northward, from Dugway to Mould Bay. It was possible to determine the dip – 6° – from the Pec tables[64].

Some time later Wickens extended the study to the rest of Canada. Selecting pairs of stations, he sought earthquakes along the same great-circle path as each station pair and studied the Love and Rayleigh waves for each pair. In selecting a model to explain the observed dispersion curves he used refraction results, where available, to define the crust, and adjusted the assumed mantle structure to match the longer period dispersion. He found five "regions" in Canada – Shield, North, West, South and East – their boundaries not sharply defined, but each with a distinctive structure[65]. For sharper definition he recognized the need for controlled experiments with long-period instruments deployed along the lines of existing or proposed refraction profiles. The realization of this idea came after the period we are reviewing[66].

STUDIES OF THE EARTH'S DEEP INTERIOR

A principal contribution of seismology has been in the exploration of the interior structure of the earth. The Observatory has not been a major contributor to this study; its interest has been largely limited to the crust and upper mantle. There are however a number of important papers which should be discussed.

Two of these were produced by "Distinguished Visitors". Both K.E. Bullen and Inge Lehmann worked at the Observatory during the summer of 1954, and Dr. Lehmann returned again in 1957. Both of them considered the travel-times of body waves. Bullen was interested in the minute details of the travel-time curves for pP and PP[67], and their relationship to the fine structure in the upper mantle. Lehmann worked from the observed curves of P and pP in Europe[68] to define the velocity structure in the upper mantle. It was a great privilege to work with these distinguished guests.

From 1964 on, G.G.R. Buchbinder made a major contribution to the knowledge of the core and the core-mantle boundary. This work was done as part of a doctoral program at the Lamont Geological Observatory of Columbia University.

In a first paper he studied the phase reflected at the outer core – PcP – and found that it underwent a phase reversal at 32° epicentral distance, with a consequent amplitude minimum. From this information he was able to compute a model for the lower mantle and upper core which satisfied other core phases as well as PcP. His model did not agree with the conventional one; the data could however be reconciled by postulating an inhomogeneous layer at the base of the mantle[69].

The records used in this study derived from nuclear explosions and from earthquakes. In a second paper[70] Buchbinder examined the amplitude spectra of the PcP and P phases. He found that the spectra of the two phases were the same for any one event, showing that the core-mantle boundary effects were small, and that the general shape of the spectra was related to the source. In general he found a difference between spectra produced by explosions and earthquakes, but some intermediate and deep-focus earthquakes produced spectra very like explosions. For the latter the magnitude of the event influenced the spectrum; this was not generally true for earthquakes. No evidence was found for layering at the core-mantle boundary.

In his studies of the variation of amplitude with epicentral distance it was necessary to consider also the effect of focal depth. Adjustments had to be made to the travel-times for which Buchbinder provided the necessary tables[71].

Buchbinder's interest in the deep interior of the earth continued after his thesis work was completed. After his return from Lamont he produced three important papers in which the velocities and densities within the core were defined and tested against travel-time observations[72].

STUDIES OF EARTHQUAKE MECHANISM

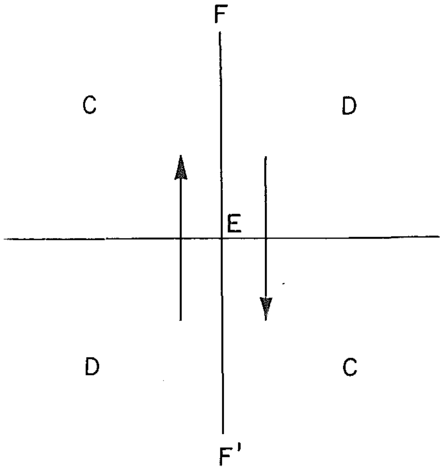
During the time that I was on the staff of the University of Toronto I was under the influence of Tuzo Wilson's dynamic thinking about earth processes. Some of his theories seemed plausible, others less so. Shortly before moving to Ottawa in 1949 I became aware of Byerly's work on the mechanism of earthquakes. If the method was reliable it should provide definite information on the forces acting in the earth's crust and upper mantle, and I determined to exploit it fully.

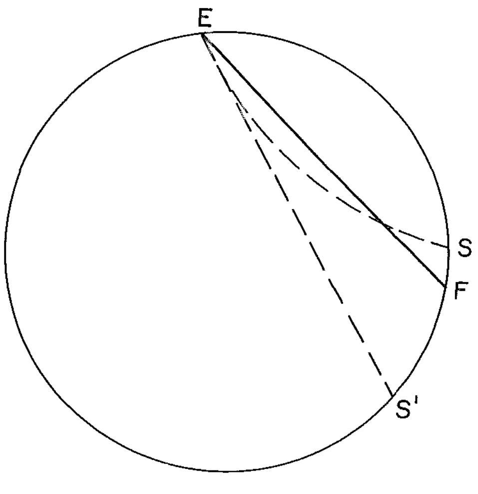
What was the method? The then generally accepted idea of the cause of earthquakes was embodied in the "elastic rebound theory" developed by Reid to explain observations in the 1906 San Francisco earthquake. That earthquake resulted from transverse motion along the almost vertical San Andreas fault. Reid's theory was that stresses had built up along the fault over a long period of time and had been released, impulsively, at the time of the earthquake.

In the figure (opposite column, top), FF represents the trace of the fault on a horizontal plane through the focus E, presumed, for this definition, to lie on the surface. The motion along the fault at the instant of the earthquake is indicated by the arrows. It is intuitively evident that, at the time of the earthquake, points "ahead" of an arrow will feel themselves pushed, points "behind" an arrow will feel themselves pulled. The area will be divided into four quadrants alternatively compressional (C) and dilatational (D). If we had a number of seismograph stations in the area, we could separate those receiving an initial C from those receiving an initial D by a pair of orthogonal planes, one of which would represent the fault; the other is called the "auxiliary" plane.

Faults are not normally vertical and fault motion is not normally purely horizontal, but we may generalize the situation, and extend it to a world-wide distribution, as shown in the figure below (bottom). Here the fault plane, drawn perpendicular to the paper, meets the earth at the point F. Then the four zones will be defined by the plane EF and the plane of the paper.

However, there is a difficulty. Consider the station S. Because the ray reaching it is curved due to the increase of velocity with depth within the earth, the ray reaching S, to the right of the fault actually originated to the left of the fault; an observation of C or D plotted at S will simply confuse the picture. This is avoided by plotting the data at the point S' where the tangent to the ray meets the earth. It would be possible, with the stations plotted in these "extended positions" to draw orthogonal great circles on the globe separating zones of initial compression from zones of initial dilatation.





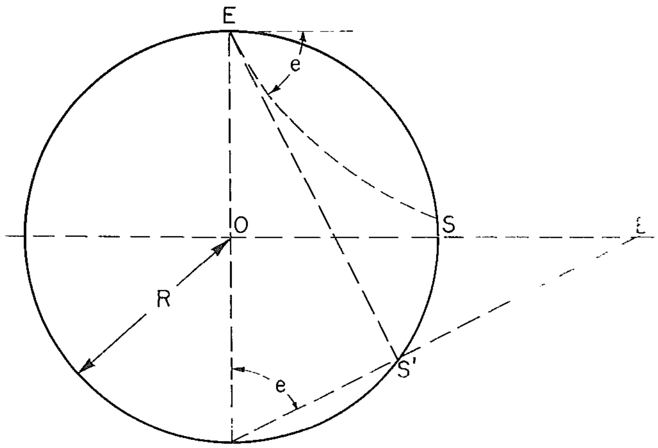
It is not convenient to work on the globe. Instead we make use of the stereographic projection in which, as we saw in Part I, great circles on the globe translate to circles on the map. There are a variety of ways in which this may be done; Byerly projected from the antipodal point of the epicentre on to the corresponding diametral plane as shown in the left-hand diagram of the figure below; the point S', the extended position of the station S, projects to the point B at an "extended distance" OB. Once the points are plotted the compressions can be separated from the dilatations by a pair of circles, as shown in the diagram on the right. There is a constraint on these circles since they must represent orthogonal planes. The tangents to the circles at the epicentre E represent the strikes of the planes and their radii are proportional to the dips; one of the planes represents the fault, the other the auxiliary plane, but it is not possible to distinguish between them on the basis of P data alone.

We shall see presently that the mechanism of faulting is not as simple as this intuitive model suggests, but it was this model, and the possibility it held for defining the details of faulting in earthquakes, that led to the acceptance of the "fault plane project" as a major Divisional research effort, starting in 1949 and continuing for some 15 years.

As a first step in the Observatory program Milne attempted a fault plane solution for the British Columbia earthquake of June 23, 1946[73], for which he had collected the records from around the world. The results seemed consistent with geological data. The next step[74] was to consider four earthquakes of the north Pacific. One of them was the 1946 earthquake reconsidered; for the others the data were taken from bulletins, augmented by questionnaires. Again the results seemed reasonable.

All the earthquakes so far considered had normal focal depth. In a general program it would be necessary to consider earthquakes of all focal depths. The first step was to compute tables of extended distance for all focal depths. This was done[75], and the project was ready to move into high gear.

A decision had to be made about the data to be used. In the case of the 1946 British Columbia earthquake the original seismograms had been available. This was a great advantage but the problem of assembling original records for large numbers of earthquakes was formidable. In those days it normally took about a year to collect a set of records; to collect many sets would have imposed an intolerable burden on collaborating stations. It was decided to depend on the stations' own readings. Questionnaire forms were prepared for each earthquake, or for each group of earthquakes, and these were filled in from bulletins as much as possible before being dispatched. The response from stations world-wide was excellent and solutions were published for many earthquakes[76]. Where faulting had been observed or could be inferred, the solutions were generally confirmed in that one of the planes fitted the observed strike of the fault; however, to an increasing degree, motion along this fault had a much stronger lateral component than was observed or expected. Faults with large lateral movement – transcurrent faults – were known in some areas, notably California, but vertical faulting, normal or reversed, was believed to be the norm.



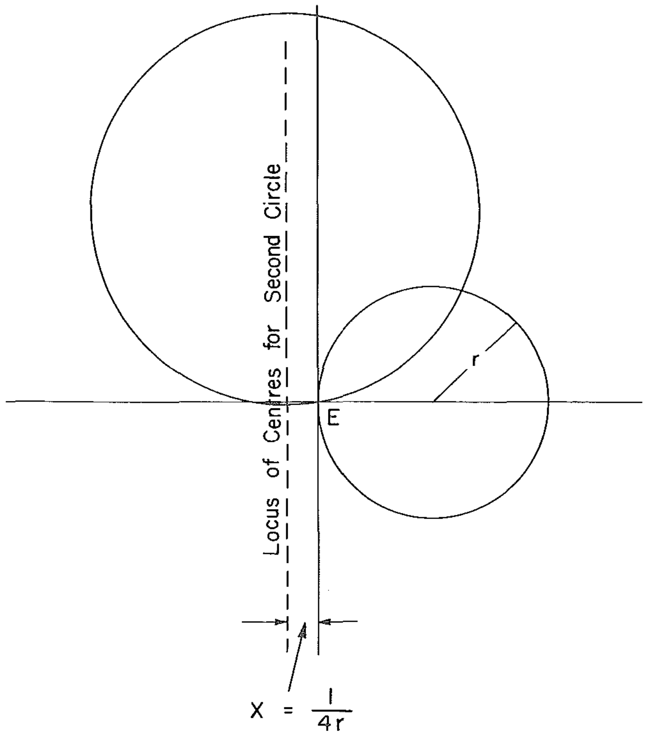
A number of sets of records, collected from around the world, were found in the Observatory files – for the St. Lawrence, Temiskaming, Grand Banks and Cornwall earthquakes in Canada and for the Tango, Japan, earthquake of 1927. These had been collected for research purposes and had not been returned to their owners. Solutions were attempted for all these earthquakes but, while tantalizing solutions were suggested for all of them, the data could support publication for only one, the Tango earthquake[77].

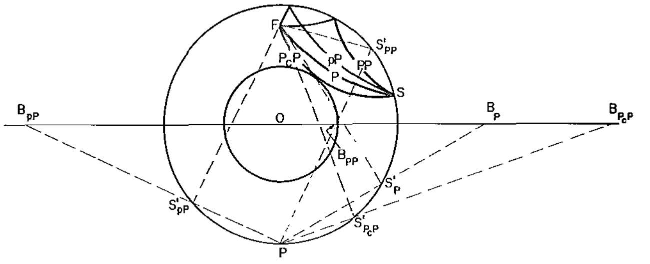
The returned questionnaires frequently gave information on phases other than the direct P wave, phases reflected at the surface of the earth or at the core; these phases are illustrated in the figure opposite page (left column, bottom). If they were to be used, extended distances would have to be computed, and much effort was devoted to this[78].

Phases reflected, either internally at the earth's surface of externally at the earth's core, may suffer a phase change on reflection, an initial compression becoming a dilatation, or vice versa. This would of course have to be taken into account in plotting the data. Father R.E. Ingram, S.J., of Dublin, Ireland, joined me in an analysis of this problem[79].

With these matters accomplished the project became almost routine. Each year a selection was made of 30 or 40 of the world's largest earthquakes, questionnaire forms were prepared and distributed, returns were collected, data analyzed and published[80]. Handling of these vast amounts of data was only possible because of the dedication of a succession of summer students whose names will be found as co-authors of the referenced papers.

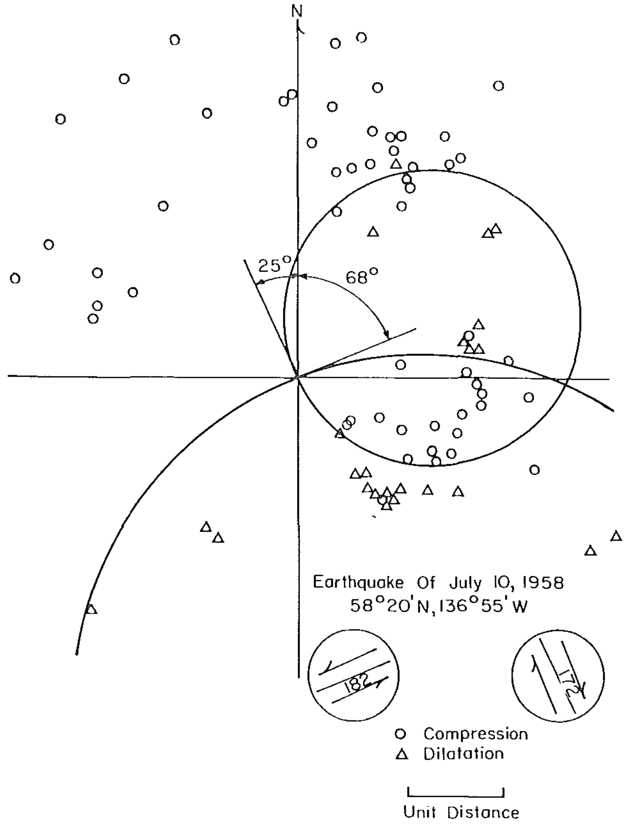
Before a solution could even be attempted it was necessary to compute the azimuth and distance of each recording station from each earthquake. At the start of the program this was done with a desk-top calculating machine using seven- place logarithms; one could do about four such calculations per day. When the first University of Toronto computer became operable C.C. Gotlieb visited the Observatory to drum up work for it. The only thing we could think of in seismology was to have them compute distances and azimuths for the fault-plane work. They were given the coordinates of all seismograph stations and when a group of earthquakes was selected for a questionnaire distribution their coordinates were submitted to Toronto; by the time the questionnaires had been returned the computation was usually completed. However, as the computer became busier the





delay was longer; Willmore developed a chart which permitted the computations to be made mechanically, with good speed and adequate accuracy[81].

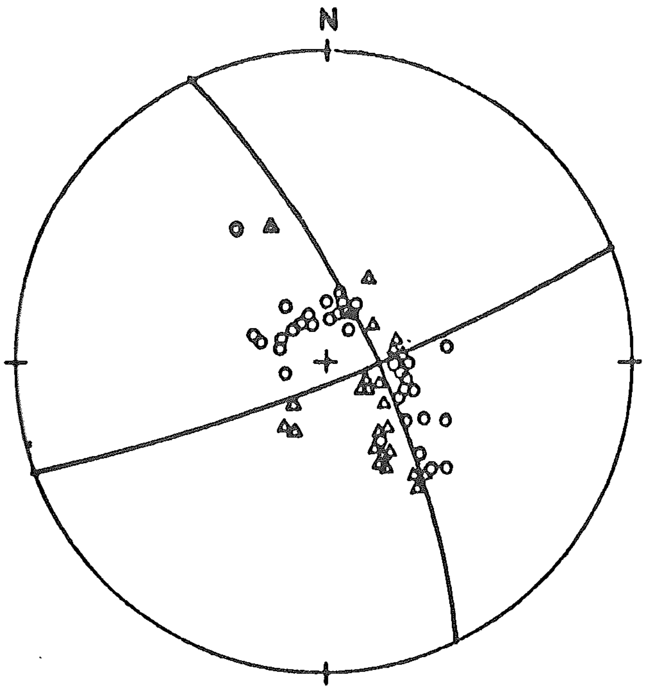
By 1956 sufficient data had accumulated to justify a major summary[82] of the first five years of the project. Seventy-five solutions were available for analysis, the majority from earthquakes around the Pacific rim. The paper first discussed all those solutions that could be compared with observed or inferred faults; in general the strike of the planes was confirmed but the motion was much more strongly transcurrent than had been observed. When all the earthquakes were analyzed, transcurrent motion predominated everywhere, the normal component showed no systematic relationship to region or to focal depth. To overcome the ambiguity about which plane was the fault, Willmore suggested that the line of intersection of the planes, which was an invariant, should be studied. The paper showed that these "null vectors", while steeply dipping, did tend to define a plane parallel to the island arc or mountain range with which the earthquakes were associated. In a companion paper[83], D.B. McIntyre and J.M. Christie equated the null vector to the B kinemetric axis of geology and forecast that it would be of great significance in defining the tectonic forces in an area.



Ottawa was not alone in pursuing the study of faulting in earthquakes: the Japanese, who had originated the theory of the method, were active; so were the Soviets; and Ritsema, a Dutch seismologist working in Jakarta, had produced solutions for many Indonesian earthquakes. When A.E. Scheidegger joined the Division in 1956 he compiled all these solutions into a single catalogue[84] and showed that transcurrent faulting was indeed predominant. A later paper provided a further summary of Soviet solutions, bringing the catalogue up to the end of 1952[85].

The particular form of stereographic plotting that Byerly had developed and which was used in Ottawa, was not otherwise in general use. Geologists had been trained in the use of a stereographic projection on a tangent plane of the earth, rather than a diametral plane, and this projection was widely used. In his summary paper Scheidegger discussed the various projections[86] which he had unravelled. In retrospect it is unfortunate that we, in Ottawa, did not adopt the standard equal-area projection. Geologists found the Byerly projection a difficult one to visualize. The figure on next page (left column) shows the same fault-plane solution in the two projections.

In defence of the Byerly projection it should be said that it facilitated the trial-and-error graphical separation of compressions from dilatations into four quadrants. The circles of the Byerly projection were easier to draw accurately than the arcs of the equal-area projection. Furthermore, data from distant stations, which were always more numerous, crowded near the centre of the equal-area projection, whereas they were clearly separated in the Byerly projection. Of course when, much later,



solutions were derived entirely by computer, the projection was needed only to illustrate the solution, and the equal-area projection was finally adopted by Ottawa.

While my summary paper[82] was in preparation, a companion study was made on the validity of the techniques of the project[87]. Were data collected by questionnaire reliable, could any judgement be made about the relative reliability of stations, was the use of reflected waves justified? I was joined in this study by W.M. Adams who was completing a doctoral program at St. Louis University on fault-plane work.

In the 75 solutions available for discussion, observations on the direct P wave (P or PKP) were found to be inconsistent 18.3% of the time. Most of the stations were consistent most of the time, several stations were inconsistent about as often as they were consistent, and some stations, after a long history of consistency, would suddenly become inconsistent. It was suspected that their galvanometer connections had become reversed. I wrote to these stations, outlining the evidence and bracketing the date on which the inconsistency began, and in almost all cases my suspicions were confirmed and the galvanometer connections were corrected.

Reflected phases were found to produce random observations. The use of these phases was stopped, but it was concluded that, insofar as the use of direct phases was concerned, the program was consistently sound, and it was continued[88].

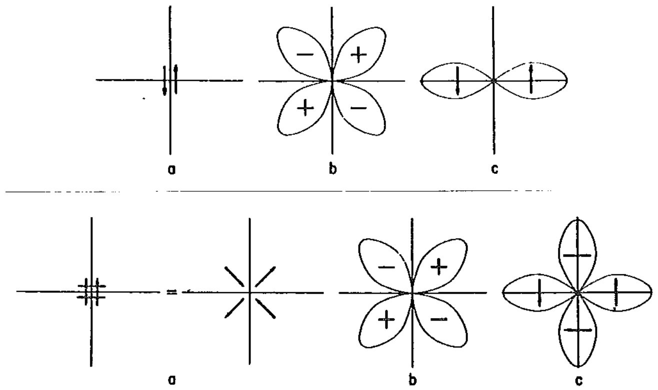
I convened a symposium on earthquake mechanisms at the IUGG meetings in Toronto in 1957 and edited all the papers for publication by the Observatory[89]. There were representatives of all "schools" – American, Dutch, Soviet, Japanese, Canadian – and questions of projections and intercomparison of methods and results were thoroughly explored. There were enough differences in the results of the different groups to suggest caution in accepting solutions; they pointed to the need for more theoretical work on the nature of fault failure[90].

Japanese seismologists had been pioneers in such studies, and H. Honda[91] continued this tradition. He investigated the radiation pattern for P and S waves for the single-couple fault model we have been using. His findings are shown in the upper part of the figure below: the quadrant distribution of P compressions and dilatations is confirmed, the S wave amplitude is zero on the fault plane, a maximum on the auxiliary plane. Use of the S waves should have allowed him to identify the fault plane. However, when he tried, he found that the S waves were a maximum on both planes!

He developed a theoretical source, the double couple, to explain the observed pattern; its radiation patterns are shown in the bottom section of the figure. Again the quadrantal distribution of P compressions and dilatations is found, but the radiation pattern for S is four-lobed, as Honda had observed. Honda showed that the double couple is mathematically equivalent to two sets of forces at right angles, bisecting the angles between the nodal planes. This provided a reasonable geological model; the forces would be the major and minor stresses at the point of failure. The models came to be known as Models I and II. Which was correct?

The interpretation of S amplitudes is not as straightforward as noting whether the initial P motion on a seismogram is "up" or "down", and the Observatory program's dependence on questionnaires made it impossible for us to study this question. It was actively pursued elsewhere and was a major consideration at a second mechanism symposium held in connection with the 1960 IUGG meetings in Helsinki. The proceedings of this symposium, arranged by V.I. Keylis-Borok but chaired, in his absence, by myself, were also published by the Observatory[92].

K. Kasahara, who held a postdoctoral fellowship in the Division in 1960-61, made a study of a Japanese earthquake in which good records of S waves were available. Establishing first the position of the P nodal planes he showed that the S waves supported a double-couple mechanism rather than a fault one[93]. In a much more extended study, making use of the records from the expanding Canadian and WWSSN networks, he examined



Above: Theoretical radiation pattern for (b) P-waves and (c) S-waves from a single couple oriented as in (a). **Below:** Theoretical radiation pattern for (b) P-waves and (c) S-waves from a double couple or equal deviatoric stresses oriented as in (a). After H. Honda, Geophysical Notes, University of Tokyo, 15, 1, 1962.

the frequency spectrum of the P waves of many earthquakes. He found that earthquakes deeper than about 40 km[94] showed much more impulsive spectra than shallower ones.

A.E. Stevens began intensive work with S waves at about the same time as Kasahara, and we shall return to her work presently. We must first however turn our attention to the increasing use of computers in the fault-plane work.

The first approach, by Kasahara[95], was based on work by Knopoff presented in the 1960 symposium[92]. Knopoff there presented a program in which he assigned to each station a probability that it would report a correct first motion in the presence of background noise; he summed these into a probability function which had to be maximized by the appropriate position of the nodal planes. Kasahara modified the method in two ways, first by avoiding the use of a planar projection, which might exaggerate the influence of certain stations, second by assigning weights to stations on the basis of the Ottawa experience. In several test examples he improved somewhat on the graphical solutions but demonstrated that no magic could overcome a lack of stations in appropriate azimuths.

A.J. Wickens was also working on the problem, along different lines. In his program, a pair of orthogonal planes was allowed to assume a systematic sequence of positions, and the best position was selected on the basis of the minimum number of inconsistencies. The program was tested against 138 published solutions; it was found that the computer produced as good or better solutions than the graphical methods 97% of the time but that the graphical solutions were sustained only 72% of the time. It was also shown that only 39% of the solutions were closely defined by the data.

These findings were presented to the IUGG meeting in Berkeley in 1963[96] and to the IGY Symposium which preceded it[97]. These and several other papers presented at the meetings cast so much doubt on the validity and closeness of the earlier solutions that all present agreed that some means must be found to express the accuracy of published solutions. To meet this need, Wickens devised a micro-search program around the best 16 solutions found in his initial search. The best of the final solutions were selected for publication with a number of measures of reliability, to be discussed later.

In the meantime his original program was used to analyse the accumulated data on large earthquakes[98], and a search was instituted to collect all the published data, to be subjected to a definitive test. All known fault-plane solutions were examined and the data on which each was based were converted to punch cards. Where two or more authors had considered the same earthquake each set of data was compiled separately and a combined set was also produced. In addition to these earthquakes, observations for a number of European shocks were supplied by the European Seismological Commission. Data were available for a total of 618 earthquakes.

It was a massive program, and arrangements were made to run it on the IBM 7090 at the University of Toronto. It was necessary to wait for several weeks for our turn. It finally came, Wickens travelled to Toronto with a car full of punched cards, and was set up, ready to go. At that very moment the power went off, all over eastern North America, the largest and longest blackout in history! Our turn was gone. Wickens returned to Ottawa, rewrote the program to run on the Departmental CDC 3200 and we nursed it through a succession of all-night sessions.

The results were published in a dedicated volume of the Publications[99]. For each earthquake the data set of each author was considered, and then, for the best solution, re-considered with the inconsistent observations discarded; the process was repeated for the combined data. The print-out, considering the single couple model, treated each nodal plane in turn as the fault plane and computed the component of motion in the strike and dip direction and the sense of this motion. Considering the double couple model it gave the orientation of the pressure, tension and null directions. Finally the closeness of fit was indicated by a listing of the rotation around each axis permitted by the data and by a "score", expressed as a percentage, which was a weighted sum of the correct and incorrect observations.

Of the 618 earthquakes considered, 70 provided solutions in which less than 10° of freedom were permitted in these rotations. For these earthquakes the data and the Byerly plot were given. The paper concluded with a complete listing of the program.

These 70 "well-defined" solutions were examined further by Stevens and Hodgson[100], who concluded that even they could not be considered completely reliable. The solutions satisfied the data but the observations limiting them were few. To be reliable they would have to be supported by a good distribution of observations in all azimuths and with a reasonable spread of distance. Under these criteria, only 10 earthquakes survived as "well-defined"!

In retrospect it is questionable whether the vast amount of work involved in this re-analysis of old data, was justified, except insofar as it established Wickens' program. By the time the project was completed the WWSSN and Canadian standard networks were in operation, copies of all records from both of them were available on microfilm, and the production of reliable mechanism solutions became a routine part of all earthquake studies.

This work marked the end of the fault-plane project which had been a major preoccupation of the Division for some fifteen years. It seems reasonable to claim for the Observatory a leading position in establishing the importance of the method. The computation of the tables of extended distance, the laborious collection of data, the plotting of the data and the trial-and-error search for the best solution, were tedious and extremely time-consuming, but they proved that the method worked, and no one else applied it on such a worldwide basis. By the mid-1960s, when the WWSSN and Canadian standard networks provided reliable and numerous data, and powerful computers were available to analyze them, the potential of the method had been established and the production of mechanism solutions became routine.

The computer program was turned over to the International Seismological Centre, which published routine P nodal solutions for all large earthquakes for a number of years. It was gradually concluded however that reliable solutions could only be produced by the individual researcher reading the original seismograms in a consistent manner; this publication was stopped.

One of the criticisms of the Observatory work, and indeed of all early work, was that it showed an unacceptable degree of strike-slip motion. This was because the nodal planes were accepted as fault and auxiliary planes, following the single couple model. They might equally well have been interpreted as indicating the directions of pressure and tension. In all of the latter summary papers this was done. The pressure and tension axes showed consistent patterns, with tectonic significance.

The question of model, of course, depended on the use of S. I made an attempt to study this question during a sabbatical year in Paris, working with the collected original records of the 1946 Vancouver Island and 1949 Queen Charlotte earthquakes. I was not able to reach any final solutions but it is gratifying that my notes were used, many years later, to refine the mechanism solution for both shocks[101].

We have discussed above Kasahara's contribution to the use of S waves. A.E. Stevens was also active in the work, adopting it as a thesis problem for her PhD work at the University of Western Ontario. Assuming a point source system with components along three axes, she developed a computer program that defined the position of this force system from S data alone in the two special cases of a single and a double couple. Testing the method on a number of earthquakes where the S-data had been published she was able to forecast the position of the two P-nodal planes under each hypotheses. By comparison with the observed P data, she was able to select the correct model. The "double-couple" solutions outnumbered the "single-couple" by about two-to-one; which of course had been known by the original investigators; Steven's method was established[102].

She applied the method to an earthquake on the Hindu Kush[103] for which the P-nodal solution had already been published and found that only the double-couple model was consistent with the P observations. It was also consistent with surface-wave data[104]. She extended the study to consider a general Hindu-Kush earthquake mechanism, a matter of much interest since these shocks constitute an anomalous group of intermediate-focus earthquakes.

In a third, major, paper deriving from her thesis[105], she reconsidered 28 earthquakes for which data and solutions had been published by other investigators. She was able to add considerable data by reference to the ISS, which, being published after considerable delay, presented data not available to the original authors. Of the 28 earthquakes, from several parts of the world, only one was definitely due to a single couple mechanism; it had a shallow focus. The remaining earthquakes were all deeper than normal and all, with one possible exception, were of the double-couple type.

Additional solutions using S waves are included in a review paper published by Stevens in 1969[106].

A symposium on "Processes in the Focal Region" was held at the 1967 Zurich meetings of the IUGG, with K. Kasahara as convener. This symposium was not intended to deal with the so-called "fault-plane" studies, as the two earlier symposia had, but rather with processes within the focal region itself. The papers presented were published in the Observatory Publications[107], edited by Stevens and Kasahara.

THE DETECTION OF NUCLEAR EXPLOSIONS

As we saw in an earlier section, the need for the nuclear powers to find some way to monitor a Test Ban Treaty led to an explosive expansion in the support of seismology. The development of the Canadian seismological network from a few inadequate stations in the southern populated fringe to a first-class network covering the entire country was one result. Our geographic position between the test areas of the United States and the Soviet Union led to the temporary establishment, by the United States Air Force, of a number of Long Range Seismic Measurement (LRSM) stations. These were stations and arrays used to test the possibility of long range detection of nuclear explosions. It was one of these stations that assisted with the study of the earthquake swarm at Mould Bay (page 187).

There were, in addition, two important developments, which we shall now consider.

The Arctic Institute Project

Early in 1962 Dr. John Reid, Executive Director of the Arctic Institute of North America, suggested that the Division should join it in a study of the relative efficiencies of the Canadian seismograph stations. The research was related to the problem of nuclear detection; if one knew why one station was more effective than another, better stations could be deployed. We should therefore apply for a grant from the United States Air Force Office of Scientific Research (AFOSR). To overcome the problem of a Canadian government agency accepting money from an American one, the grant would be to the Institute, which would then meet the expenses for staff and material. The application was made and accepted, and work began in September 1962.

M. Ichikawa was recruited from the Japanese Meteorological Agency, and P.W. Basham moved to the project upon the completion of his short-term contract as station operator at Mould Bay. They approached the problem in a number of ways.

Microseisms

The ability of stations to detect earthquakes is obviously related to their average microseismic level. To investigate the importance of this factor Ichikawa and Basham read the amplitude and period of microseisms on the north-south records, both long and short period, for eight standard stations. Readings were made on alternate days at 00 and 12 hours UT, and also at the time of maximum amplitude. Basham and Whitham[108] analyzed these data, producing diagrams of the monthly mean amplitudes of both short- and long-period microseisms and of the Fourier amplitude spectra of typical long-period summer and winter microseisms for each station. How would these microseismic levels affect the ability of each station to detect earthquakes? For teleseisms the signal would be the initial P wave, for local earthquakes the maximum in the S group. Assuming that the signal-to-noise ratio would have to be greater than one, they produced curves for each station relating the minimum magnitude detectable and epicentral distance for both distant and local earthquakes. They checked these findings against data submitted by the station operators. It was interesting to find that small local earthquakes in southern Canada might be more effectively detected as teleseisms by the Arctic stations than as locals by the southern stations.

Professor R.D. Russell, of the University of British Columbia, joined the project during the summer of 1964. He and F. Kollar made a detailed study of the spectrum of microseisms at Ottawa. Using an FM tape recording system and a Willmore seismometer, both of which were exhaustively calibrated, they recorded the microseisms over one-hour periods in July and in November and made a spectral analysis of the tape with an IBM 7040 computer. They found that in both cases the peak amplitude occurred at a period of about 5.2 seconds, but that the amplitude of the November microseisms was about six times that of the July ones[109]. Because it was necessary to calibrate the seismometer precisely, an analysis of calibration techniques was made[110].

Body-Wave Magnitudes

Ichikawa and Basham next investigated variations in body wave magnitudes. Amplitudes and periods of the initial P wave for some 1000 earthquakes were measured on the short-period vertical records of 10 of the Canadian standard stations[111]. For each observation the body-wave magnitude was computed and compared with those published by the USC&GS.

They found that the average Canadian magnitude was about one-quarter unit larger than the USC&GS one. Comparison of the residuals obtained at Coppermine between the American and Canadian standard instruments (see page 182) suggested that this might be the result of different calibration techniques. For each station the residuals were dependent on the azimuth of the earthquake zone; for each earthquake zone they were dependent on the stations. This suggested the influence of ground conditions at the stations and possibly of the earthquake mechanism.

They investigated the influence of mechanism and concluded that, in general, its effect was negligible, although it might become significant at a small group of stations if the nodal planes from a particular group of earthquakes lay near those stations.

Signal-to-Noise Ratios and Recording Limits

The background noise level was measured at the time of each of the P wave recordings used in the magnitude residual study. Curves of signal-to-noise ratio vs distance were then drawn, showing for each station an upper and lower recording limit; no earthquake would be recorded above the upper curve, all would be recorded below the lower limit. The difference in recording ability became evident from these curves; there was a factor of thirty difference between the best station, Mould Bay, and the poorest, Halifax. Mould Bay had long been recognized as the best Canadian station; this is obviously due to its exceptionally low noise level rather than any crustal factor.

Amplitude Spectra

It had been known for some time that the amplitude spectra of long-period P waves could be used to deduce the large-scale features of the crust and upper mantle. Could short-period P waves be used to show the finer-scale features; conversely, could the fine structure of the crust explain recording anomalies at the various stations? The P wave-train was digitized for a large number of records and amplitude spectra were computed for the various stations. Theoretical curves, based on assumed crustal structures under the stations, were produced for comparison. The correlation was not good. Two explanations suggested themselves: it was difficult to accurately digitize the short-period paper records of the standard stations and the crustal structures under the various stations were not precisely known.

T. Utsu succeeded Ichikawa on the project in 1963 and he continued the spectral studies, but extended them to include long-period seismograms. He limited his study to four Arctic stations – Mould Bay, Resolute, Alert and Coppermine – and studied their records for 33 shallow-focus earthquakes originating in Alaska and the Aleutians. Normalized amplitude spectra were obtained for each station for all three components for both short- and long-period records. Various crustal models were then tested as a means of explaining the observed curves. The fit was better than in the earlier work, particularly with the long-period records, and he joined Ichikawa and Basham in recommending a controlled experiment over a known structure[112].

The Controlled Experiment

The field work was carried out during the summer of 1965, by Basham and Professor R.M. Ellis, of the University of British Columbia; Ellis spent the winter months in Ottawa, reducing the observations, and Basham joined him at the University in September to complete the reduction as an MaSc thesis. A.M. Bancroft assisted with the field work.

Central Alberta was chosen for the experiment. Because of the intensive oil exploration in the area the sedimentary column under each station was precisely known; the crustal column was also known through the work of the University of Alberta crustal group.

An FM tape-recorder, developed by Bancroft and Basham[113], recorded seven tracks; three short-period Willmore components, at two levels of gain, recorded on six of these traces, the seventh provided a timing trace. One of these instruments was operated at Leduc in the standard vault of the University of Alberta; the other recorded, in succession, for about two months at three different locations in central Alberta, with thickening sedimentary sequences. Thirty-four teleseismic events, from a variety of distances, were recorded.

It was thus possible to construct spectral curves for various angles of incidence for each station and to compare these with the observed spectra. The comparison[114] was not as satisfactory as had been hoped. This was ascribed to short-period scattering in the crust and upper mantle, and to possible P to S conversion close to the base of the sediments. There was also evidence of possible dipping, not previously recognized, of the M-discontinuity.

If the part of the P-wave train immediately following onset is difficult to explain, what about the long following section, the so-called coda? Basham and Ellis considered this in a second paper[115]. The FM tapes were digitized and analyzed with the help of filters; they were able to identify the core phases PcP and PKP where the epicentral distances were appropriate, and the depth-of-focus phases pP and sP. They also confirmed the suspected P to S conversion at the base of the sedimentary layers, which had so confused the spectral studies.

There were two other areas in which recent refraction studies had provided detailed information about the structure of the crust and upper mantle. The experiment was repeated there, in the vicinity of Yellowknife in 1966 and on the Grenville Front, near Schefferville, in 1968. The data were analyzed by Hasegawa[116]; in both cases the agreement between computed and observed spectra was poor. The use of the spectra of short-period P phases to define crustal structure seemed not to be satisfactory.

Later Hasegawa reviewed the theory of the method[117], and re-examined the Yellowknife recordings, in an attempt to refine the technique. This effort was more successful, but the paper falls beyond the period we are reviewing.

The Vela Uniform/AINA project ended in August, 1967; Basham transferred to the Observatory staff.

The Yellowknife Array

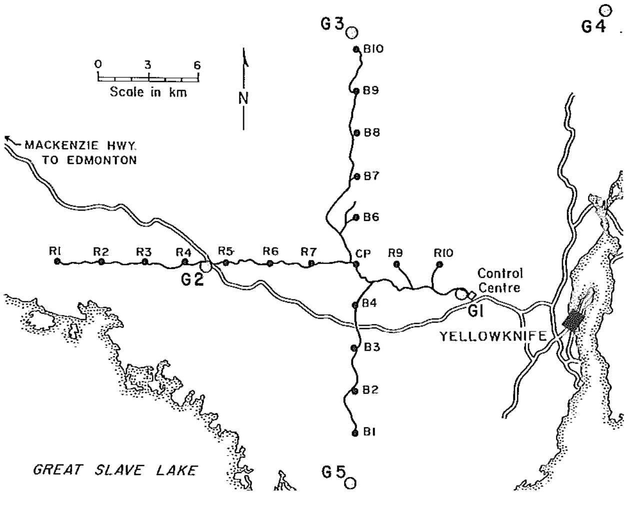
In April 1962, the British Ministry of Defence approached the Canadian Defence Research Board about the possibility of locating a large seismic array in Canada. The purpose of the program was to seek some way of distinguishing earthquakes from nuclear explosions, in order to monitor a proposed test-ban treaty. Since Canadian competence in seismology lay with the Department of Mines and Technical Surveys, the Board arranged a meeting of all interested parties under the chairmanship of W.E. van Steenburgh, Director-General of Scientific Services.

We must have some understanding of what an array is, and does. The figure (right column) shows the plan of the Yellowknife array as it eventually evolved, an arrangement of seismographs at equal distances along two lines at right angles, in this case north-south and east-west. If an earthquake were to occur due north of the array, the longitudinal wave would reach the stations on the north-south line at progressively later times, which would be a function of the vertical angle of approach and, therefore, of the epicentral distance; it would reach all the stations of the east-west line at the same time. If the epicentre were due west of the array the situation would be reversed. If, in the general case, the ray approached from some intermediate direction, the arrival times along the two arms would be a function of both the azimuth and the epicentral distance of the earthquake. When the outputs of the stations are recorded on magnetic tape the location of the earthquake may thus be approximately determined; there are other analyses that may be made of them, as we shall see.

The British had operated arrays at Eskdalemuir, Scotland, and Pole Mountain, Wyoming, and had found the method promising. They wanted arrays in other parts of the world, particularly in Australia, India and Canada, and they wanted them as soon as possible because of the imperatives of the test-ban negotiations. They proposed that Canada carry out, and pay for, the physical construction of the array, that Britain supply and install the equipment, that Canada be responsible for operating the array and Britain for interpreting the data.

The Defence Research Board and External Affairs representatives urgently recommended that the British proposal be accepted. Board members promised that military assistance would be given in transportation and logistics; the full cooperation of the Department of Public Works (DPW) was promised. I was asked if we could fulfil our part of the bargain. "Not without three more man-years", said I, off the top of my head; van Steenburgh turned to the Chief of Personnel and said "find them". We were on our way.

Immediately on leaving van Steenburgh's office I phoned E.B. Manchee in Calgary, offering him the job of managing the project. He was working for an oil company but had expressed an interest in joining the Observatory staff. He accepted my offer, which was regularized later by the Public Service Commission, sold his house, moved his family to Ottawa, bought a new house, and was in the field searching for a site for the array within two weeks. The following description is taken from his account[118].



The selected site had to be seismically quiet, which suggested a remote, mid-continent location, but with reasonable transportation facilities. Manchee concentrated his testing in the vicinity of Yellowknife and, after extensive noise tests, located the array west of the town, with its control centre near the airport. The noise level was about 1/10 that at Eskdalemuir.

Once the general position of the array had been decided, north-south and east-west lines were cut and surveyed by the Department of Public Works and the locations of the seismometers were staked out. An Army survey team made a precise survey of the array, tying it to Geodetic Survey monuments in the neighbourhood. This work was completed by early November 1962, and 13 of the 20 seismic vaults, or pits, were constructed before the end of the year. These pits, blasted out of the rock, were about 3 feet deep and 5 feet in diameter. A steel cylinder, 40 inches in diameter and 20 inches deep, with a bolted steel lid, was set in concrete in this excavation. An army helicopter and two Department of Transport tracked vehicles provided transportation during this phase of the work.

A Willmore vertical seismometer was placed in each pit, along with an amplifier and a remote calibration device, and was connected to the Control Centre by cable. This cable carried the seismometer output to the Control Centre, and power and calibration pulses from the Centre to the seismometer station. During the first winter the neoprene covering on the cables was attacked by rodents and, during the next summer, the cables had all to be changed to an armoured type. Even then the rodents could get their tiny teeth through the mesh of the cable, and it became necessary to carry the cable on poles.

In addition to the array seismometers, 24 seismometers were deployed as a cluster, within a circle of 2 km radius. This, when the signals were summed, was intended to provide a high signal-to-noise ratio.

The output of the array was recorded on a 24-track magnetic tape; pick-up heads mounted behind the recording heads allowed the re-recording of any or all of the signals on a second tape deck or on visual recorders. This recording equipment was housed in a pre-fabricated building, flown to Yellowknife by the RCAF and erected by DPW personnel. Later a Canadian standard station was installed adjacent to the Control Centre, where it could be serviced by array personnel.

The installation of this complex within the almost impossible time-frame requested by the British reflected great credit on the coordinator, Manchee, and showed that government agencies can operate efficiently when their senior officers clear away the red tape.

H. Somers joined the project late in 1962 and went immediately to England to work with the British group and to learn the techniques of processing the data. He described the then state of the art in the paper already referenced[118]. At this time the Yellowknife tapes were being sent to England for processing, so that Somers was working with his own data.

We saw above that the azimuth and distance of an event could be determined from the arrival-time differences along the two arms of the array. These differences could be measured in terms of a time delay inserted in each trace, sufficient to bring them into phase. Since the signal was coherent over the dimensions of the array, and the noise incoherent, the sums of the in-phase signals on the two lines showed a much enhanced signal-to-noise ratio. When these two sums were multiplied together to produce what was called a "correlogram", the signal-to-noise ratio was further enhanced. The method of searching for events on the tapes was to sweep all azimuths and to form correlograms for each position; if for a particular azimuth the correlogram was larger than some selected level, the signal was taken to be coming from a real event, explosion or earthquake[119].

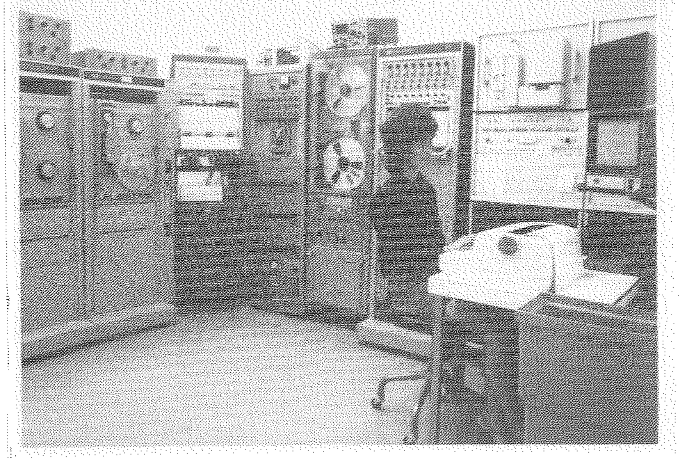
The correlogram also served with some reliability to distinguish between explosions and earthquakes. Because explosions provided a single sharp wave-front their correlograms were usually a simple envelope whereas those for earthquakes were more complex. Unfortunately it didn't work all the time.

After Somers' return to Ottawa, attention was focused on providing a Canadian facility for analyzing the tapes. Provision was made for producing duplicate tapes, one for Britain and one for Canada, and research was conducted into the optimum search mode to be used, and the appropriate programming[120].

In 1965 the Department installed a CDC 3100 computer, which was powerful enough to process the Yellowknife tapes, although it was only available in non-prime time for the Yellowknife analysis. The programming for this new computer was done by D.H. Weichert. For a variety of reasons, of which flexibility was a principal one, it was decided to process the data digitally, and a first step was to convert the raw tapes from analogue to digital form. A search method was devised, again depending on the cross-correlation of the in-phase signal sums on the two array arms. The first successful processing by the new program was achieved on New Year's Eve 1965; then, operating at twice real time the search procedure was tested on a 30-day output of the array. It detected seismic events satisfactorily, and located the corresponding epicentres within 300 km of the corresponding USC&GS locations[121].

The processing of the tapes on the Departmental computer continued throughout 1966. The success led to a decision to install an in-house computer and the requirements for such a computer were studied by Weichert[122]. A PDP-124 (next page, left column) was selected and was in operation by the late spring of 1967.

While this was going on, many modifications were being made to the array itself[123]. The provision of a second tape deck has already been mentioned. Calibration routines on both the seismometers and laboratory equipment were improved. Flooding of the seismometer pits was experienced and overcome. Because of the damage from rodents already mentioned, the cables had been elevated on tripods; they then became susceptible to lightning strikes which became a serious problem. It was decided to telemeter the seismometer output to the Central Control, but this took some three years to accomplish[124], which puts it beyond our period of interest.



The in-house PDP-124 computer, used for the processing of the Yellowknife tapes.

Anomalies in the location of sources suggested that errors might exist in the location of some seismometer pits. A photometric survey was carried out to check this, with the cooperation of the Topographic Survey[125]. Small errors were found; the new survey provided a relative precision in the position of the seismometer pits of about 4 m.

With the array in continuous operation, and the in-house computer available in Ottawa, routine processing of the tapes was established. Programs were developed to increase the speed of search to four times real time and several studies of the effectiveness of the array followed[121,126,127]. Given the distance of the Yellowknife array from possible nuclear explosion sites, attention was focused on "third zone" events, those arriving from distances of 30° to 90°. Manchee and Weichert found that the 50% detection probability of the array for such events was for magnitude 4.0, whereas for the Yellowknife standard station it was 4.4. The accuracy of an event location, when compared with the USC&GS epicentres, was 3 to 4 degrees in distance and 2 to 3 degrees in azimuth[127]. They could find no evidence that these limitations varied with azimuth, which suggested that differences in structure in the vicinity of the array were not to blame. This was confirmed by the study, discussed earlier[53], in which the array recordings of nearby explosions were used to define the crust and upper mantle in the vicinity of the array. In a later paper[128] Weichert sought an explanation in the nature of the search program, and still later[129] showed that, by modifications in the search program, the discrepancies could be substantially reduced. Considering that we are comparing the accuracy of a single facility with that of a world network of stations the errors are not remarkable, but they did preclude the use of the array to pinpoint the source as a basis of on-site inspection.

In one remarkable study it was shown that the system could discriminate between two events, one in China and one in the North Atlantic, the signals from which arrived in Yellowknife within 0.2 s of each other[130].

One of the array stations with its telemetering antenna.

A paper summarizing the geophysical results of the array program was prepared for the 1967 Zurich meeting of IASPEI[131].

F.M. Anglin studied the array capabilities during a period of high instrumental reliability and low noise level in the winter of 1968. During this period, which he regarded as optimum, he found a 50% detectability level at magnitude 3.9, rather than the 4.0 found earlier, and for the first time found some variation of this with geographic areas[132].

A useful review of the use of arrays in general and of the Yellowknife array in particular was published in 1975[133].

In 1968 three vaults were added to the Yellowknife array to house long-period seismometers. They recorded on FM analogue tape and were intended to test the enhancement of surface waves for use in explosion discrimination and to examine the potential of a long-period array.

Due to the Yellowknife project the seismology group acquired its own computer at a time when Departmental policy forbade decentralized computer facilities. The new seismology computer room was christened a "data laboratory", not a computing centre. Projects in seismology unrelated to Yellowknife gradually began to use some of the spare computer capacity. The seismology group thus became computer-literate early in the computer revolution and has remained in the forefront of data processing within the Department, an unexpected but positive side effect of the Yellowknife project.

Contributions to Nuclear Test Ban Seismology

From the time of the 1958 Geneva meetings Canada was interested in the possibility of monitoring a nuclear test-ban treaty. The fact that Willmore attended these meetings and afterwards successfully proposed the Canadian Standard Network has been documented earlier in this chapter. Any international negotiations on the subject were the responsibility of the Department of External Affairs, and Willmore acted as an advisor to them and to the Defence Research Board. When he left, this responsibility was taken, briefly, by me, then by Manchee, subsequently by Whitham, and then by Basham. Progress toward a Nuclear Test Ban treaty was negligible, despite the advances seismologists had made. The negotiators from the nuclear powers each had their own national and personal interests to protect. Could an independent body make more progress? Toward the end of the 1960s the Swedish government set up the International Institute for Peace and Conflict Research, charged with providing this independent outlook. In 1968 it convened a meeting on 'Seismic Methods for Monitoring Underground Explosions'". Whitham presented the Canadian research results. A major report was produced, as a result of which the focus of research began to move away from special projects toward the more effective use of existing stand- ard networks. The methods gaining favour depended upon a comparison of body-wave and surface-wave magnitudes.

The magnitude of an earthquake is a measure of the energy released in the focus as determined by the amplitude of specific waves recorded at seismograph stations. The energy reaching the station may travel as a longitudinal wave through the interior of the earth to provide the first arrival. A magnitude based on this wave is designated m_b; it is reasonably independent of the focal depth of the event. Other energy may reach the station as a surface wave; a magnitude based on this surface wave is called M_S. The energy in surface waves decreases markedly with the depth of the earthquake, becoming negligible for a deep-focus earthquake. Since almost all earthquakes are appreciably deeper than explosions, the ratio of M_S to m_b should be greater for explosions than for earthquakes. This splendid analysis of the situation turned out to be quite wrong; the ratio M_S/m_b is higher for earthquakes than for explosions, and is related to the dimensions of the seismic source. Nevertheless it was hoped that the ratio would provide a reliable discriminant, and Canadian research concentrated on this problem.

The method turned out to have its limitations, principally due to the difficulty of measuring the two magnitudes. Surface-wave magnitudes are based on Rayleigh waves which, as we have seen elsewhere, are dispersive. One must agree on the period at which the measurements are made; furthermore the propagation of Rayleigh waves is affected by the structure, oceanic or continental, through which they are propagated.

Measurements of m_b are not as subject to uncertainties but there were reasons to believe that they could be influenced by the band-pass of the seismograph used in the measurement; this had led to disagreement at the Test Ban negotiations. The USSR used broad-band seismographs in the determination of M_b, the United States short-period seismographs. In an attempt to resolve this dispute Basham[134] made use of records from the Collège Jean-de-Brébeuf station at Montreal which operated vertical seismographs of both short- and intermediate-period. He measured the magnitudes of a large number of earthquakes on the two systems and found that they did in fact differ by about 0.3 magnitude units.

Basham next considered the recordings at Canadian standard stations of 28 explosions from the Nevada Test sites and 28 earthquakes as nearly as possible from the same area. Canadian magnitudes differed from those determined by the USC&GS, but the scatter of the differences was so great that no significance could be attached to it. The threshold at which the M_S/m_b discriminant could be applied was m_b 4.0 for earthquakes and m_b 4.5 for explosions, but this varied with the background noise level and was much poorer when the surface waves had to traverse a continental margin[135]. He extended the study to Asia, examining the Canadian records of 33 earthquakes and 36 underground nuclear explosions. He found that the M_S/m_b discriminant could be applied from $m_b = 4.9$ for earthquakes and 5.9 for explosions. This poorer performance, as compared with North American sources, was partly due to the greater distance of the events but largely to the effect of mixed oceanic and continental paths on the Rayleigh wave amplitudes[136].

On the basis of the magnitudes studied in the two foregoing papers Basham published corrections to be applied to the observed M_S and m_b magnitudes for each Canadian standard station for various earthquake and explosion sites[137].

Hasegawa and Whitham compared the Yellowknife records of Nevada explosions with the record that should, theoretically, have been obtained. There were substantial differences which they ascribed to complex reverberations in the crust at the Nevada Test Site and to signal-generated noise at Yellowknife[138].

An opportunity to check the effectiveness of the array and of the Canadian standard network was afforded by a series of aftershocks that followed the large nuclear explosion "Benham", detonated on December 19, 1968 at the Nevada test site. The explosion had a magnitude of 6.3; the USC&GS listed 45 aftershocks, occurring within the next 41 days. The Canadian facilities detected all shocks with magnitude m_b greater than 3.6 and none with magnitudes less than 3.3; for surface wave magnitudes M_S the lower limit was 4.0. Within these limits the M_S/m_b discriminant was successful in distinguishing the aftershocks from explosions in the same area[139].

In September 1969, a nuclear device was fired in northwestern Colorado to investigate the feasibility of recovering natural gas by fracturing the reservoir medium. It was known as Project Rulison. The explosion registered throughout the Canadian standard network; the arrival times and amplitudes were published[140] and the values of m_b and M_S computed but no major study was undertaken.

In the late 1960's the United States proposed to test nuclear devices so powerful that they could not be detonated at the Nevada Test Site; a new test site was developed in the Aleutians. The seismological community recognized that the proposed explosions provided an opportunity to improve travel-time tables, and many additional seismograph stations were deployed for the purpose. A series of explosions, code-named Longshot, Milrow and Cannikan, followed. The second of these, Milrow, with a TNT equivalent of one megaton, occasioned much alarm among the general public; they feared that it might trigger a major earthquake or set up a tsunami in the Pacific. Neither of these things happened, and the records of the Canadian stations indicated a smaller magnitude than had been expected from extrapolated values of earlier, smaller, explosions[141]. As hoped, the explosions permitted the preparation of new, accurate travel-time curves which became the world standard.

The success of the magnitude ratio method in the Canadian experience led Canada to sponsor a United Nations resolution that a survey be made of the seismic data that would be available on an exchange basis, and of their effectiveness in deciding whether or not a test-ban violation had occurred. It took some time for the member countries to respond but eventually 75 countries, none of them from the Soviet bloc, promised to exchange data on request, and submitted information on the seismic facilities within their borders. These submissions were analyzed by Basham and Whitham in a major paper[142].

They concluded that the detection limit for both earthquakes and explosions, based on P-waves, was m_b 4.2 for North America and Europe, m_b 4.5 for Asia, and m_b 5.0 for most of the southern hemisphere. The detection limit for Rayleigh waves, on which the identification of an explosion would depend, was, for earthquakes, m_b 4.8 in North America and northern Europe, m_b 5.1 for Asia; for explosions the Rayleigh wave limits would be 5.8 and 6.1, respectively.

These results suggested that a test ban would not be as difficult to police as the major powers were suggesting, and it allowed Canada to take a position independent of the major powers in deliberations at the United Nations and in the Geneva Conference on Disarmament. The importance of the work was recognized by a Public Service Award to Basham and Whitham.

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Over the period 1947 to 1970 Observatory seismologists defined seismic risks in Canada, studied crustal and upper mantle structure under Canada, took a leading roll in the study of the focal mechanism of earthquakes, studied seismic methods of nuclear detection and discrimination with implications for international peace - problems ranging from our own backyard to the world stage. Technical staff, working in close collaboration with researchers constantly maintained and improved instrumentation and computing facilities to make this possible.

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XIV – HEAT FLOW

The earth's internal heat supplies the energy to drive the tectonic processes. The results are easily observed, but the source is difficult to study directly. One way to do so is to measure the heat escaping from the earth by conduction through a unit area in unit time. It is determined at any point by measuring the temperature gradient and rock conductivity and applying corrections for a variety of surface conditions. Heat flow measurements in Canada were largely a university interest before the Upper Mantle Project. The Universities of Toronto and of Western Ontario were particularly active.

In 1948, when P.C. Bremner visited Resolute Bay to explore the possibility of establishing a seismograph station, he was expecting to drill holes in the overburden to investigate the depth and nature of the bedrock. Andrew Thomson, Director of the Meteorological Service of Canada, suggested that temperature measuring elements be placed in the holes to determine temperature fluctuations within the soil. This was done, using instruments supplied by the Department of Transport. The measurements showed that, at a depth of 8 inches, the soil temperatures followed the daily and seasonal variations of the air temperature with little lag; at a depth of 60 inches the daily fluctuations had disappeared and the seasonal variations showed a delay of from one to two months[1].

From this small beginning there developed a major program in heat flow determination. During the next four summers Bremner undertook an extended drilling program at Resolute, which was made very difficult by the presence of permafrost. The problems were overcome and, in the end, temperature measuring elements were placed at five foot intervals to a depth of 70 feet, at ten foot intervals from there to 100 feet and at fifty foot intervals from 100 to 650 feet[2]. Cores, suitable for conductivity measurements, were taken at intervals of fifty feet, and a mean value of heat flow was determined. It proved to be about twice as high as the normally accepted value[3]. As we shall see later, this was due to the proximity of the ocean; correction reduced the value to the world average.

While the seismic operator at Resolute continued to monitor the temperatures, there was no further Observatory involvement in heat flow measurements for several years. In the meantime however the work expanded in the Universities, and they began to lobby for a program within the Department of Mines and Technical Surveys to give guidance and coordination to their work. The Director of the Geological Survey agreed that the group should be within the Dominion Observatory, since the discipline had a physics rather than geological background. Agreed, but how was it to be funded?

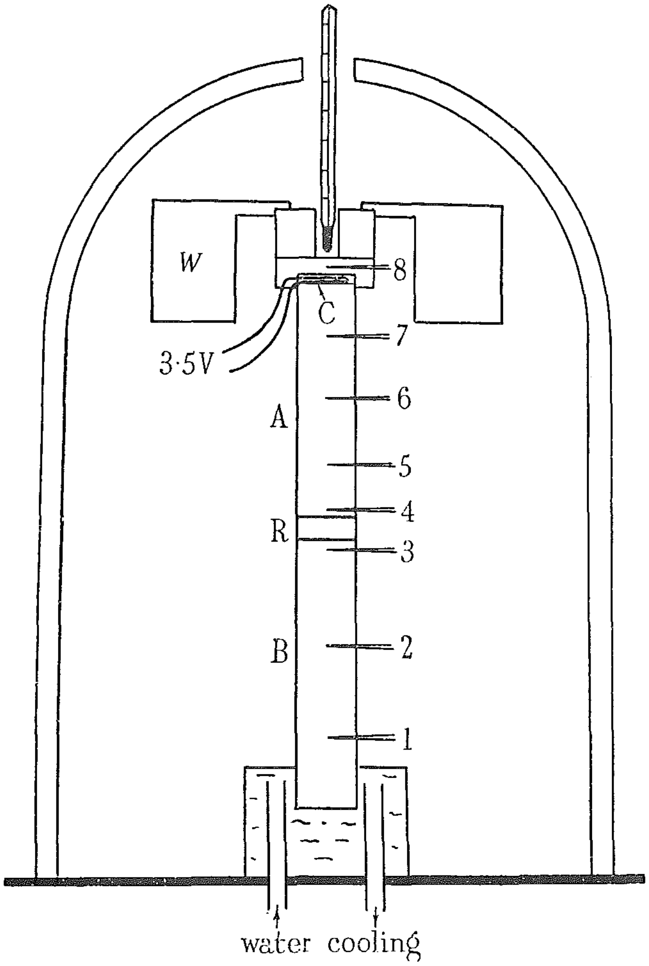
At about this time the Upper Mantle Project was adopted by the IUGG, and the Department invited submissions about work that should be done. A small heat flow section was proposed in the Departmental submission[4]. It called for a group of three people – a scientific officer and two technical officers – the expenditure of $50,000 for laboratory and field equipment, and an annual budget for field work of $10,000. It recommended that holes be drilled at each of the 25 seismograph stations currently under development, with temperature-measuring devices in the holes and recorders in the seismograph stations. This would give a country-wide distribution of heat-flow observations obtained in non-anomalous areas. It recognized however that the section head, when recruited, might have other ideas, as indeed he did. The proposal was accepted; the Group was attached to the Seismology Division.

Dr. A.M. Jessop, a post-doctoral fellow at the University of Western Ontario, was recruited into the position as soon as it was established and took up his duties in March 1962. He was joined soon afterwards by Roger Blais, who left after two years and was replaced in late 1964 by T.J. Lewis. Instead of the second Technical Officer originally proposed, Jessop opted for a Technician but because of the staffing freeze imposed by the Diefenbaker government this position was not filled until early in 1965 when J.G. Bisson joined the staff. Jessop was working by himself through much of 1964.

Initially, Jessop borrowed equipment from the University of Western Ontario, but his primary concern was to develop his own. For field work, cables of suitable length, fitted with thermistors and mounted on winches, were a first requirement. Because the currents developed by the thermistors were small, it was important that the resistance between the individual conductors of the cables and between the cable and ground should be very high, and should be unchanged throughout the experiment. Early experience with two-conductor cables suggested that this condition was not being met. Four-conductor cables were then adopted. One pair of conductors were used for the measurements, the second pair carried power to control a switch on the measuring conductors. This switch could be set in three different positions: in the first position it shorted the two conductors, permitting a measurement of line resistance; in the second position it left the two conductors open and separate from the thermistor to permit a measure of resistance between the wires, and between each wire and ground; in the third position it connected the cable to the thermistor. Measurements of the resistances were made at frequent intervals during a run[5].

Laboratory equipment consisted of machinery to cut and polish cores and an apparatus to measure their thermal conductivity. Cores were usually obtained from the drill holes but where this was impossible they were cut from a large piece of rock as nearly as possible approximating the drilled formation. However the cores were obtained, disks of standard thickness had to be cut from them, and the faces of these disks had to be parallel to each other and flat within 0.02 mm.

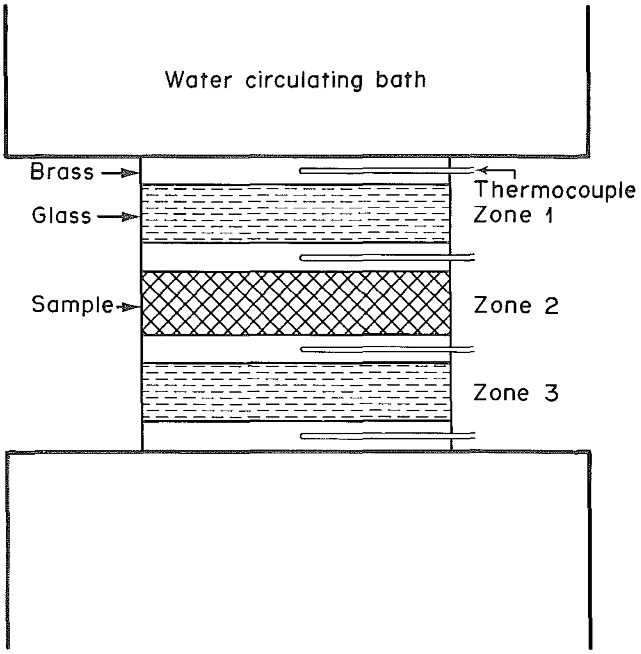
Measurements of conductivity were made in a "divided bar" apparatus, an early version of which is shown in the figure on the next page. It consists of two brass rods, A and B, between which the rock sample, R, is inserted. Heat enters the system through the small heating coil C, passes down



through bar A, sample R and bar B, and is carried away by the water cooling at the base. Thermocouples, indicated by the numbers 1 through 7, measure the temperature at several points in the brass rods. Once a steady state has been reached, the product of temperature gradient and conductivity is a constant for any part of the system. By measuring the gradients across the sample and in the rods, and by assuming the value of conductivity in brass, the conductivity in the rock sample can be calculated.

This early model of the divided bar apparatus was not very satisfactory for measuring the conductivity of rock samples; it was known that the conductivity of the bars and of the sample should be of the same order. The Ottawa apparatus took account of this. The brass bars were replaced by disks consisting of glass sandwiched between two layers of brass. By judicious selection of thicknesses the condition of equal conductivity could be met. The figure in the next column shows the arrangement[6].

Another source of error in the conductivity measurements arises when the core diameter differs from that of the bar. Since cores of many different sizes were expected, bars of equivalent sizes – 7/8", 1 1/8", 1 1/4", 1 3/8", 1 5/8", 1 7/8" and 2 3/8" – were provided. This new equipment was available late in 1965, at which time the large numbers of core samples which had accumulated were measured or re-measured in the new apparatus.



The original proposal that holes be drilled at each of the seismograph stations, was somewhat expanded to include holes at cooperating universities. In 1962 a hole was drilled in Ottawa, on the Observatory grounds, to a depth of 602 m; this hole provided a laboratory for testing the equipment being developed. The following year holes were completed at Halifax, London and Penticton, to depths of 366 m, 594 m and 611 m respectively. In 1964 the program was continued with holes drilled at St. Jerome, 760 m, Roberval, 611 m, and Winnipeg, 610 m. The holes at London, Halifax, Winnipeg and St. Jerome were drilled in cooperation with the Universities of Western Ontario, Dalhousie, Manitoba and McGill. By this time it was realized that the program would provide heat flow determinations too widely dispersed to permit anything but the most general interpretation and this method of selecting sites for drill holes was abandoned.

From the beginning an attempt was made to use existing drill holes, on the surface or underground in mines. It was hoped that the holes being drilled in the meteorite crater program could be used for heat flow measurements. A hole at Franktown, Ontario, which had been drilled to a depth of 318 m was logged early in 1962, shortly after drilling was completed, but later attempts were frustrated, the hole being blocked at a depth of 130 m. Another "crater" hole at Neilson Island, near Great Whale River, drilled in August and September 1962, was temperature logged in August, 1965 and provided a good heat flow value. Neither the Franktown or Great Whale holes showed any evidence of meteorite impact.

In addition to the Observatory's own drilling program, holes were made available by a number of other organizations. By 1965 thirteen sites in six provinces were available for logging. This was fortunate; 1965 was the first year that the Heat Flow group was at full strength, and it also happened that no money was available for drilling. The group could concentrate on the logging program. The results of this season's work are given in a number of papers[7].

Mines appear to offer many advantages for heat flow measurements. By working in boreholes drilled from existing workings deep in the mine one might expect to get away from the influence of surface temperatures and to obtain measurements deep within the earth without excessive drilling. The first experiment of this sort was made by Jessop in 1963 in the Bralorne Mine in the Coast Range of British Columbia. The high temperatures and moisture at depth in the mine made working conditions very difficult, but usable measurements were obtained at six different levels to a depth of 1500 feet[8].

In 1966 Lewis made a successful survey in a uranium mine at Eldorado, Saskatchewan. Measurements were made in boreholes drilled at right-angles to the drifts at six different levels down to 1060 m. The measurements were reliable; a more difficult problem was to correct for the contribution of the uranium in the mine[9].

The year 1964 saw the beginning of a second Heat Flow group within the Polar Continental Shelf Project. The details of this have been given in the chapter on Geomagnetism. Magnetograms from the newly-established station at Mould Bay showed that the fluctuations of the vertical field were seriously attenuated. This could best be explained by postulating a layer of high conductivity, and presumably of high temperature, at the base of the crust. During the next two years sea-bottom measurements of heat flow were made, using a probe developed for the purpose. The work was successful, although no evidence was found of the high temperature layer.

It was decided that the probe survey should be extended to other parts of the Arctic. A.S. Judge was recruited, in 1967, to develop this work, initially in a Polar Shelf position. He later transferred to the Observatory staff. During the next few years a large number of measurements were made in the vicinity of Mould Bay and of Alert, and in Baffin Bay.

While many holes were drilled in Canada each year by petroleum or mining companies, they were usually completed and brought to production, or abandoned, before the Heat Flow group found out about them. Beginning about 1965, a greater emphasis was placed on learning about them before drilling commenced. Mining and petroleum organizations and drilling companies were asked to cooperate in the program. The success was limited since companies were usually unwilling to reveal their exploration strategy. However, the cooperation of the Mines Branches in the Provinces and of the Department of Indian Affairs and Northern Development, which licensed drilling in the Arctic, greatly improved the coordination. The 1970 Canadian Geophysical Bulletin reports:

> "Several new boreholes, originally drilled for industrial purposes, have been preserved and measured, mainly in the Cordillera, the Arctic, and Northern Ontario. In particular, two northern oil exploration wild-cats have been preserved, one at Horton River on the Bathurst Peninsula ... and one on Ellef Ringnes Island"[10].

Judge[11] has described the problem of using holes drilled for petroleum exploration for heat flow measurements. There are strict regulations about the abandonment procedure for such holes – they must be plugged to the surface to prevent contamination of surface or ground water. Since several months are required for a hole to return to temperature equilibrium, logging could not proceed immediately after the drilling was finished. It was necessary to freeze a multi-thermistor cable into the hole, which frequently resulted in damage to the cable. Despite these difficulties several holes were measured.

The difficulties were not only technical. Because the sites had been abandoned, temporary airstrips and roads had deteriorated badly, particularly in permafrost areas. There were no vehicles to transport the heavy reels of cables, nor winches to hoist them into place. The pursuit of science gave way to heavy labour.

Great caution must be taken in interpreting heat flow values to correct for surface influences. One important correction is for mean surface temperature. Daily fluctuations of temperature can penetrate to a depth of a few centimetres, seasonal ones to as much as a metre. These fluctuations have no effect on measurements made at depth, but the mean surface temperature, which prevails for long periods of time, does influence temperatures to the depths at which measurements are normally made. For example, the mean temperature of the sea floor is higher than that of land, and in a hole drilled near the sea or a deep lake this must be taken into account. The very high heat flow value found at Resolute in 1955 was reduced to the average world value when correction was made for this factor[12]. The influence of topography must also be considered. If a hole is drilled in a valley, the observed heat flow is too high because the escaping heat tends to avoid the extra insulation provided by the hills.

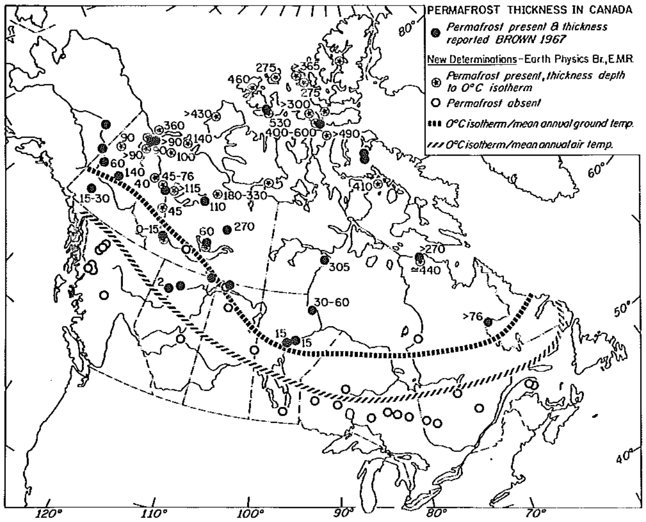
Long-term changes in surface temperatures can influence the temperature gradients to depths comparable with those of the standard measurements. The effects of Pleistocene glaciation are very important, particularly in Canada. During glaciation the surface of the earth was not subject to the mean air temperature but to the ice temperature, which is a function of the thickness of the ice. The duration of glaciation, the times of its beginning and end, are also important factors. Fortunately the extent and variation of glaciation in Canada is fairly well known and reasonable corrections can be made. Jessop has discussed the problem and provided a contour map of the corrections[13].

Another correction, the importance of which was increasingly recognized, was for heat generated locally by radioactive substances. In 1970 Lewis[14] developed a gamma-ray spectrometer to measure the radioactive heat generation in the drill cores, and this correction soon became routine.

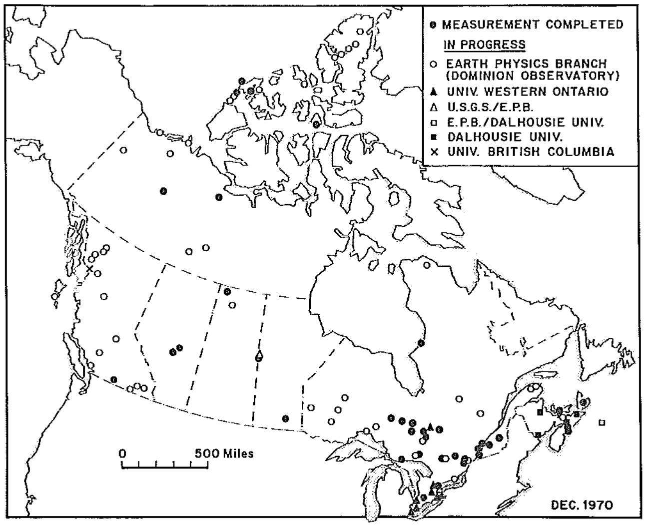
By 1970 the Heat Flow group was firmly established. Laboratory and field equipment had been perfected, field techniques had been developed, the various corrections to be applied to observations were understood. Heat flow had been measured at a large number of widely dispersed points, so that the general pattern was defined, but this broad dispersion of heat flow values made their correlation with other geophysical or geological phenomena difficult. It was realized that to study the complex relation of heat flow to crustal structure, the spacing of the heat flow observations on a horizontal scale must be comparable with the depth of the crust or the dimensions of the individual geological bodies. The emphasis of the Observatory's drilling program was changed from the original concept of widely spaced holes, to the placement of sets of holes across interesting geological features. As the decade ended, the group was poised for much more detailed and sophisticated studies, most of which, unfortunately, fall outside our period of interest. We may however discuss some that were begun before its close.

With the advice and cooperation of J.G. Souther of the Survey's Vancouver office, the first set of such holes was drilled in 1966 in a geologically complex area in northern British Columbia, centred on Mount Edziza and the associated Stikine Volcanic belt. The area had been subjected to repeated episodes of magmatic activity including a major one in the late Tertiary. Could the thermal effects of this be detected? By themselves the three holes could not answer the question, but over the course of the next several years four additional holes became available through commercial drilling, and heat flow was measured at each site. The observations were corrected for the radioactive heat generated by the rocks themselves. These correction were complex, varying from one hole to another, but when they had been made it was possible to determine the temperature and heat flow at the base of the crust[15].

The gravity map of Canada shows a positive anomaly some 1000 km long, reaching from the shores of James Bay south through Kapuskasing. Innes attributed this "Kapuskasing high" to a tensional fracture zone, extending to great depth in the crust, along which basic magma has risen to the surface. In 1968 holes were drilled at Cochrane, Kapuskasing and Hearst to provide a traverse across the feature. Cermak and Jessop[16] analyzed the data. Corrections for the heat generated were complicated by the extreme geological complexity of the crust, and the effects of glaciation were difficult to evaluate, but they were able to infer a temperature profile for the crust, and to conclude that the heat flow pattern was consistent with the gravity interpretation.



Permafrost thickness in Canada and the boundaries of the 0° isotherms for mean ground and air temperatures. After Judge, Proceedings of a Symposium on the Geology of the Canadian Arctic, 307, 1973.



SITE OF HEAT FLOW MEASUREMENTS IN CANADA

Heat flow determinations in Canada to the end of 1970. From Canadian Geotechnical Journal, 10, 7, 1973.

Despite the complications in evaluating the effects of glaciation, Cermak, who was a postdoctoral fellow with the Heat Flow group in 1968-1971, found that in two of the holes the temperature gradient showed a small local maximum at a depth of about 300 m. He was able to correlate this with a warm period, the "Little Climatic Optimum", lasting from A.D. 1000-1200 and a following cold period, the "Little Ice Age", beginning about 1500 A.D[17].

In 1969 three holes were drilled in the Central Patricia area of northern Ontario at Otoskin River, Minchin Lake and English River. The results had to wait nearly ten years for publication[18], by which time 21 values of heat flow, with associated measurements of radioactive heat generation, were available in the southern part of the Superior Province. This is the oldest Province in the Shield, with a mean age of 2.5 billion years. It is deeply eroded, and the heat generation has had time to decrease by the process of natural decay. As a result the measured heat flow and heat generation are low. This is typical of Archean shields but there are significant differences between them.

An interesting, and negative, study of past temperatures[19] was made in a deep hole in the Flin Flon area. At 2865 m this hole was deep enough that it was expected to see the beginning and end of the Pleistocene glaciation imprinted on the temperature gradient. There was no evidence of this. This casts some doubts on the practice of correcting for the glaciation effect.

In the 1970s a knowledge of the distribution and thickness of permafrost became very important in studies of the feasibility of oil and gas pipelines. The Heat Flow group was able to supply the information. In 1973 Judge[11] published the map, on the previous page, showing permafrost thickness, and in the same year discussed means by which permafrost thickness could be predicted[20].

The second map[11] illustrates the progress of heat flow measurement to the close of our period of interest. It is an impressive achievement for a small group in an effective period of not much over six years.

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G.J. Odgers and R.M. Petrie on top of Mount Kobau.

XV – THE FINAL CHAPTER

THE RISE AND FALL OF THE QUEEN ELIZABETH II TELESCOPE

Proposal and Acceptance

Some time early in the 1960s Canadian astronomers began to feel the need for a larger telescope. The leading position that Canada had enjoyed a generation earlier, with the 72-inch telescope at the Dominion Astrophysical Observatory and the 74-inch at the David Dunlap Observatory of the University of Toronto, was being rapidly eroded. Accelerating astronomical research in other countries had provided larger and larger instruments, and Canada had slipped into tenth place among countries possessing large telescopes. Modern astronomy was turning increasingly to the spectrographic and photometric study of globular and galactic clusters, which were believed to hold the secret of stellar evolution, and to the observation of external galaxies, of their distances, ages and motions, which held the clues to the history of the universe. Canadian astronomers were excluded from these studies by the lack of a large-aperture telescope.

Canadian universities had expanded since the end of the war, both in numbers and in complexity, and many of them had started departments of astronomy. These worked under severe handicaps with inadequate observing facilities, and their brighter students were attracted to the United States where these were available. Only the University of Toronto had a major telescope, and this was proving inadequate for even its own students. There was a ten-fold increase in the number of students enrolled in astronomy, and a questionnaire circulated to universities, government laboratories and industrial firms suggested the need for 30 fully trained astronomers and 30 astronomy-trained physicists within the next five years. Further studies by Professor Donald MacRae showed that this accelerated need would continue, and that four or five new centres of graduate training in astronomy would have to be developed to keep abreast of the demand. Not all of these anticipated students would work in optical astronomy. Radio astronomy and theoretical astrophysics would occupy many of them, but a steadily increasing demand for observing time on a first-class telescope had to be anticipated.

A similar situation had been developing in American universities; to meet the need an "Association of Universities for Research in Astronomy" (AURA) had been established "to provide American astronomers and qualified graduate students with modern telescopic and instrumental equipment for astronomical research". Already two moderate sized, Cassegrain focus, telescopes, a 16-inch and a 36-inch, were in operation at the Kitt Peak National Observatory, in Arizona, and an 84-inch telescope of advanced design was undergoing final testing. Plans were well advanced for a 150-inch telescope, and its funding was assured.

Much of this expanded interest in astronomy was directed toward photometry. We learned something about photometry in Chapter VII. By measuring a star's apparent brightness at a number of different colours it is possible to discover its physical properties – its absolute magnitude, temperature, radius and spectral classification, and to measure the interstellar absorption that its light suffers.

By the early 1960s photometry measured colours and brightness with photoelectric detectors, which were at least twenty times more sensitive than the photographic emulsions then used in spectroscopy. There were a number of consequences: photometric observations could be made much more quickly than spectrographic ones, at the rate of 20 to 50 an hour; the same stars could be studied with much smaller telescopes; fainter and more distant objects, beyond the limits of spectroscopy, could be observed, thus pushing back the limits of the observed universe.

Photometry had not been a major Canadian interest. It requires very clear skies without the contamination of city lights, and stable atmospheres. Neither the Dominion Astrophysical Observatory nor the David Dunlap Observatory at Toronto provided these. But there was a growing demand for photometric facilities. This was particularly true in university groups; the speed with which data could be accumulated made photometric studies ideal as thesis material.

Petrie, with advice from his astronomer friends at Kitt Peak, Mount Wilson and Palomar, started to consider the specifications for a large telescope early in 1962. At the same time Odgers began a search for a suitable site. In late October 1962 they discussed their plans at a meeting of the Canadian Committee for the IAU, which supported the idea with a formal resolution[1]. Beals sent a copy of this resolution to the Deputy Minister, W.E. van Steenburgh, who replied[2]:

> "There is no question about the need for a larger telescope in Canada, – the problem is . . . to present the need in such a way that it will receive 'agreement in principle' in the reasonably near future.
>
> In the future, no costly scientific program will receive recognition and approval unless it is *completely* documented. This means a consideration of need, location, design costs and final costs. Such a document should be prepared in acceptable booklet form for provision of adequate copies for all levels of consideration. The 'Upper Mantle Project' presentation might serve as an illustration of what is needed."

Clearly planning had not progressed to the point that such a detailed study could be presented, but publicity could be given to the idea. Petrie[3] published a paper in the Journal of the Royal Astronomical Society of Canada calling for "A Large Optical Telescope for Canada". He reviewed the history of astronomy in Canada, described some of the exciting and important fields of research open only through a large telescope, and speculated on the telescope design. Professor Helen Hogg, of the University of Toronto, wrote about the need for a new telescope in her column "With The Stars" in the Toronto Star[4] and Professor Wehlau of the University of Western Ontario supported the project with a paper in the Journal of the Royal Astronomical Society of Canada[5].

Support for the telescope came from an unexpected source. In 1963 the government set up an Interdepartmental Committee to plan for the celebration of Canada's Centennial in 1967. Each Department, and ultimately each Branch, was asked to suggest projects. Beals made two proposals:

1. "a 150-inch optical telescope available to all universities and qualified students in Canada ... to be erected and operated by the Department of Mines and Technical Surveys".

2. "a geophysical, geological, oceanographic and topographical study of Hudson Bay and its environments."

The second of these suggestions led to a major retirement project for Beals, the editing of a two volume study on "Science, History and Hudson Bay". We are here interested in the first proposal. It appealed to van Steenburgh; he sent it to the Interdepartmental Committee with a strong recommendation, and he immediately took charge of the campaign for its acceptance by the government. I have described elsewhere van Steenburgh's enthusiasm for imaginative and worthwhile scientific projects, and his great ability to push them through. As Beals put it[6]:

> "We are all hoping that Dr. van Steenburgh's almost uncanny feeling for the promotion of a scientific project will run true to form when the large telescope project comes to a showdown".

I think the astronomers in Victoria were a little surprised, perhaps even alarmed, at the speed with which things moved. Five days after receiving the proposal van Steenburgh was calling for "a well thought out brief on the proposal for a new telescope"[7]; again the pamphlet on the Upper Mantle Project was suggested as a model.

The brief reached Ottawa by November 15, 1963, and 150 copies had been printed within a month. Van Steenburgh also called for letters of support from astronomers and organizations: appropriate resolutions were passed by the Ottawa Centre of the Royal Astronomical Society of Canada[8] and by the Council of the parent society[9] and many astronomers, including Dr. I.S. Bowen, recently retired Director of the Mount Wilson and Palomar Observatories, wrote in support of the project.

Van Steenburgh was clear on one point: the telescope must be built in Canada. Millman had proposed that, considering the dearth of large telescopes in the southern hemisphere, Canada should join with other Commonwealth countries to support a telescope in the southern hemisphere instead of lobbying for a large telescope in Canada. Commenting on this van Steenburgh wrote[10]:

> "Dr. Millman has expressed the same thing to me in the past. I told him at that time, and I wish to repeat myself again, that the chances of securing such an instrument for outside Canada would appear impossible.
>
> It is going to be difficult enough to persuade the Government to construct a large telescope in Canada. To suggest such an instrument for a foreign country would compound the difficulties several hundreds of times."

Juan Geuer's conceptual drawing of the 150" telescope and dome. This drawing appeared on the cover of the initial brief for a Confederation telescope; the caption was modified when the telescope was renamed to honour the Queen.

The Interdepartmental Committee liked the proposal for the telescope, although it questioned how much of the Observatory could be completed in time for the Centennial. When it was suggested that a road, support buildings and some equipment transferred from Ottawa could be in place by 1967, the Committee accepted the telescope[11] as "The Confederation Telescope" and agreed to support it. A first draft of a Memorandum to Cabinet was prepared by February 25, 1964.

Van Steenburgh was glad of any support but he had no intention of leaving the matter there. In late March 1964, he and Beals appeared before the National Research Council to present the case for the new telescope. Beals reported to Petrie[12]:

> "The meeting consisted of about 25 persons including Millman and Herzberg who were present for the discussion on the telescope. ... The reaction so far as I could judge was a good deal more favourable than I had anticipated. Millman and Herzberg both gave us strong support as did several of the physicists from whom I had

> expected opposition because of the competing accelerator project. Members of the NRC were inclined to insist that universities and other organizations (such as NRC!) should have a definite voice in policy and management and the Deputy Minister agreed with them.
>
> It would probably be a good thing therefore to give some thought to the formal organization of what, regardless of its value, will almost certainly be a new Institute for advanced research in the Astronomical Sciences with a board of directors not unlike a miniature Research Council with representation across Canada. My own impression is that a good preliminary proposal along these lines could go a long way toward assuring the success of the project so I recommend it to your attention."

Leaving nothing to chance, Beals himself revised the Cabinet submission to accommodate the concerns of the NRC. These having been met, President Ballard wrote to C.M. Drury, Chairman of the Privy Council Committee on Scientific and Industrial Research, giving the Council's unqualified support for the project. The Departmental submission was sent forward late in June, was reviewed by the Committee Secretariat, and forwarded to Treasury Board for study.

The Committee could not meet to consider the submission until Treasury Board approval had been obtained. There was no way of knowing when this would occur, which complicated van Steenburgh's plans for Departmental representation at the meeting. Beals had retired at the end of June; I would replace him, but would need strong astronomical back-up. Petrie was committed to attending the meetings of the International Astronomical Union and planned to leave Canada in early August, returning in early October. When it seemed clear that the meeting would take place during Petrie's absence, van Steenburgh arranged with the Committee Secretary that we be given four days notice of the meeting, and instructed that Odgers should hold himself in readiness to come to Ottawa on short notice.

The meeting was held on September 21[13]. The Committee consisted of six Ministers chaired by C.M. Drury, the Minister of Industry. It was augmented by a distinguished group, including the President of NRC, the Chairman of the Defence Research Board, and the Deputy Ministers of the Departments of Finance and of Industry. Dr. F.A. Forward, of the recently established Scientific Secretariat was also there. Our Department was represented by its Minister, Mr. Benidickson, van Steenburgh, Odgers and myself.

The reception was most cordial. Benidickson introduced the proposal, van Steenburgh spoke briefly, and I was invited to make the principal defence. This was done, I think, very effectively, but it was a nervous time because Odgers had not yet shown up at the time of my presentation. He arrived, very much out of breath, almost an hour late. He had made an error of an hour in adjusting his watch to Ottawa time, had spent the past hour pacing up and down in front of the Parliament Buildings, until he had happened to glance up at the clock in the Peace Tower!

He explained the situation so amusingly, pointing out his chagrin as an astronomer in not knowing what time it was, apologised so charmingly, that the Committee was in fine humour. His mistake was almost an asset in gaining the Committee's sympathy.

There were many questions, some administrative, which van Steenburgh or I answered, most technical, which Odgers fielded. At the end "the Committee agreed to recommend to the Cabinet that approval in principle be given to a Confederation telescope ... subject to a review by the Treasury Board, in consultation with the Department of Mines and Technical Surveys, of the most suitable time-phasing of the project."

After the meeting Odgers and I returned to the Observatory in a high state of euphoria. Odgers sent a telegram to Victoria, with the good news, I a cable to Dr. and Mrs. Petrie[14] at their hotel in London. I ended the cable with "recommend Cordon Rouge as appropriate", referring of course to the splendid product of G. H. Mumm and Co. of Reims.

In our euphoria neither Odgers nor I thought to spread the glad tidings around the Observatory. In particular we neglected to tell Locke, who had worked hard for the telescope, especially after Beals' retirement had placed a non-astronomer in the Director's chair. It was an oversight which I much regretted, and which created the erroneous impression that the Ottawa astronomers were being excluded from the planning.

On September 23, 1964 Cabinet accepted the project. I cabled the Petries, now returning home on the Empress of England. They had not understood my reference to Cordon Rouge, but by the time my cable arrived telling of the final acceptance the message had been interpreted. They cabled back[15]: "Message received congratulations appropriate ceremony observed".

Her Majesty the Queen was to visit Canada on October 5-11. The Cabinet was in something of a quandary about a suitable gift to mark the occasion. Someone suggested, perhaps facetiously, that they might give her the telescope, which they had just approved. The suggestion appealed to the Ministers and it was agreed to.

An interesting idea, but how could the presentation of an observatory, still in the astronomer's minds, be symbolized? Odgers had shown some drawings of the proposed Kitt Peak Observatory at the meeting of the Privy Council Committee. Could these be made available? They could, and better still, so could a scale model of the telescope and dome. These were shipped by air freight from Tucson and arrived in time for the presentation. Her Majesty was obviously pleased, as the photograph on the following page will attest.

Not everyone was happy with the new name for the telescope. A Dr. Bennett, from Nanaimo British Columbia, wrote to the Prime Minister objecting to the name. The telescope should have been named after some pioneering astronomer, probably J.S. Plaskett. It fell to me to write the reply. Mr. Pearson signed a letter which contained the following paragraph[16].

Her Majesty Queen Elizabeth II, Prince Phillip and Prime Minister Pearson admire the model telescope. National Archives of Canada, Photo C135459.

> "I am sorry that you are disappointed in the name 'Queen Elizabeth II' telescope which we had felt was a particularly happy selection. It honours the gracious and courageous Queen of Canada and, at the same time, removes the telescope from the realm of partisan politics throughout the long period which will be required for its completion."

With hindsight we may regard this as a prime example of dramatic irony!

Even before the Government had announced its intention of dedicating the new telescope to the Queen, van Steenburgh wrote to Petrie[17]: "I think it is time that we finalize the site – i.e. if you are ready to do so. I have not been anxious to give out specific information about location, but after the announcement is made my reasons for keeping quiet will have disappeared."

This was requiring the astronomers to make a site selection before they would have wished to; they had settled on the southern Okanagan Valley, and Mount Kobau was certainly the preferred site of the three under consideration but Petrie wanted another year, perhaps two, of testing[18]. However, he opted for Mount Kobau and an announcement to this effect was made by the Government on October 28. The announcement included a sentence which, while true, would come back to haunt us. "In the dry belt of the British Columbia Interior, the area is a northward extension of the Great American Desert."

Before making the announcement on the location the Prime Minister required some reassurance on the choice, and I was bid to his office. Mount Kobau was not in a Liberal riding; could I assure him that there was no Liberal mountain that would be just as satisfactory? I could, and did, having phoned Victoria earlier in the day for moral support.

Before anything could be done it was necessary to comply with the Privy Council Committee's directive for a review, with Treasury Board, of the "most suitable time-phasing of the project". Departmental plans were spelled out in a memorandum[19] which called for a six-year schedule, with completion in the fiscal year 1970-71, and a total cost of ten million dollars. This was accepted by Treasury Board[20], and funds and positions for the first year were approved. The memorandum made the point very firmly that "the minimum time for completion ... is determined almost entirely by the lengthy period required to manufacture the optical components of the telescope. ... For a six-year time schedule to be met, the order for the 150-inch mirror blank must be placed without delay." The point might also have been made that planning for the grinding and polishing of the mirror, involving the design and construction of the polishing machines and of the Optical Shop to house them, must also begin but this was not done. The need to order the mirror blank was accepted; the requisition was submitted almost immediately, tenders were received by early July 1965, and the contract was awarded to the Corning Glass Works at a contract price of $1,148,000.

In December 1963, months before the telescope had been approved, it was announced that the instrument and electronics division of Canadian Arsenals Ltd., in Scarboro, would be closed. This division had been a source of much fine optical equipment for Canadian astronomers. Professor MacRae, of the University of Toronto, brought the closing to Beals' attention[21], suggesting that some means be found of hiring the opticians. This was done, and Roy Dancey and John Miller were brought on staff by June 1965. They were able to contribute expert knowledge to all questions involving the polishing of the mirrors.

Scientists regard administration as, at best, a necessary evil, but in this case it was very important. The work would involve two departments, Mines and Technical Surveys and Public Works, and the Treasury Board, and the expenditure of large sums of money; the separation of senior management from the scientists by half a continent complicated the arrangements.

Within the Department the lines of communication were simple: in Victoria, Petrie and Odgers were responsible for all aspects of telescope planning, in Ottawa, the responsibilities rested with, in order of seniority, W.E. van Steenburgh, Deputy Minister, J.M. Harrison, Assistant Deputy Minister for Research, J.H. Hodgson, Director of the Observatories Branch, and J.L. Locke, Chief of the Stellar Physics Division. Because Petrie was almost completely occupied with the design of the new telescope, K.O. Wright assumed responsibility for most of the management of the Dominion Astrophysical Observatory.

The aim was always to let the astronomers make all technical decisions and to keep them fully informed of administrative decisions in Ottawa, but to intrude on their time as little as possible. There were some failures, but, by and large we did this; as I have reviewed the files from those exciting times I am quite impressed with what we did. I am equally impressed by the fact that the astronomers responded promptly, cheerfully and effectively to the administrative demands that were made on them.

While all technical decisions about the telescope and the associated observatory would be made by the Department of Mines and Technical Surveys, the actual work on the mountain top would be supervised by the Department of Public Works. This was the responsibility of the Chief Architect, J.A. Langford, and of the Regional Architect for British Columbia, R.J. Bickford. To provide effective cooperation between the two Departments, P.Z. Marcsan was appointed as a Liaison Architect.

A number of committees were set up to coordinate the work. As we shall see presently, the firm of A.B. Sanderson, of Victoria, was appointed as prime consultant. Sanderson arranged for monthly meetings of everyone involved in the project to discuss the progress of the work; this group was referred to as the Steering Committee. It usually met in Victoria and the meetings were frequently attended by representatives from Ottawa.

The idea that a committee of senior astronomers, government and university, should be established to advise the Department was in Beals's mind some time before his retirement and was certainly advanced by van Steenburgh's agreement to the NRC suggestion "that Universities and other organizations should have a definite voice in policy and management" of the proposed telescope. Discussions were held with university astronomers at Toronto, Western and Queen's. All agreed that such a committee would be valuable, that it should consist of approximately equal numbers of representatives from universities and government, but that the number of university representatives should not be sufficient to override the government, that it should be chaired by the Director of the Observatories Branch or his designate and that the Secretary should be appointed from the Observatory staff. A proposal for the establishment of a National Advisory Committee on Astronomy, so constituted, was approved by Treasury Board in early April 1965[22]. The first meeting of the committee was held on October 26, 1965.

The amount of planning that had to be done was clearly beyond the resources either of the Regional Architect, or of the small design group in Victoria. The Department of Public Works proposed[23] "that a competent firm be hired to carry out a study for the project in its entirety and ultimate form, so that the planning is carried out with the whole project in mind and includes all aspects, present and future, in the proposal. This study is to form a basis for the whole development." This call for a Prime Consultant was echoed by Petrie[24]: "It is now our opinion that the appointment of a consultant, for the whole project, is the best way to make progress and this should be done as soon as possible."

Very well, but what would the responsibilities of the Prime Consultant be? The astronomers were clear about how the design of the telescope was to be carried out. It must proceed under their close supervision aided by experts from a variety of engineering disciplines and with the advice of their colleagues in the United States. The design would depend on the research for which the telescope was to be used and each step in the design must be examined for feasibility from an engineering point of view. Only when these studies had been completed would it be possible to draw up specifications to permit competitive bidding.

This approach was completely unacceptable to Treasury Board[25]. All past experience "in the design and development of unique equipment" had taught them that "competitive tenders for the design and fabrication of such equipment" led to better cost control and lower prices. They advocated that "the scope of the prime consultant's work should be limited wherever possible to the establishment of the necessary specifications, and that the design of the telescope and its ancillaries should be accomplished through competitive tenders, preferably in a major package, i.e. telescope, mount, drive and controls."

This was unsatisfactory both to the astronomers and to the Department. In his reply to the Treasury Board[26], van Steenburgh pointed out that each large telescope produces unique problems and that no firm existed with specific competence. "We do not wish to set up our own engineering competence, as the Americans have done. Rather we wish to employ, through the prime consultant, the best specialized skills available. These people are in a variety of industries and in Universities, and we believe that most of the experts we need can be found in Canada. The consultants will, for the most part, need to work in Victoria so that they can be in constant consultation with the prime consultant and with the astronomers". Treasury Board remained unconvinced.

At the same time an English firm, Sir Howard Grubb Parsons and Company, was campaigning to obtain the contract to build the telescope. This firm had an unquestioned competence in the construction of large telescopes. It had supplied the 48-inch telescope at Victoria and was currently building the 98-inch Sir Isaac Newton telescope, to be erected in the United Kingdom. Their approach[27] urged exactly the arrangement which Treasury Board was proposing: "that competitive designs to a performance specification prepared by your Department, be requested, to include prices for

(a) Grinding and polishing the mirror blank;

(b) Design and manufacture of the complete package;

(c) Supervision of erection, optical testing and commissioning.

I have not been able to find the original letter from Grubb Parsons, but to judge from the reaction of astronomers, it must have been pretty offensive[28], suggesting the incompetence of Canadian astronomers, and industry, and offering not more than 10% Canadian content in the finished product. But, since it accorded so closely with Treasury Board thinking it required careful consideration. There were many things against it: the limited Canadian content, the fact that the Grubb Parsons design would be an up-scale of the Sir Isaac Newton telescope, which was yet far from being successful, that the mirrors would be ground in England, with no provision for final figuring in the telescope dome. Most serious of all, the opportunity for developing a Canadian expertise in telescope design would be lost. A major submission to Treasury Board[29] restated the Departmental position on the design and construction of the telescope and, in particular the reasons why the Grubb Parsons proposal was unacceptable. The submission was supported by a resolution of the newly-formed National Advisory Committee on Astronomy. It ended with a plea for the early appointment of the prime consultant and of an engineering design team, and for the early construction of the optical shop.

This seems to have ended the discussion except for one question: why have a *prime* consultant, rather than having the Department itself hire consultants as needed? The arrangement was opposed by at least one senior officer of Treasury Board[30]. Time proved him right, but the idea of a prime consultant was accepted and the firm of A.B. Sanderson of Victoria, which had been acting in a limited way, was appointed on January 27, 1966. Sanderson retained the architectural firm of Wade, Stockdill, Armour and Partners, of Vancouver, to assist in the design of ancillary facilities on the mountain top, and, on the advice of the astronomers, the firm of Dilworth, Secord, Meagher and Associates of Toronto as engineers to work with them on the design of the telescope. Treasury Board set up a Monitoring Committee[31], consisting of representatives of Treasury Board and the Departments of Mines and Technical Surveys and Public Works, to which Sanderson was to report monthly on the state of the work, on the money spent to date, and the projected costs.

The contract for Phase I covered the preliminary planning of all aspects of the telescope and the Observatory – the telescope itself, carried to the point of establishing the practicability of the design, the optical components that this design would entail and the best method of procuring them, the facilities and services required on the mountain top and the design and location of a headquarters building and an optical shop. All alternative methods of supply were to be investigated, costs were to be estimated, and the probable percentage of Canadian content to be determined. A critical path study of all the elements of the project, showing time schedules and phasing, was to be prepared, and the most effective method of monitoring and controlling costs and timing was to be suggested. The period of the study was to be one year; this was later extended to 15 months, bringing the closing date to March 31, 1967.

Two blows hit the project early in 1966. One, the retirement of van Steenburgh, had been anticipated. He was succeeded as Deputy Minister by C.M. Isbister. Isbister supported the project fully, but was never deeply involved in its management. Van Steenburgh was appointed Scientific Advisor to the Cabinet and in this capacity continued his involvement with the telescope. Harrison assumed the leading Departmental role, assisted by a newly-appointed Executive Assistant, Duncan Turnbull, formerly a senior officer with the Civil Service Commission.

The second blow was the sudden death of Petrie on April 8, 1966. K.O. Wright was appointed Acting Director of the Dominion Astrophysical Observatory, a position in which he was shortly confirmed by the Civil Service Commission. Odgers, who had been working closely with Petrie, assumed responsibility for the telescope design, and for overall planning of the entire project, and E.H. Richardson, G.A. Brealey and D.H. Andrews were assigned to the design team. This arrangement, both in Ottawa and in Victoria, continued throughout the life of the project.

Phase I

Planning the Mountain Top

Mount Kobau lies on the Thompson Plateau, a long, narrow, flat-topped ridge, at an elevation of approximately 6000 feet above sea level. The ridge extends in a north-south direction, and its top varies in width from about one to two miles. There are many knolls and small peaks on the plateau, the highest, Mount Kobau, having an elevation of 6,148 feet.

A first requirement was to secure the land. Since it was Crown Land, in the right of the Province of British Columbia, this was a formality. An area one mile wide and two miles long was set aside for the Observatory.

The planning for the development of Mount Kobau was the principal responsibility of the prime consultant and of the architects. This was a much more complex matter than simply arranging for the large telescope. We saw in Chapter V, in the Beals-Petrie correspondence, that these two leaders hoped that the new telescope would become the focus of a major observatory, to which most if not all of the astronomical work at the Dominion Observatory would be transferred. Petrie even envisaged the ultimate transfer of the two Victoria telescopes to the new site. This vision of a National Observatory, that would bring together in one institution positional astronomers, astrophysicists, solar astronomers and radio astronomers was perhaps as important to Beals as the large telescope itself. The idea was not stressed particularly in the presentations to the Government, but the correspondence makes it clear that a National Observatory was very much a part of their thinking. From the beginning it was part of ours, and it had to be an integral part of the planning of the mountain top, even though its implementation might be far in the future.

In late September 1965 a submission was prepared for consideration by the Advisory Committee describing a long-range astronomical research program for Mount Kobau. It called for the possible transfer of the two telescopes from Victoria, the construction of two photometric telescopes of 40-inch and 16-inch aperture, the construction of two solar telescopes, a spar and a solar tower, the transfer of the Mirror Transit telescope from Ottawa and the construction of a 60-inch astrometric telescope, and the transfer of the Super-Schmidt cameras from northern Alberta. The total cost, exclusive of the 150-inch telescope, was estimated at three and a half million dollars, of which approximately 60% could be found by re-directing existing budgets. The plan called for the establishment of an "Institute of Astronomy" as a Headquarters for the complex. The Committee approved the plan with minor variations, and urged that an approach be made to the University of British Columbia as a possible location for this Institute. The revised submission was then issued as a "Blue Book" embodying the Departmental plan for the development of Mount Kobau. Treasury Board accepted the submission, in principle, in May 1966[32]; this did not of course commit it to funding the development.

During the summer one could drive to the mountain top by four-wheel vehicles travelling on existing ranch roads and cattle trails, but in winter it could be reached only on skis or snowshoes. A road to the top was a first consideration. Should it be the minimum road required for initial studies, or the road which would ultimately be required? The decision was for the latter; engineering studies were begun in June 1965, tenders were called in October, the contract was awarded to Peter Kiewit Sons of Canada, Ltd. Work proceeded throughout the winter. Construction was very difficult; snow conditions were the worst in 50 years, and the continuous grade presented unusual problems. Nevertheless the road was completed by the early summer of 1966, at a total cost of $1,480,000, and was officially accepted by the Minister, the Honourable Jean-Luc Pepin, on July 27, 1966. It had a width of 22 feet with six foot shoulders, an average grade of 7.5%, a maximum grade of 7.95%. Paving was deferred to the completion of the project.

Dr. V. Dolmage was retained to make a geological examination of the site. He reported[33]: "the topography and the geology of Mount Kobau as well as its recent geological history augur well for the permanent stability of the foundation of all the instruments and other structures of the observatory complex." Five holes were drilled at the proposed site of the large telescope. They encountered metamorphic rocks, quartzite and amphibolite schists, with considerable jointing. Dolmage suggested that grouting would be desirable.

Dolmage also investigated the available water supply. The conclusions were not encouraging. The only source of surface water was a small lake on Observatory property – Testalinden Lake – which obtained its water from the rain and snow falling on its relatively small drainage area. In 1937 the lake had been extended to provide water for irrigation in the valley below; the lake bottom was deepened to bedrock level and an earth-filled dam was constructed to raise the level of the lake. It was believed that this lake would provide adequate water in normal years but that it would be desirable to provide additional storage for dry years.

Drilling showed the rock under the dam to be badly fractured, resulting in considerable seepage. A small creek, fed by this seepage, begins some distance below the lake and it was suggested that this might be dammed to form a second reservoir.

Water rights, to the lake and to the creek, were held by local ranchers, who would have to be compensated if the water were diverted to Observatory use.

A second study of the water supply was made by E.C. Halstead of the Geological Survey of Canada[34]. It agreed essentially with Dolmage's conclusions: the total precipitation was about 22 inches per year, almost all of which was lost by run-off and evaporation; if the run-off could be ponded it would provide an adequate supply of water of good quality.

The philosophy dictating the design of the mountain top is well stated in the report[35]. "Major observatories are located upon the tops of remote mountains so that, among other reasons, they will be removed from the glare, haze, dust, and atmospheric disturbances associated with human habitation and industry. Accordingly, the arrangement and layout of the observatory should be designed so that, as far as possible, the services and facilities will not themselves create the very conditions it has been sought to provide by the remote location of the scientific instruments."

The top figure on the next page shows how it was proposed to meet the problem. The various telescopes would be located along the ridge, each having an unobstructed view as required and far enough apart so as not to influence the seeing conditions at neighbouring locations. The "Village" or operating centre for the mountain, the astronomer's residence and the Visitor's Centre, all heated buildings and therefore potential hazards to good seeing, would be far removed from the observing sites. The access road would enter at the lower left, pass a picnic area, and end in a parking lot outside the Observatory complex. This parking lot, itself a major source of thermal disturbance, would thus be removed as far as possible from the telescopes.

The lower figure shows the limited extent of the initial installation.

A major problem in protecting the environment on Mount Kobau was posed by the large numbers of visitors expected. Even before approval for the telescope had been announced we had been asked by our Minister about the number of visitors to be expected; Locke, basing his estimates on the numbers visiting other observatories, estimated the number at not less than 100,000 annually. The consultants, considering a large amount of data available on tourism in British Columbia, expected this number to be exceeded each month during the tourist season; they fixed on 130,000 visitors per month as an upper limit for planning.

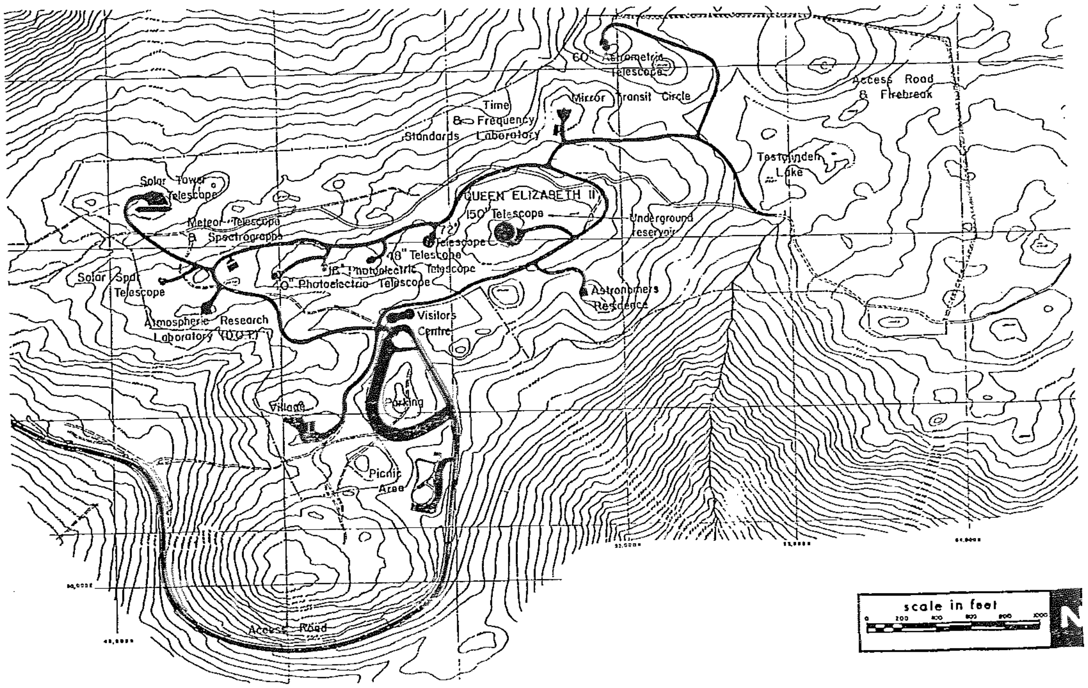
The maintenance of the fragile ground cover on the mountain was a major consideration. If this were destroyed it might never recover, with the resulting problem of wind-blown dust causing a deterioration in the seeing. Visitors must always be confined to paved areas. To prevent them from stopping on the way up the mountain and wandering off the paved road, the shoulders of the road were kept narrow, with paved look-out points provided to overlook the more interesting views.

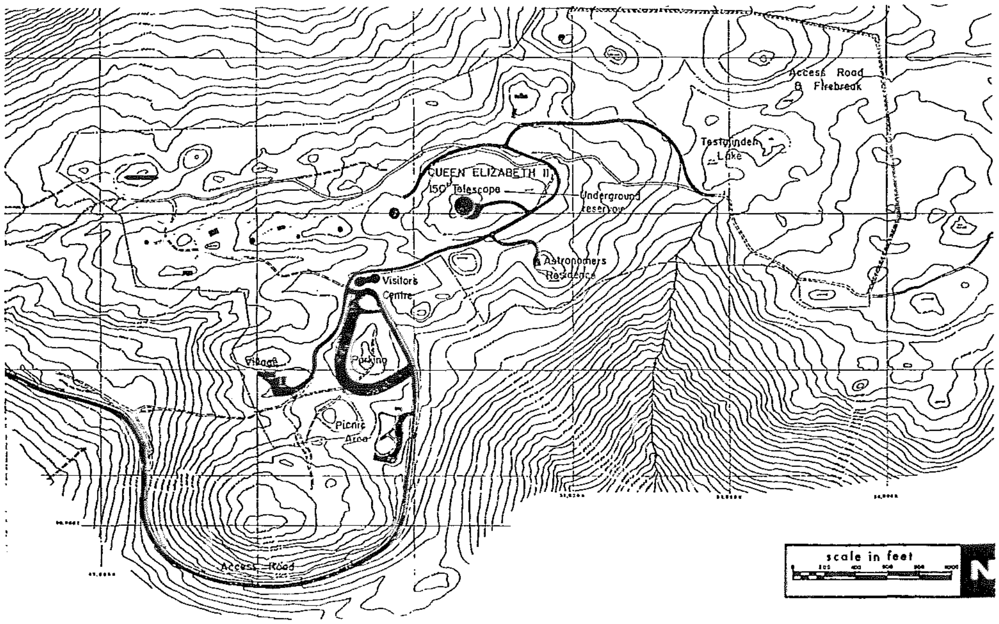
The visitors must also be kept out of the way of the astronomers. They would park outside the observatory fence, and proceed on foot to a reception and display building. The architects planned a quite elaborate and attractive building, worthy of a National Observatory, where visitors would learn about the design and construction of the telescope as well as some fundamental facts about astronomy. The building, as planned, would have three floors. Visitors would move from there to the large telescope in controlled numbers. In the dome there would be viewing galleries, from which they could see the telescope through non-radiating glass, and also an exterior gallery from which they could admire the countryside.

Climate[36] and Seeing[37]

Initial estimates of the number of clear hours that might be expected on Mount Kobau were based to a considerable extent on the observations of George Seaman, a school principal from Bridesville, who during the years 1964-1966 made observations on cloud cover and on seeing conditions. Additional information was obtained from an all-sky camera operated at Omak Mountain, about 30 miles south of Mount Kobau, at an elevation of 5200 feet. For 1966 it showed 1100 hours of clear sky on 160 nights; many nights with light overcast, suitable for spectroscopic observations, were not included in this figure. At Victoria in the same year there were 731 hours actually observed, making 1966 the worst year for observing in the 50 year history of the Observatory – the 40 year average, 1919-1958, being 1172 hours on 194 nights. That 1966 was also an anomalous year in the Okanagan Valley was confirmed by solar observations recorded at Summerland, some 50 miles to the north of Mount Kobau, over a thirty year period; 1965 and 1966 were well below the thirty year average, and 1966 had the least sunshine over the past six years.

On the basis of this information and comparison with the detailed records from Victoria Odgers stated[36] that "one would expect to have from 1200 to 1400 hours observing on





Mount Kobau per year, but with the same wide range as at Victoria, so that the extreme values would be 1,000 to 1,500 hours."

The collection of additional climatological data was an important part of the feasibility study, to which the Micrometeorological Section of Transport Canada contributed. They assembled them from published and unpublished observations from ten neighbouring Canadian meteorological stations and from five weather stations south of the international border maintained by the United States Geological Survey. In September 1966 they erected a 100-foot tower, which carried wind and temperature sensors at a variety of heights; the aim was to establish the boundaries of the ground turbulence layer and so provide data for the optimum height for the large telescope.

While the danger of basing climatological conclusions on such limited data was recognized, the study concluded that the mean annual precipitation on the mountain lay in the range of 20 to 24 inches per year. The snow load was highly variable and much of the snow ablated rather than melted. The temperature extremes were from -40 to +100°F but these extremes were to be expected only once in 50 years; a temperature of -20° was to be expected once in ten years.

Odgers and Petrie made the initial "seeing" survey of the Mount Kobau region between 1962 and 1964, using two small telescopes, an 8-inch Cassegrain reflector and a 3.5 inch Questar. They made continuous observations of a number of close double stars. On most nights binaries were resolved down to the theoretical telescope resolution. An objective calibration of this was supplied by observing in Victoria, with the 8-inch telescope set up outside the dome of the 48-inch, under a variety of conditions, and comparing the observations with those obtained at the coudé slit of the large telescope. Seeing at Mount Kobau was thus calibrated to the Victoria scale. Observations were also made of the moon and of the major planets; detailed and steady images were obtained to within 15° of the horizon. These methods were exactly those which had been used in selecting the sites for the highly successful Mount Wilson and Palomar telescopes.

The figure on this page shows their estimates of the diameter of the stellar disk on Mount Kobau as compared with that observed over the same period on the 48-inch telescope at Victoria. The former has a maximum at 1.3 seconds of arc, the latter at 3.5. The observations suggested that a disk value of 0.5" occurred frequently enough to be adopted as the resolution factor to be used in the design of the telescope.

Odgers reported[38]

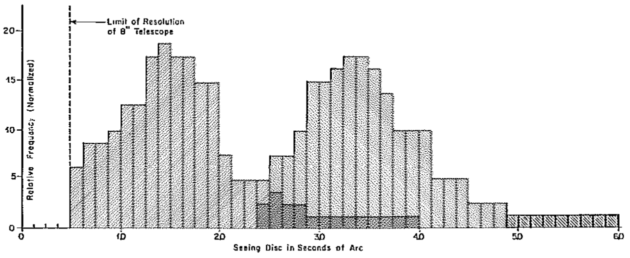
> "It became clear very early in the survey that seeing conditions at the mountain sites in the British Columbia interior were very much better than Victoria and that much of the summer seeing was very good indeed. The same instruments were used at Kitt Peak and Mount Wilson in March and April 1965 without altering the impression that the seeing during the good observing periods of spring, summer and fall was equal to Mount Wilson and that there could also be periods of excellent seeing in winter.
>
> . . .
>
> Hence, comparing seeing at Mount Kobau with Victoria, an 150-inch telescope at the former site has an efficiency greater by more than twenty compared with the Victoria 73-inch telescope. This factor is increased further by the use of more precise optical surfaces and an improved optical design, and present indications from the current state of the design indicate that this factor is more than forty."

During the years 1964-1966 Seaman made nightly estimates of seeing and sky transparency. During the winter months, because of the absence of a road, it was almost impossible for him to observe on Mount Kobau; he observed instead on two lower mountains that had easy access. His observations were believed to represent a lower limit for sky quality on Mount Kobau. Although the road to the summit of Mount Kobau was completed in 1966, permanent living quarters were not in place until mid-1967, after the completion of the Phase I report. Then, as we shall see, a thorough site-testing program was carried out, but it was too late to protect the site from the attacks of those who wished to denigrate it.

The Departmental plan called for the establishment of a solar observatory on Mount Kobau, and V. Gaizauskas conducted extensive site tests during the summers of 1966 and 1967. He used a 6-inch spar telescope and a ciné camera to monitor solar areas containing sun spots. Two sites on the mountain were selected for testing, and observing platforms were built at both locations by Mel Lytle, the Observatory carpenter. These raised the objective lens of the telescope six metres above the ground. As we have seen, 1966 was much below average in the hours of sunshine, which led to great frustration. Gaizauskas was heard to fulminate against those who would build an observatory on a mountain where the sun never shone, a phrase which later became something of a battle cry for those opposed to the Mount Kobau site for the large telescope. There were in fact many successive cloud-free days over the two years but the seeing was seldom of the highest quality. There was no point in having a new solar observatory with inferior seeing at the same longitude as the superb observatories in the southwestern United States. The search switched to the Ottawa area.

Design of the Telescope[39]

Petrie, assisted by Odgers, had begun the initial planning of the telescope even before its future was assured. The basic concept had already been tentatively agreed on, in consultation



Estimates of the diameter of the stellar disk, left at Mount Kobau, right with the 48-inch telescope at Victoria.

Observing platform, telescope and ciné camera used by Gaizauskas to monitor daytime seeing on Mount Kobau.

with university astronomers[40]. It would be a "Cassegrain" telescope, designed to provide optimum photometric observing at that focus, but with observing also at a prime focus and with powerful spectrographs utilizing a coudé focus.

To meet the photometric requirements the telescope would employ Ritchey-Chrétien optics. This design, then relatively new, employs primary and secondary mirrors of large departure from sphericity which, in combination, produce a field of about 45 minutes of arc at the Cassegrain focus free of coma and spherical aberration. Because of the aspherical primary mirror, the fields at the prime and coudé foci are badly distorted but these can be adjusted by correctors.

The prototype of this design was the 84-inch telescope at Kitt Peak, which had proved successful at the Cassegrain and coudé foci, but had not been designed for use at the prime focus. However a good deal of work had already been done on the design of the prime-focus correctors and it was generally accepted that these problems could be met.

Under the Ritchey-Chrétien system the surface of all the mirrors and lenses, primary, secondary and correctors, more than 30 in all, are extremely complex, and must be figured with the greatest accuracy. This is why the astronomers were so insistent that they must be figured in their own shop under their own supervision.

By the time engineering consultants had been appointed the specifications for the telescope had been decided on[41]. The mirror would have a diameter of 154 inches with an aperture of f/2.8. There would be three secondary mirrors; one with aperture of f/30 to supply the coudé focus, and two, of apertures f/8, and f/15, for the Cassegrain focus. The f/15 system was intended to provide optimum photometric and photographic operation. Correctors would provide a field of 1° at the prime focus. A coudé spectrograph with a beam length of 60 feet was planned to allow for the possibility of 24-inch gratings becoming available in the future.

Once these decisions were made, Petrie and Odgers had turned their attention to the telescope design. Should the secondary mirrors and the prime focus correctors be provided in a universal fixed end or as detachable units, to be stored outside the telescope and to be installed as required? How should the telescope be mounted to provide a maximum viewing area? Two methods suggested themselves, a fork polar axis or a horseshoe yoke. What were the ramifications of selecting one or the other? How would the mirror, expected to weigh some 16 tons, be supported so that it would suffer no distortions as the telescope changed position? In studying these questions design sketches of specific parts had to be prepared.

To help in this work, Beals[42] suggested that G.A. Brealey, who had been responsible for the mechanical design of the mirror transit, and who was "a minor genius with mechanical design", should become part of the design team. The need for this quickly became apparent; Petrie asked for Brealey's assistance, initially for a period of six months. He proved to be so valuable, not only in the initial design but as a liaison between the astronomers and the consulting engineers, that his transfer was made permanent.

The firm of mechanical engineers, Dilworth, Secord, Meagher and Associates, was appointed in January 1966. They established an office in Victoria, with E.E. Eggmann as chief engineer, and the astronomers and engineers worked in close and most satisfactory collaboration. At the same time a second design office was set up in Toronto, under the direction of J. Farrell, to study some of the purely mechanical problems involved in the design of such heavy yet precise equipment.

From the beginning the teams enjoyed the closest collaboration with other groups involved in the design of large telescopes: at the California Institute of Technology led by B. Rule and I.S. Bowen; at the Kitt Peak National Observatory led by W. Baustian and D. Crawford; at the European Southern Observatory, led by O. Heckmann and W. Strewinski. There was also close cooperation with astronomers at the Lick Observatory on Mount Hamilton, who operated a number of telescopes including a 120-inch reflector. Canadian astronomers and engineers visited the American observatories on many occasion and were invited to a number of large-telescope design symposia[43]. Nor was the cooperation limited to technical personnel; almost everyone with any responsibility to the project, from the Deputy Minister down, profited from visits to the American observatories and discussions about their particular part of the planning.

The magnitude of the problems involved in the design and manufacture of the telescope are well described in the following quotation[44]. "The telescope, which weighs many tons, must be directed in any viewing direction to an accuracy of ± 10 seconds of arc and must not allow a displacement of the optical components of more than one fiftieth of an inch. The optical surfaces of these components must be supported to within millionths of an inch."

A first problem, the solution of which influenced all other design, was the configuration of the prime focus end of the telescope. How were the various secondary mirrors, the cage for an observer and the several potentially large prime focus correctors to be installed and stored? One possibility was exchangeable components which would be installed directly into mountings on the telescope. This idea was discarded immediately because of the danger of accidents, both to equipment and personnel. Two possibilities remained, a cage that could be flipped in and out of position and from which the mirrors could be changed, or exchangeable ends. The moments acting on the telescope tube were three times higher with the former than with the latter solution; this would demand a 25% increase in the length of the fork tines needed to support the telescope tube and would increase the tube deflection by more than 200%. The use of exchangeable ends was adopted.

There would be three ends:

(1) A prime focus cage which would carry the observer, the plate holder and a number of small correctors. Provision would be made for carrying much larger and heavier correctors, the future development of which was anticipated;

(2) A "ring and spider" carrying the f/8 Ritchey-Chrétien secondary for the Cassegrain focus;

(3) A ring and spider carrying the f/15 secondary for the Cassegrain focus;

(4) A ring and spider carrying the f/30 secondary for the coudé focus.

In each case the ring would attach to the upper end of the telescope; the spider was a system of thin supports that would hold the mirror in position on the optic axis.

To interchange the ends, the telescope would be positioned horizontally and to the north. The exchange would be made by the night assistant, controlling an exchange machine on a hydraulic platform. It was estimated that one end could be removed and placed on a storage dolly on the observing floor, and a new end installed, in approximately fifteen minutes. This was very important; it would allow the telescope to be changed quickly from one mode to another to accommodate changes in the seeing quality.

The telescope tube would be open, an arrangement of trusses that would maintain the precise distance between the primary and secondary mirrors but provide a minimum weight to be carried by the supporting fork.

There are a number of ways in which a telescope can be supported with two freedoms of motion allowing it to be pointed in any direction. The easiest one for a large telescope is a "yoke" mount in which the telescope is carried in a large member which is held in the polar direction by supports at either end. Unfortunately, with this mounting the telescope is unable to scan some areas of the sky; these areas become larger as the polar axis becomes steeper, and the mount was therefore not acceptable for a telescope at the latitude of Mount Kobau.

Could a fork mounting be built? In this mounting a huge fork, lying in the direction of the polar axis, is cantilevered out from a supporting base and carries the telescope between its tines. The fork rotates around its axis to provide motion in right ascension, the telescope moves in the fork to provide motion in declination. There are two principal problems in the design: can the fork be made heavy enough to carry the telescope to any position without appreciable distortion; and can bearings be designed to permit the smooth rotation of the fork around its axis? Both these problems were investigated to a level that ensured they could be met.

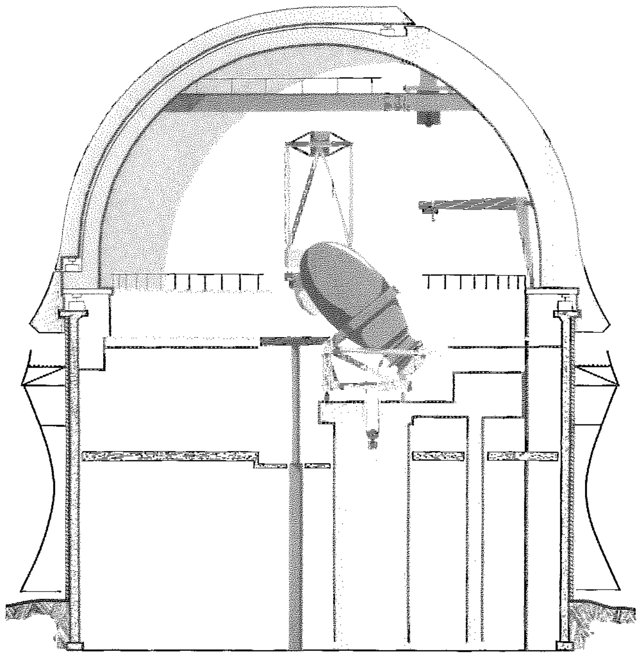
A more difficult problem concerned the support of the primary mirror. The mirror was expected to weigh in excess of 16 tons; in operation, when it might occupy any position from horizontal to vertical in any azimuth, its reflecting surface must not deviate from the designed configuration by more than 1/20 of a wave length of light! How was this to be accomplished?

A novel and very effective method was developed. The mirror would be centred in the optic axis by a three-point defining device located in the central hole. Its weight would be supported by twenty-four pneumatically operated pistons, the pressure to each of which would be automatically adjusted as the inclination of the mirror changed.

The changing lateral forces on the mirror would be met by a counterweight levering system that could provide tensile or compressive forces at 32 points around the periphery of the mirror through pads, bonded by adhesive to the mirror edge. The radial force imposed by each lever support system would be proportional to the mirror's inclination to the gravity field and its position around the periphery, the force varying from maximum tension at the upper location, through zero at the horizontal diametral axes, to a maximum compression at the lowest support.

Because the mirror support system was so critical, its design was carried much further than that for other parts of the telescope or the dome. One radial support and one axial support prototype were fabricated and a series of performance tests were conducted to determine precisely the operating efficiency that could be expected from them. Experiments were also set up to test the feasibility of bonding pads to the periphery. Pad and fused silica materials similar to those proposed for the telescope were tested for a variety of adhesives, and for the effects of moisture, corrosive agents, thermal cycling, tensile strength and creep under load[45].

The proposed dome was 118 feet in outside diameter and would rotate on 34 steel-wheeled trucks at a slewing rate of 36° per minute. A viewing slit nineteen feet wide was planned and the dome would track automatically during observing in conjunction with the telescope. There would be inside and



Cross section of the proposed dome and telescope.

outside viewing galleries for both the public and the astronomers. The declination axis of the telescope would be located 80 feet above the ground to avoid the temperature stratifications and turbulence caused by ground effects. Beneath the observing floor would be a large, two level, coudé room, and, at a still lower level, a room for the aluminizing of the large mirror. An elevator would transport the mirror to the telescope.

The estimated weights of the various parts of the assembly were as follows:

Cantilever fork –	108 tons.
Telescope tube –	75 tons,
Mirror –	about 16 tons,
Dome –	950 tons, 118′ diameter.

The costs, not including the Optical Shop, were estimated as follows:

Optical components –	$ 1.3 million
Telescope –	4.5
Controls –	0.8
Dome –	2.7
Building & foundations –	3.3
	$12.6 million

The National Institute of Astronomy and its Optical Shop

As outlined in the "Blue Book" and approved by the Advisory Committee, the National Observatory would consist not only of a complex of instruments on Mount Kobau, but also of a headquarters building, equipped with all the facilities, including computing, necessary for the reduction of observations. Most government astronomers would work at this headquarters, and university astronomers would return there after their periods of observation, or at any time that they wished. The general feeling, based on experience in other observatories, was that the headquarters should not be on Mount Kobau, nor even at one of the nearby towns, but should be attached to a major university, as a National Institute of Astronomy. One of the facilities of this Institute would be the Optical Shop to be used to grind the mirrors and lenses of the 150-inch telescope. Planning for the Institute could wait; planning for the Optical Shop could not.

Late in 1965[46] Petrie and Odgers, following the recommendation of the Advisory Committee, approached the President of the University of British Columbia about the possibility of this Institute being established on the campus of the University of British Columbia. The approach was supported by V.J. Okulitch, Dean of Science, who was a member of the National Advisory Committee. The proposal was enthusiastically accepted and, on May 10, 1966[47], the Board of Governors of the University approved, in principle, the assignment of a five-acre site for the Institute Headquarters and the Optical Shop, subject to its approval of the design. Planning for the shop proceeded on this basis.

Two polishing areas were planned, one to hold the 150-inch polishing machine, a second to hold the smaller machines. The two areas would be connected by a horizontal testing tunnel, but would normally be isolated from each other to prevent coarse grinding material in use in one area from contaminating the operations on the other. In addition to the polishing shops there would be a completely equipped machine shop.

A vertical test tower would be centred on the 150-inch polishing machine, rising about 75 feet above the table. It would consist of an inner instrument tower, surrounded by an outer tower to provide insulation and with working platforms and thermostated electric heaters every ten feet. During periods of testing, temperatures in the tower would be kept constant to ±2°F, and ventilation would be closed to prevent turbulence.

Access to the polishing shops would be through a change room, opening into the secondary shop. Personnel would change to and from their "clean" clothing on entering and leaving the polishing shop.

Since the large polishing machine would be needed only to produce the 150-inch mirror, consideration was given to the possibility of providing temporary facilities to house it, and limiting the final shop to one able to polish mirrors of smaller size. A possible temporary shop was found, an unused generating station of the BC Hydro, and the costs of the two possibilities were compared. The costs of the two-phase operation was greater by about a quarter of a million dollars; it was therefore recommended that the larger shop be built.

Consideration of the Phase I Report

The Phase I Report consisted of two volumes. The first, which dealt with the development of the mountain top and of the Institute, was written principally by the Sanderson staff, with a major input from the architectural consultants, Wade, Stockdill, Armour and Partners. Because the report considered the potential development of the mountain top as a focus for all government astronomy, there were submissions also from Ottawa astronomers about their specific requirements. The second volume, dealing with the design of the telescope and of the dome, was produced by the engineering consultants Dilworth, Secord, Meagher and Consultants, with the full cooperation of Odgers.

The report was submitted on March 31, 1967, in type-written form, in four large binders, two binders to a volume. It had been intended that the report would be printed, but in the interest of economy and prompt distribution this was not done. A number of Xeroxed copies were produced and these were distributed to all responsible government officials and to all members of the National Advisory Committee on Astronomy.

The most thorough examination of Volume I of the report was made by engineers and architects of the Department of Public Works Regional Office for British Columbia. Their report[48] consisted of 22 pages of specific criticisms and six more of summation and recommendations. They found the report repetitious, inconclusive and incomplete, with too much space devoted to the additional astronomical instruments which might later be deployed. They were particularly critical of the tourist facilities proposed: "it seems that the maximum has been aimed at ... as if a secondary feature is being allowed to obscure the real purpose of the project".

In fairness to Sanderson these latter two criticisms apply as much to the Department of Mines and Technical Surveys as to the prime consultant; it was important to us that our plans for a National Observatory should be taken into account in planning the mountain top. The tourist potential of the telescope had been one of the selling points in the approach to the Government. The Departmental astronomers didn't, of course, like this. As Wright said of this section[49]: "The discussions of the village and the Visitor's Centre are extensive, elaborate and grandiose."

A criticism that might have been levelled at the report concerns its failure to expand the knowledge of the climate and the seeing on Mount Kobau by direct observation during the seven months that had elapsed since the completion of the road.

The Public Works team were much kinder to Volume II:

> "The Sub-Consultants work on the telescope, mounting and immediate ancillary equipment seems to have been subjected to careful research and study and in any event the recommendations are strong and in most cases firm."

Dr. I.S. Bowen, former Director of the Mount Wilson and Palomar Observatories, and Mr. Bruce Rule, Director of the Central Engineering Services at the California Institute of Technology, were retained as consultants to decide on the merits of the preliminary telescope design. While each made some suggestions for improvements both were enthusiastic. To quote Rule[50]:

> "You have ample reasons to proceed with the final telescope design and construction phases with full confidence of the project success and in the work schedules planned and hopefully within the costs estimated."

At the suggestion of Bowen and Rule, John Case, a Californian with broad experience in the design of domes, was retained to consider the part of Volume II dealing with the dome. His comments on the dome and building were somewhat critical but were all in the direction of money-saving simplifications[51].

The Gathering Storm

From the beginning, astronomers at the University of Toronto had questioned the competence of the Victoria group to supervise the design and construction of a large telescope. Petrie found this somewhat annoying because, while they made vague criticisms, they would never offer concrete help in design problems[52]. When I visited them in May 1966, shortly after his death[53], their objections had become much stronger. They pointed out that the all three existing large telescopes had had severe problems, and thought that American designers, specifically Bowen and Rule, should be recruited to the design team.

During the visit they also attacked the concept of the Institute of Astronomy. They did not visualize having any need for the Institute; their staff and students would merely travel to Mount Kobau and return to the University. The Institute, as opposed to the Mount Kobau National Observatory, would serve no useful purpose for any university except that of British Columbia.

This was a remarkable change from the desire expressed by the Advisory Committee at its first meeting. The Observatory would be used by staff and students from many universities other than Toronto, few of which had the ancillary equipment necessary for the reduction of their observations. It seemed to me that there was a deeper reason for the Toronto change of attitude – that an Institute on the campus of the University of British Columbia would challenge Toronto's position as the senior graduate school of astronomy in Canada. In the struggles that followed I had no reason to change that appraisal.

The Toronto attack on the competence of the design team continued in subtle ways over the following months but was pretty well negated by an action of the design consultants Dilworth, Secord, Meagher and Associates. In late February 1967[54] they held an open seminar in their offices to disclose the status of the entire project, particularly of the telescope design. Representatives from government and universities as well as from potential industrial bidders on the telescope construction were invited. Senior people from the telescope design team, and from the various groups considering the design of the mountain top, discussed their work. All had come armed with good illustrations and with in-depth data that could provide answers to the most penetrating questions. It was apparent that the design team was competent, that the project was essentially on schedule and that the wishes of the universities had all been accommodated.

From this time on the Toronto attack was levelled, not at the competence of the design team, but at the site. Many astronomers were in Toronto early in March 1967, attending a meeting of the National Committee for the IAU. Toronto astronomers scheduled a discussion at the conclusion of the meetings to consider the suitability of Mount Kobau. I had some warning of this and had arranged for Beals to be present.

Professor Sidney van den Bergh was the principal speaker. He had his material well prepared, and he delivered it in so dramatic a fashion that I was reminded of Marc Anthony's oration over the corpse of Julius Caesar; graduate students played the role of the Roman mob.

He first attacked the site, suggesting that the number of usable hours would be 800 rather than the 1200 estimated by Odgers. Next he questioned the validity of seeing estimates based on the resolution of double stars by a small telescope. Bad seeing on a small telescope implied bad seeing on a large one, but good seeing on a small telescope did not ensure good seeing on a large one. The fact that Odgers had carefully correlated his measurements on Mount Kobau with the seeing on the 48-inch telescope at Victoria was not mentioned. Image motion techniques were now the preferred way of evaluating seeing and he would accept nothing less.

Whether or not one agreed with this didn't matter. The whole point of the discussion was, that no matter what values one adopted either for the number of observing hours or for the quality of seeing, Mount Kobau was much inferior to sites in Chile, where the six-year average of clear hours was 2600, of which 2200 were photometric, and where the median value of the seeing disk was 0.7°. Some rapid blackboard calculations demonstrated, to the converted, that even accepting Odgers' values, a 24 to 36 inch telescope in Chile would produce the same photometric results as a 150-inch telescope on Mount Kobau, and that a 50-inch telescope would produce equivalent photographic results. Even for spectroscopy a Chilean site would far outstrip Mount Kobau. Considering that future research would be involved with increasingly faint objects, a telescope on Mount Kobau would become less and less significant with the passing years. The conclusion? The telescope must be built in Chile!

Another factor to consider was the number of viewing hours. Assuming that a research astronomer would require 200 hours per year, the Mount Kobau telescope would provide facilities for five astronomers, whereas for a site in Chile the equivalent number would be thirteen. More importantly, an astronomer assigned time on a telescope in Chile could count on 12 or 13 clear nights in any fortnight. Such conditions were important for university professors and students who cannot leave their classrooms for months at a time. Anything less was quite unsuitable for a "national" observatory.

Finally, accepting the unsuitability of Mount Kobau for photometric observations, there was no point in the sophisticated optics which were proposed. A simple parabolic mirror, more within the capacity of inexperienced opticians, would suffice.

Beals presented the case for Mount Kobau. The excellent conditions available in Chile had been known to Canadian astronomers during the early days of planning but they had been assured by responsible officials that the government would not consider funding a telescope outside Canada. No one had suggested that Mount Kobau was not the best site in Canada. There was no question that it was inferior to sites in Chile, but an 150-inch telescope on Mount Kobau would give a performance at least 20 times better than that at Victoria. Over the years Victoria had published some 300 important papers, had established itself as a world centre in spectroscopic astronomy and, he might have added, produced two Fellows of the Royal Society of London; surely an output twenty times that was not to be lightly discarded. The argument that the Mount Kobau site would not be useful to university astronomers was specious; many astronomers from western universities, some of them photometrists, were already working with the Victoria telescopes and were looking forward to observing on Mount Kobau.

He also discussed the problems of building and maintaining a large telescope in a distant, scientifically unsophisticated and politically unstable country.

Finally he turned to the subject that was on all our minds: if the attack on the Queen Elizabeth telescope were to become public the result must certainly be that we would lose a telescope in Canada rather than gain one in Chile. "Would anyone", he questioned, "rather have no telescope than one on Mount Kobau" and called for a show of hands. Van den Bergh's hand, and those of the graduate students, shot up; hands of some other eastern astronomers were raised rather slowly and doubtfully.

With the publication of the Phase I Study at the end of March 1967 the Toronto attack expanded, and the absence of additional climatological or site-testing data did nothing to oppose it. Representations began to be made to senior officials in Ottawa as is attested by a letter which passed between the Department of Industry and the Treasury Board in late June[55]:

> "In the course of our enquiries, a serious question affecting the feasibility of the entire project came to light, viz. it is understood that a study of atmospheric conditions at Mt. Kobau performed by a University of Toronto Group concluded that due to prevailing temperature instability, accurate observations would only be possible for a very small percentage of the time. Unless it can be positively established that this condition is the exception rather than the rule, then the advisability of proceeding with this project is open to serious doubt."

At about this time a complicating factor appeared. The Carnegie Institution which, with the California Institute of Technology, supported the Mount Wilson and Palomar Observatories, had for some time been planning to build a major observatory in Chile, to be known as CARSO (for Carnegie Southern Observatory). The principal telescope would be a 200-inch reflector, more-or-less a duplicate of the Palomar telescope; it would be augmented by a 60-inch reflector and a 72-inch Schmidt with a 48-inch corrector. During one of Odgers' visits to Pasadena informal and confidential discussion had been held about the possibility of Canada joining in the funding, design, construction and use of this proposed observatory. Some time later I.S. Bowen, director of the California observatories, met Harrison at a scientific meeting and again, and again in confidence, broached the possibility. He stressed that the discussion was exploratory since the CARSO proposal was under consideration by an agency of the United States Government. Harrison "expressed interest, but not much enthusiasm. ... The proposal came at the worst possible time in the relation to the Mount Kobau project ... and it would not be possible to give an affirmative answer until the matter had been discussed with Treasury Board and probably with the Science Council[56]."

A meeting of the Advisory Committee was to be held in Ottawa on July 26-27, 1967, for the specific purpose of discussing the Feasibility Study. Copies of the Study, and an outline of the CARSO proposal, were circulated well before the meeting, and members were invited to submit written comments; these too were circulated. Van den Bergh was not a member of the committee but he submitted a short paper summarizing his earlier remarks and expanding them with specifics from the Study[57].

The Toronto astronomers were very disturbed by the Feasibility Study and, before the Advisory Committee could meet, MacRae wrote to O.M. Solandt, chairman of the newly formed Science Council, to present their position. Solandt recorded the approach in a memorandum which was seen by Prime Minister Pearson[58].

> "Professor Donald MacRae, Head of the Department of Astronomy at the University of Toronto wrote on June 30 and later called to discuss the problems of the Mt. Kobau National Observatory. There is to be a meeting of the Committee that is guiding the project in Ottawa on July 26th. In preparation for the meeting, the members have received a voluminous report by consultants on the whole Mt. Kobau team. Dr. MacRae is very disturbed by the information contained in the report on the visibility from Mt. Kobau. Based on the experience of last winter, there will only be 800-1000 useful viewing hours a year which is only enough to support 2-3 full time observers. This compares with about 2500 hours per year in the better sites in Chile. There were only about 15 nights out of 100 during which observations have been taken reasonably satisfactorily. DOT weather reports from nearby sites confirm the observations taken on the mountain top and also indicate that this was not an exceptionally bad winter. He suggests that the reason Mt. Kobau was chosen is because its summer weather is exceptionally good. Unfortunately, summer is not good for observing. At the 49th parallel, observations are virtually impossible in June and are limited in May and July. I questioned him carefully about why the site had been chosen. He says that the evidence is reasonably good though by no means conclusive, that the "seeing" as opposed to visibility on the Mt. Kobau site is as good as can be found in Canada, though not as good as is found at Kitt Peak or in Chile.
>
> He is also disturbed by the growth of the Mt. Kobau scheme. It now envisages moving virtually all the activities of the Federal Government in the field of astronomy to the Mt. Kobau site. The total cost is now estimated at about $29 million. It will include not only the 150" telescope but also an 80" telescope primarily for positional astronomy. He feels that if $29 million is spent on this institute, there will be no money left for the universities. He feels that even if the universities are given full opportunity to use the facilities at Mt. Kobau, many of them will not prove suitable for university work. He feels that the emphasis in the program is on positional astronomy and the time service and not in the kind of forward-looking research that universities would like to follow. He also said that a good deal of the expenditure was being devoted to providing facilities for tourists. The report visualizes as many as 30,000 visitors per month during the summer.
>
> Professor MacRae also outlined to me the great advantages that could be gained by having a major telescope in the Southern Hemisphere. He said that a group of astronomers at the California Institute of Technology had already located a suitable site in Chile and were well on the way to designing a 200" telescope for this site. The estimated cost of the project was $20 million. The Cal Tec people had tried to get help from the Ford Foundation without success. They are now seeking partners in the venture and have suggested that Canada might become a half owner of the scope for $10 million. Professor MacRae considers that the project would be far more valuable to Canadian universities than the Mt. Kobau project. I urged him to try to get this proposal into written form and to seek the support of other universities for it. In the meantime, I said I would discuss both problems with Dr. Harrison. I also urged Dr. MacRae to present his misgivings about the Mt. Kobau proposal clearly and firmly at the meetings on July 26th and to urge other university representatives to do the same if they shared his views."

Mr. Pearson objected to the fact that Solandt had "urged [MacRae] to get this proposal into written form and to seek the support of other universities for it". He commented: "I wish he had not done this – it involves the govt. in some responsibility if the support is secured".

MacRae may have spoken to some fellow members of the Advisory Committee about his approach to Solandt, but he did not inform government members of the Committee, and there is no mention of the approach in the minutes of the meeting. The above memorandum has only come to light, twenty years after the event, with the opening of Privy Council files.

Those files contain a later memorandum to the Prime Minister[59], which reminds him of his reaction to the Toronto approach and outlines some subsequent developments.

> "At that time you agreed that a change in the location of the observatory at Mount Kobau was not possible, and that the Science Secretariat might consider undertaking a study to see if the scale and scope of the project should be changed, having in mind Dr. MacRae's criticism: purportedly representative of the feeling of the universities.

> The Science Secretariat has considered this matter and now reports that to economize on the telescope itself at this stage would involve costly redesign and would leave the Dominion Observatory with an instrument which would have no significant scientific purpose. Under the circumstances they suggest that the most effective means of controlling costs, would be for the Secretariat to maintain close liaison with the Treasury Board, in order that appropriate pressure for maximum possible economy on the ancillary aspects of the project, can be exercised. The Science Secretariat has also indicated that the Department of Energy, Mines and Resources is suggesting substantial economies in the total program, [to] affect supporting facilities rather than the telescope itself.
>
> In the light of these comments it does not appear that the study by the Science Secretariat on the Queen Elizabeth II Telescope project would be useful, and if you agree, I shall inform the Science Secretariat not to proceed with it."

The memorandum went on to assure the Prime Minister that his concern about appearing to commit the Government to support for the CARSO project had been conveyed to Dr. Solandt, "who now appreciates that the difficulties with scientific priorities and expenditures with which the Government is attempting to cope at this time, make it clear that the above mentioned proposal is out of the question in so far as the Canadian Government is concerned". The same message had been conveyed to Dr. J.M. Harrison, who "will exert his efforts to avoid any embarrassment which would be caused by selling co-operation with the California Institute of Technology, in Chile, to universities, in the expectation of Government support".

When the Advisory Committee met on July 26-27, 1967 it was of course unaware of the MacRae's approach to the Science Council, or of the Government's reaction to it. It was clear from the beginning however[60] that the Committee, which had been so united in support of the telescope and of the National Observatory at its first meeting, was now divided into two groups, one consisting of astronomers from eastern universities, the other of representatives of government and of western Universities. Eastern astronomers insisted that from the beginning, due in part to the reference to "a spur of the Great American Desert", they had been "under the impression that the climatic conditions at Mount Kobau relative to optical observing were among the best on the continent – if not in the world – and it was in this climate that [they] had planned ... to centralize almost all Canadian astronomy there"[61]. Dr. G. Herzberg expressed the view of most of the other members[62]. He had "never gained this impression. The impression gained was that Mount Kobau has the best climatic conditions for a telescope to be located in *Canada*. ... The seeing disk is on the average less than half of what it is in Victoria and, in a not negligible percentage of the nights it is very much less than that."

This dichotomy led to two positions about the telescope: on the one hand the amount of observing time, and its distribution, could not satisfy the need of the eastern astronomers which was unacceptable in a national telescope; on the other hand there was a need for the national telescope to be in Canada, for the training of young astronomers, for the continuation and expansion of existing programs, and as a matter of prestige.

The two positions could be reconciled if Canada were to join in the CARSO project. With the understanding that a vigorous attempt would be made to do so, the Committee passed, unanimously, two resolutions, one calling for the completion of the Queen Elizabeth II telescope and the optical shop, the other for participation in the Carso project. These resolutions were forwarded to the Minister[63].

It was recognized that the government could not be expected to finance the two projects at the same time. Drastic cuts in the scope of the Mount Kobau development, particularly of the Visitors Centre, and delay in the transfer of other instruments, was recommended, and a small committee, independent of the National Advisory Committee, was set up to seek provincial and industrial funding for the CARSO cooperation. The membership of the committee was:

Professor D.A. MacRae, Toronto, Chairman;
Professor W. Wehlau, Western;
Professor G.A. Harrower, Queen's;
Dr. G.J. Odgers, DAO;
Dr. J.L. Locke, NRC.

A first meeting of the committee was held in Toronto on October 1st, 1967; it was attended by Dr. H.A.Babcock, Director of the Mount Wilson and Palomar Observatories, and by E.A. Ackerman, Executive Officer of the Carnegie Institution. They outlined the suggested terms of the agreement[64], that Canada should assume half the costs of the project and receive half the observing time, and that as much construction as possible would be undertaken in Canada, although a study would be required before the proportion of Canadian content could be established. The estimated capital costs were $18,000,000; the operating costs were more difficult to estimate, but would be in the range $250,000 to $800,000 per year. Ackerman proposed a period of approximately 12 months for exploring the possibilities of a joint venture.

The primary hope of the committee was that the costs would be met by the government instead of, or in addition to, the costs of the Queen Elizabeth II telescope. While Harrison made it clear that the Department could not support the project financially he agreed that the polishing equipment and the services of the design team and opticians would be available[65]. This would be a major contribution to the costs. For the rest the Committee proposed to approach provincial governments.

In late February 1968 Babcock came to Ottawa and outlined the Carnegie proposal on CARSO to Dr. Schneider, President of the NRC[66]. The reception was enthusiastic but, like EMR, NRC was not in a position to assist in the financing.

Meanwhile the Toronto attack on the Queen Elizabeth telescope didn't cease; rather it increased in intensity. It is a remarkable fact that nothing, not so much as a single letter, was ever forwarded to us for Departmental comment. We heard rumours, some fairly detailed. Letters from distinguished astronomers in several countries, praising the telescope sites in Chile, and "Dear Mike" letters from former colleagues at the University of Toronto were deluging the Prime Minister's office. There were personal attacks on me, suggesting that as a non-astronomer I was incompetent to direct the project. The case was used as the basis for a campaign against direct government involvement in science. We shall see later that some, at least, of these rumours were completely false, but they made for bad relationships.

One thing at least was definite. Dean Vincent Bladen appeared before the Senate Science Policy Committee in mid-March 1968, warning against narrow scientific nationalism, and deploring the idea of a National telescope taking precedence over a share in an American one in Chile. Bladen must also have had a chat with Simon Reisman, Deputy Minister of Industry, his old colleague of Auto Pact days, who twitted me one day about wanting to "build a telescope on a mountain where the sun never shines".

The attack had its desired effect. On December 7, 1967, Finance Minister Benson announced that the government found it necessary to curtail expenditures in a number of areas. One of them was the Queen Elizabeth Telescope project; the schedule for its construction would be stretched out and construction of the optical shop would be postponed. The project had been slowed down, not stopped, and planning must continue. It was not a happy climate in which to do so. We shall digress from the story of the conflict to review these developments.

Post Phase I Studies

Climate and Seeing

The most important work done related to the investigation of climate and seeing. Too late, it completely supported Odgers' appraisal with incontestable scientific evidence.

Living quarters were established on Mount Kobau during the summer of 1967. They included offices, dormitory facilities for 16 people, kitchen and workshop.C.J. [Jock] Crawford, who had represented Sanderson on the mountain, transferred to the Departmental staff and continued to manage the facility[67]. At the same time a standard weather station was installed and made regular measurements of wind direction and velocity, temperature, rainfall, relative humidity and evaporation. Results from the 100-foot mast, erected in September 1966 to establish the boundaries of ground turbulence, were inconclusive and a 200-foot mast was erected. It fell in a heavy ice and wind storm and was not replaced. MacRae used this as another basis of attack on the Mount Kobau site, despite the fact that the same thing had happened at the CARSO site in Chile[68].

By good fortune a dedicated amateur astronomer, E.L. Pfannenschmidt, became available to carry on regular site testing. A mechanical engineer, he had been a co-founder of the Friends of Astronomy in Germany; on emigrating to Vancouver he joined the local Centre of the Royal Astronomical Society of Canada and became co-director of its telescope committee. He visited the mountain late in 1966 with a group from the Centre, was fascinated with its potential, and happily accepted a position on the Observatory staff. When the camp had been established he took up permanent residence and began a long series of climatological and site-testing observations.

There are two points which may usefully be made here. The first concerns the northern latitude of Mount Kobau. Because of this, the number of viewing hours in the summer are very much fewer than in the winter. In the research here described the viewing period is taken as beginning 1/2 hours after sunset and ending 1/2 hours before sunrise. At the height of summer this leaves about six hours. On the other hand, during the depth of winter the viewing period is almost twice as long. The viewing hours could be extended by using the coudé focus during twilight hours; this had proved feasible on the 48-inch telescope at Victoria. Use of the telescope could be increased still more if the coudé spectrograph were used in twilight hours for infrared spectroscopy[69].

The second point concerns the viewing requirements for the different applications of the telescope. Spectrographic plates are exposed over an extended period of time, often several hours. If a cloud should pass over during this period the observation isn't lost; it is only necessary to extend the observation appropriately. For photometric measurements an extended period of completely clear sky is needed, and the same is true for photographic observations. We saw that, in planning the design of the telescope, provision was made for changing from a spectroscopic mode at the coudé focus to a photometric mode at the Cassegrain focus with a minimum delay, in cases where the weather improved during the night. This is of limited value; one would not likely have a photometrist and a spectroscopist standing by all night waiting for a possible weather change.

Beginning in July 1967 cloud cover was measured, visually by Pfannenschmidt, and instrumentally with an automatic all-sky camera. The camera was lent to the project, and the films were analyzed, by the Upper Atmosphere Research Section of the NRC, through the courtesy of P.M. Millman. Having regard to the limitations outlined above, there are 3190 night-time hours per year at the latitude of Mount Kobau. For a 39-month period beginning in July 1967 the yearly average of usable observing time was 1363 hours, on 221 nights, 123 of which were totally clear. This value was almost exactly in the middle of the range Odgers had suggested. Rather stringent conditions were imposed; for example to be regarded as usable time the sky had to be clear to within ten degrees of the horizon, and there had to be at least two continuous hours of such conditions. To be rated as of photometric quality, five consecutive hours of sky clear to within five degrees of the horizon and of good transparency was demanded. By this definition 106 of the 221 nights, or 29%, were photometric.

We saw in Chapter VII that a simultaneous four-channel photometer was developed at Victoria for use on Mount Kobau[70]. This instrument was operated during the summer months of 1968, 1969 and 1970. Criteria of good photometric viewing were developed, based on the actual observational results. The agreement with the visual evaluation described above was reasonably good; the visual observations were actually more demanding than the instrumental ones.

Even when the sky is clear the image of a star in the telescope may differ in size, shape or steadiness from its theoretical value. This poor "seeing", is caused by the turbulent mixing of air volumes of different temperatures with resulting refractive inhomogeneities in the atmosphere. While the cause of poor seeing was qualitatively understood at the time that Pfannenschmidt began his work, there was not good agreement among astronomers on the best way to measure the effects quantitatively.

E. Brosterhus, who had been involved with site testing in Europe, and who was carrying on similar work in Ottawa, was placed in charge of site testing on Mount Kobau in March 1967 and made frequent visits to the mountain. The researches which he supervised broke new ground in site testing[71].

A plane monochromatic wave from a star, after passing through a disturbed atmosphere, will emerge with a slightly distorted wavefront. The light rays, which are perpendicular to the wave front, will enter the telescope at all possible small inclinations to the principal ray. The result of this is predominately determined by the aperture of the telescope. For a large telescope the random direction of the rays is averaged over a large area and therefore approximates the direction of the principal ray. The image will be diffused, but stationary, and the image size is a direct measure of the seeing.

A telescope of small aperture on the other hand cannot average the ray directions; each bundle of rays produces a sharp image which moves about in an erratic way to trace out a circular disk of the same diameter as the diffused image seen in the large telescope. It is thus a good measure of the seeing to be expected in the large telescope; Petrie and Odgers were using the appropriate instrument in their initial field tests. Telescopes of intermediate size, on the other hand, show a combination of the two effects, a fuzzy disk-like appearance which changes constantly.

There is a second effect of atmospheric turbulence, the variation in the light from the star owing to the changing energy distribution in the wave front. This is called "scintillation". It is not regarded as of great importance in evaluating a site since its effect can be eliminated by observing over a length of time sufficient to average out the variations.

When Pfannenschmidt began his observations in mid-1967 he used the Questar telescope to measure the image motion of Polaris. The telescope was not driven, but Polaris tracked across the field of view very slowly. A reticule wire in the focal plane of the telescope was adjusted parallel to the star track and image motion was measured in terms of the width of this wire. This allowed a continuing estimate.

Another way of measuring image motion is to photograph the star trails over a period of time, usually two minutes. This was done using the 16-inch telescope when it became available in October 1968. The trails show clearly the variations in motion and in brightness of the star image. The values of image motion obtained in this way were smaller than those obtained by simultaneous visual or photometric observations. This is believed to be due to the larger aperture telescope; the fact that there is appreciable image motion suggests that a 16-inch telescope is not "large".

In 1967 a photoelectric seeing monitor was installed on Mount Kobau. This was designed in Ottawa by C.L. Morbey and Brosterhus, and was built in the Observatory shop. A six-inch Cassegrain telescope focuses the star image on a reticule marked with ten identical cycles of lines, each consisting of six opaque and six transparent lines of different widths. The telescope is pointed at Polaris and held motionless there. The diurnal motions of the star cause its image to move across the field of the telescope, and hence across the various lines on the reticule. Typical records are shown in the samples reproduced on the next page, in which the upper line of each set of figures is made by the direct image of the star, the lower one by the image as it moves behind the three widest slits. If the image were a perfect point it would be cut off instantaneously as it moved behind an opaque line and would reappear instantly as it moved out of the opaque area. The "wings" of the image are therefore a measure of the diameter of the stellar image. The high frequency oscillation is due to scintillation.

The agreement between Pfannenschmidt's observations and those produced by the image monitor was remarkable, except for periods of very high fluctuations, which were not of much astronomical interest. The image motion over the 39 months had a median value of 1.2 seconds of arc, a mean value of 1.5. These are very close to the values obtained by Odgers. The claims made for Mount Kobau, both as to the number of clear hours and of the quality of seeing, were substantiated.

Water Supply

E.C. Halstead made additional studies[72] of the potential water supply on Mount Kobau, this time of ground water. Five observation wells, ranging in depth from 60 to 270 feet, were drilled in the vicinity of the peak during the summer of 1967, and a 150-foot production well was drilled at the top of Mount Kobau in 1968. A 47 hour production test of this well yielded water at a rate of 7.5 imperial gallons per minute. This would provide a good auxiliary source of water.

Telescope Design

The delay which followed the submission of the Phase I feasibility study made for serious problems for the engineering consultants. They had assembled highly competent design teams, both on the west coast and in Toronto, and could not afford to maintain them without a contract. If they were assigned to other work it would be difficult to reassemble the teams.

To meet this problem Treasury Board[73], on February 29, 1968, approved a contract "to replan and reschedule the design of the Queen Elizabeth II telescope (telescope, controls, enclosure and dome) to meet revised completion date that results from an imposed funding schedule: This will be known as Phase II." The contract provided $1000 per day, to a maximum of $40,000. The consultants final report was submitted in April 1969[74].

The Optical Shop

We saw earlier that in early May 1966 the Board of Governors of the University of British Columbia approved the assignment of five acres as a site for the Institute headquarters and the optical shop, subject to approval of the final design. Treasury Board authorized[75] the production of the designs and the procurement of the equipment for the shops at about the same time. The plans were produced and accepted by the Board of Governors, and an announcement to this effect was made at a press conference in mid-December 1966[76]. Later it was realized that the designs did not provide adequate temperature control. New plans were prepared, providing the outer tower with double walls filled with four inches of insulation[77].

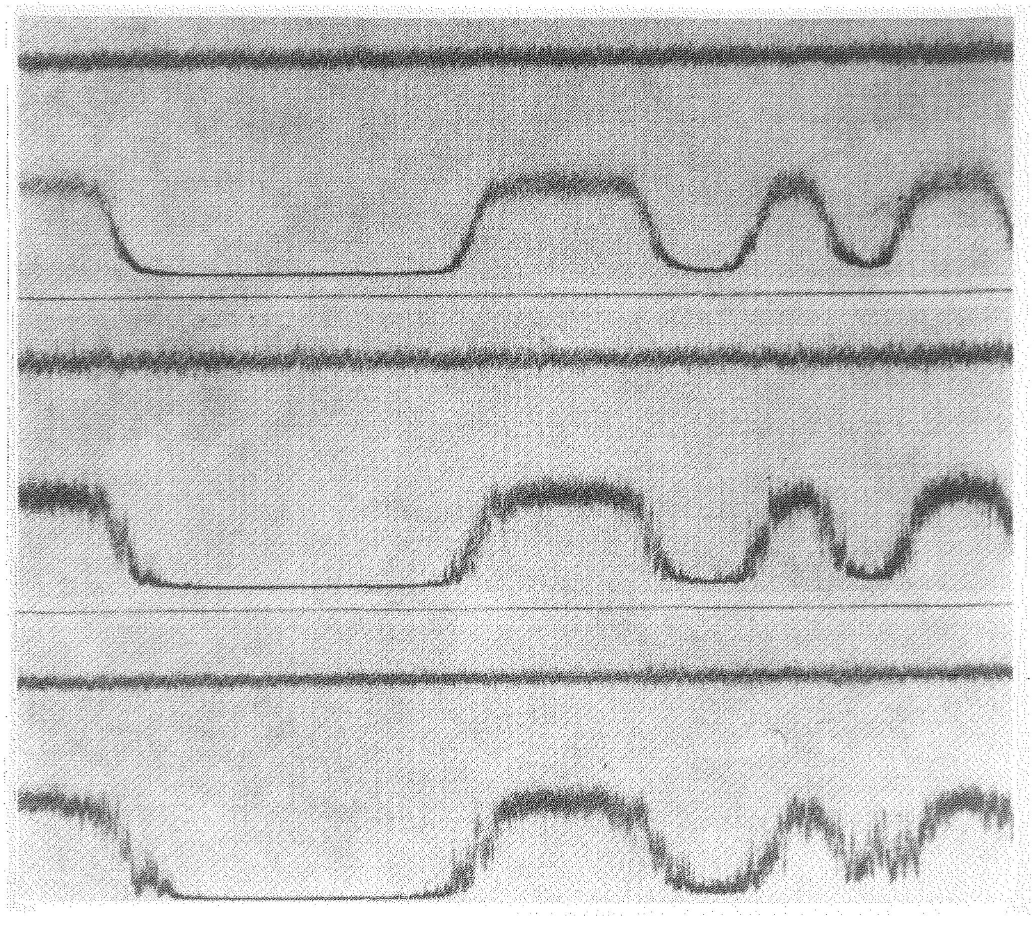
Beginning in October 1966 and continuing through March 1967 orders were placed for the equipment needed for the machine shop – the lathes, milling machine and other similar tools, as well as work benches. As these materials were delivered they were placed in storage with a commercial storage firm in Vancouver[78].

Three polishing machines were needed, of 40-inch, 60-inch and 150-inch capacity. Orders for the two smaller machines were placed with an American firm, Optics for Industry, of Milwaukee, at a total cost of $46,000 US[79]. The large machine could not be bought on an "off the shelf" basis. An agreement was reached with the AURA group to share the costs of designing the polishing machine for the primary mirror. A complete set of drawings was obtained at a cost of $4870[80], tenders were called, and an order for the machine was placed with the Canadian Aviation Electronics Machinery Limited (CAE), of Vancouver, in June 1967 at a cost $188,051[81].

In the process of grinding the primary mirror, the first operation would be to produce a spherical profile. In order to test this, a 100-inch spherical cast aluminum form, known as a Hindlesphere, was needed. A contract to produce the casting, using the AURA pattern, was made with the Aluminum Company of America at a cost of $14,795 US[82]; a contract for the rough grinding of the form was let to Allis Chalmers of Milwaukee for $5000.

This progress was not matched with progress on the optical shop itself. The request to enter into tender for its construction was with Treasury Board for several months[83], and toward the end of 1967 it was becoming increasingly clear that the entire project was under review. This was confirmed on December 7, by the announcement by Finance Minister Benson, already described, of the "stretch-out" of the Queen Elizabeth II project and the postponement of the optical shop.

The urgency for proceeding with the grinding of the mirror did not go away. The mirror itself, with a final diameter of 157 inches (next page), was completed examined and accepted by the end of 1967 and it was necessary to arrange with Corning to store the finished blank[84]. The machine shop equipment had arrived, 97 pieces of it, and was in storage. By February 1968 the Hindlesphere had been completed, by June the smaller grinding machines were finished and the large



grinding machine was nearing completion. After testing under full load it would need to be stored. The annual cost of storing all this equipment was expected to reach $25,000. If construction of the optical shop was to be delayed, would the government approve the rental of a temporary one? The coarse grinding of the Hindlesphere and of the primary mirror, would take two years, and would not require a controlled environment.

The contract for a temporary shop was never approved, and all the equipment had to remain in storage. As the costs mounted, an agreement was reached with the University to store the blank and the large grinding machine on the campus. The grinding machine had to be dismantled for the move and reassembled after it, and the mirror blank, before shipping from Corning, had to be encased in bullet-proof steel. Odgers wrote in defence of this expenditure[85]:

> "It seems ridiculous to have to protect a large mirror against rifle shots but we are advised that such protection is necessary and is provided for mirrors in transit through the United States."

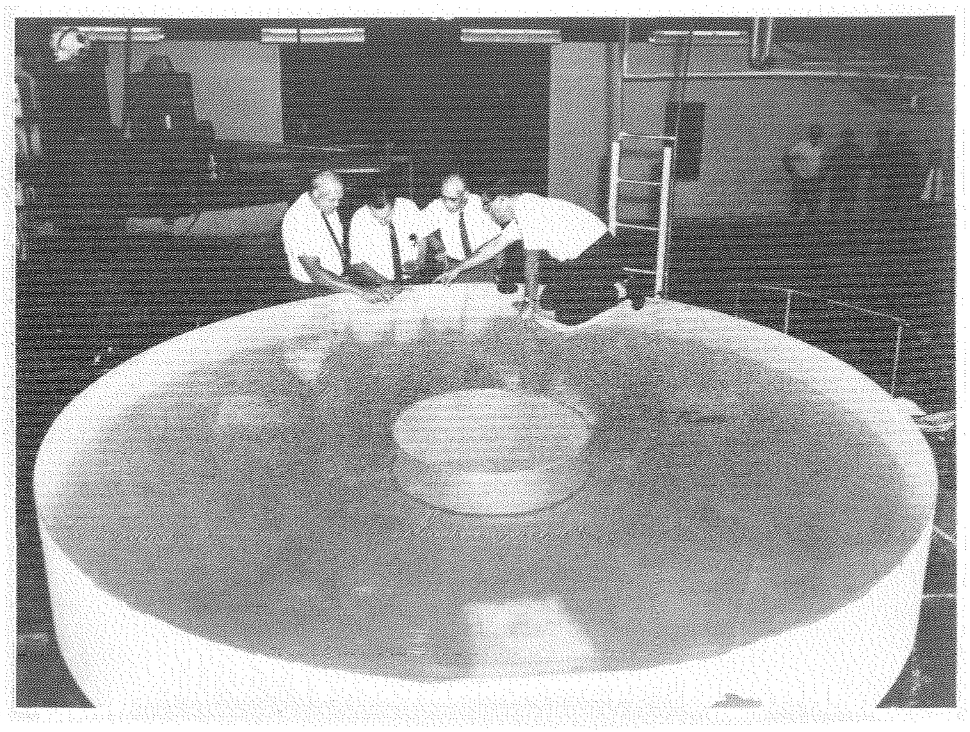
The Working Group on Astronomy

Meanwhile the controversy over the telescope continued; something had to be done to resolve the impasse. The Department, and NRC asked the Science Secretariat to step in. It set up a small Working Group[86] of senior scientists to consider the relative merits of the Queen Elizabeth Telescope and the southern hemisphere proposal, from the point of view of scientific excellence and in the context of the total effort devoted to astronomy in Canada. As time permitted they were also to consider the appropriate allocation of resources to astronomy in relation to other fields of scientific research and to consider the recommendation of the Glassco Commission that all government astronomy should be combined under one agency.

The establishment of the Working Group coincided with major changes in the Government. Pearson retired in April 1968 to be replaced as Prime Minister by Pierre Elliott Trudeau. He called an election in June, and was returned with a majority. Jean-Luc Pepin, who had been Minister of our Department since December 1965 continued in that position until after the election. He was succeeded by J.J. Greene on July 6. The Working Group, set up in early June when Pepin was minister, reported in mid-August to Greene.

The Group was chaired by D.C. Rose, recently retired Associate Director of the Division of Pure Physics, NRC. C.S. Beals and W.H. Wehlau, head of astronomy at the University of Western Ontario, were the other members; D.I.R. Low, an officer of the Science Secretariat, acted as secretary. They travelled across Canada during the first half of July 1968 holding meetings in, in order, Toronto, Vancouver, Victoria, Penticton, Ottawa and Quebec. Fifty-five briefs were presented, and Dr. H.A. Babcock met the Group at the conclusion of their tour to supply details about the CARSO proposal.

Harrison[87] was present at the Ottawa meeting and was "startled to hear Dr. MacRae state categorically that the Mount Kobau site would be of no use to eastern astronomers.



Roy Dancey, Chief Optician, second from left, examines the finished mirror blank. The diameter is 157 inches.

... There were no more than 1400 observing hours to be expected at Mount Kobau. Each graduate student required 200 to 300 hours observing time. If half the time were available for universities there would be no chance for graduate students to make proper use of the facility."

These figures were something new. Harrison made some "horseback calculations": considering the capital and operating costs of the CARSO telescope, the cost per student would be about $150,000 per year. "If Dr. MacRae's figures are correct I am sure that many eyebrows will be raised at the cost of astronomy and the advisability of investing that much for graduate research in a foreign country."

Did the CARSO offer provide the best opportunity for Canadian astronomy? Our Departmental attitude was well expressed by Harrison in an internal memorandum[88] to the Deputy Minister.

> "It seems unreasonable to me that Canada can be an equal partner in the Carso project, regardless of organization, unless her astronomers have the capability to counterbalance U.S. astronomers. Our geological scientists, for example, exert a strong influence in international sciences because [they] are among the best trained and equipped in the world. I cannot imagine that Canada, without a first-class telescope and opportunity to develop its expertise at home, will ever have a large voice in international astronomy or that it could ever become anything but an appendage to U.S. capability. ... I would, therefore, be reluctant to see Canada participate in the CARSO project without a major installation in Canada."

Shortly before the Working Group was to present its report, some disquieting information on the reason for the CARSO approach to Canada[89], came to light. They had hoped to get a major grant from the Ford Foundation but insisted that the telescope should be used exclusively by highly qualified scientists, and not by graduate students. This elitist attitude was not acceptable to the Foundation, the grant was refused, and the approach was made to Canada. It may have been true that Canadian astronomers would have unrestricted access to half the observing time on the CARSO telescope but would graduate students be welcomed? This information was passed to the Working Group, but was not made public.

Beals had begun the operation convinced that the Queen Elizabeth Telescope should be built on Mount Kobau, but he surprised his old colleagues in Victoria by proposing that it should, instead, be built in Chile. Odgers reported[90] that, "in making this proposal he referred to the Toronto group as being 'very persuasive with politicians' and he was really telling us that we *had* to agree with them".

This inference is borne out by subsequent correspondence[91]. In a letter to Wright, Beals recalls the atmosphere:

> "There were deep and irreconcilable divisions within the Committee and I wished many times that I was on some other planet. In the end it came to the point where each had to give up something or end in complete indecision that seemed certain to be fatal. I refused to accept any solution that did not involve the use of the 157-inch mirror, the optical shop and the experience of the group including Odgers, Richardson, Dancey & Co. that had been assembled to do the Canadian telescope. The other side refused to accept any solution that did not involve a Chilean or comparable site."

In the end the Working Group proposed[92], as its first recommendation, that "a wholly Canadian owned telescope, using the 157-inch mirror blank already made for the Mount Kobau telescope, be built on a suitable site in ... Chile". A site for the telescope was to be negotiated with the AURA group. "If negotiations for such a site are unproductive, our alternative recommendation would be to join with the Carnegie Institution in their project to build a 200-inch telescope plus two smaller telescopes [in Chile] and to complete the construction of the 157-inch telescope on Mount Kobau."

The report was considered by the Cabinet Committee on Priorities and Planning, chaired by the Prime Minister. It found the recommendations unsatisfactory. To build the Canadian telescope in Chile would cost a great deal more money than to build it in Canada, and would be politically unacceptable; to build it in Canada and to buy in to the CARSO telescope as well was simply too expensive. The decision was made to recommend to Cabinet that the Queen Elizabeth telescope be cancelled.

Here a mistake was made that had serious results. Instead of waiting until the full Cabinet had endorsed the Committee's recommendation, someone instructed the Department to announce the cancellation immediately. This was contrary to Privy Council instructions. Moreover the details of the decision were garbled in transmission to us. Our press release was not only premature, it was incorrect. Its gist: for reasons of economy all work would stop on the development of the telescope, and an effort would be made to dispose of the mirror blank and the polishing machines purchased at a cost of $1,500,000. Further, the government did not intend to proceed with the alternatives recommended by the Working Group. However, "because it is the best viewing site in Canada", Mount Kobau would be maintained on a continuing basis for universities for the conduct of astronomical research."

This press release, which implied that the government was abandoning "large-telescope" astronomy entirely, was a very damning statement, and quite untrue. To quote again from a Privy Council document[93]: "The Queen Elizabeth II Project was cancelled on the basis of scientific advice against proceeding with it, and on the basis of budgetary exigencies. ... [There was] a clear undertaking by the Government to examine alternatives to the Queen Elizabeth II Project: at the very least, those alternatives outlined in the Working Group's report."

There were demands in the press and in the House that the report of the Working Group be released. This could not be done. Until the Government had formulated a policy on astronomy, which was made difficult by the press release, the report must remain unpublished.

Reflections on the Cancellation

It may be useful, twenty years after the cancellation, to consider what went wrong. A first question has to be – why did communications between the Privy Council Office and the Department break down so completely? How did the misunderstanding about the press release arise, who ordered the Department to issue it prematurely and who conveyed the entirely erroneous details of the decision? Why were the university attacks on the Queen Elizabeth telescope not referred to the Department for comment? The Privy Council files shed some light[93].

Our Deputy Minister wrote to R.G. Robertson, Clerk of the Privy Council, on October 4, 1988, complaining about the lack of communication on astronomy. Robertson replied:

> "I was surprised and rather dismayed to learn from your letter of October 4th that you feel that consultation on astronomy between the Privy Council Office and your department has been unsatisfactory. My reaction was mitigated somewhat, however, because I do not believes that there is much ground for concern on that score"
>
> . . .
>
> "As regards exchanges of correspondence on astronomy over the last two and a half years, our files indicate that very few letters were addressed to either Mr. Pearson or Mr. Trudeau, and that you or your minister have copies of all of them, with the possible exception of a letter received last fall, to which a simple acknowledgement of receipt was sent by Mr. Pearson.
>
> I certainly agree with your suggestion that your department could not be held accountable for the formulation or implementation of policy, if it did not have access to the relevant papers. I can assure you that no such papers have been denied to your department."

So much for the swarms of letters which were "deluging the Prime Minister's office".

In his reply Isbister became more explicit.

> "For your information merely, your letter of October 8 surprised me by saying that copies of all the letters addressed to Mr. Trudeau or Mr. Pearson have been supplied to my Minister or to the Department. As far as this Department is concerned, we have been informed by others but not by your office of letters sent to Mr. Pearson from University of Toronto faculty, from the Chairman of the Science Council and from a U.S. Astronomer. Perhaps all of these and others were sent along to my former Minister. If so, I agree that this is my problem, not yours".

I cannot find that the problem was ever solved. Whatever caused it, it was a great pity. Had we been able to state our position, we might have changed the course of events. At least it would have reduced the bitterness between the two sides. One must also wonder why, if the Privy Council was convinced that the Mount Kobau site was a poor one, Departmental officials were not reprimanded for their bad judgement.

More importantly, how did the disagreement between government and university astronomers come to exist, and why did it become so bitter? The fundamental reason was that we believed that the telescope had to be built in Canada or not at all. We understood that Beals had been assured of this by senior officials of Treasury Board. I wonder if it was true? I have not found any reference in the Observatory files or in Beals' personal papers to support it. Van Steenburgh insisted on a Canadian telescope, but perhaps he was wrong.

A second reason for the failure is that we moved too fast. If it had taken us a few years instead of a few months to obtain approval for the telescope, the climate and the seeing on Mount Kobau would have been thoroughly understood and its inferiority to other sites recognized. A telescope in Chile might have been supported; there were some very enlightened men in the Pearson cabinet. If not, the university astronomers would have had to accept the fact. As it was, by the time they made their attack there was too much invested, both in money and in commitment, for government astronomers to back down. And always, the spectre of losing everything hung over us. We didn't want to lose a telescope with a potential 40 times that of Victoria's 72-inch!

Finally, the telescope had been approved before there was an effective mechanism in government for the consideration of such projects. The Science Secretariat had just been set up, and the Science Council of Canada was two years in the future. If these organizations had been in effective operation they would have insisted on a more careful examination of the site; they would, as Harrison did later, have calculated the cost per graduate student and compared the telescope's merits with other large-budget science projects.

Would the Toronto astronomers have mounted their attack if Beals had still been Dominion Astronomer and if Petrie had not died? No one can say, but I wish they had been there to fight the battle!

The Post-Cancellation Period

The Government was still considering the report of the Working Group, which suggests that they saw the CARSO offer as providing a good opportunity for Canadian astronomy. But what were the merits of astronomy's needs in relation to those of other pure sciences? The question was referred to the Science Council, which turned it over to its committee on Physics and Chemistry, chaired by H.E. Petch, of the University of Waterloo. The committee was to undertake "a broad general study of the place of astronomy in modern science, of Canada's role in astronomy and of how any activity in the field of astronomy would best be distributed between government, universities and industry[94]". The Committee was to report by September 1, 1969.

Many letters reached the Privy Council office after the cancellation. A large number of these were engineered by two graduate students at Ann Arbor, Michigan, using the membership list of the American Astronomical Society. Others were the result of a campaign by local Centres of the Royal Astronomical Society. There were in addition many unsolicited letters from astronomers, in Canada and from around the world, deploring the decision, particularly the decision to dispose of the assets. As one said[95]:

> "The recent decision to sell the assets of the Queen Elizabeth II telescope, including the 157-inch blank and the optical shop equipment, is an unprecedented piece of butchery. All large telescope projects have had financial troubles but were able to survive lacking anyone callous enough to sell them out."

Contrary to what had happened earlier, all of these letters were forwarded to the Department, and we were required to defend a decision in which we had not concurred.

Almost immediately astronomers from a group of western universities banded together in an effort to continue the Queen Elizabeth telescope without Federal support. They hoped to reduce the costs substantially by eliminating all "frills", and to find the money from provincial governments and commercial sources. The founding members of the group were the Universities of British Columbia, Victoria, Notre Dame (at Nelson), Calgary, Alberta and Lethbridge. On December 11, 1968, the acting President of the University of British Columbia, W.H. Gage, made a formal request[96] that the assets should be turned over to the university consortium. The response[97] was a temporizing one; the government could not make a decision at this time, but would sequester the assets pending a decision.

The decision, when it came, was favourable[98]. If the consortium would establish itself as a legal entity, and give assurance that it could raise the necessary funds, the Government would turn over to it, outright, the assets, plans and designs and the design and optical teams of the Dominion Astrophysical Observatory would assist the consortium in the completion of the telescope; in the event that the consortium was unable to complete the telescope it might sell or otherwise dispose of the assets, provided that the money so realized was used in the development of other astronomical facilities on Mount Kobau. A press release to this effect was issued on April 24, 1969.

In compliance with the terms of the agreement, the consortium of western universities was formally established with the acronym WESTAR, with Professor Brian Wilson, Dean of Arts and Science, University of Calgary, as Chairman. The University of British Columbia become the "legal entity" which would act on its behalf[99]. The arrangement seemed secure. But no; the consortium was not able to "give assurance that it could raise the necessary funds". Naturally it could not; until it was assured of the assets it could not canvas for money!

This "Catch 22" situation led to further delay. It raised the question of government responsibility in the event that the consortium failed to raise the money. The only assets that it owned would be those given it by the Government; was the arrangement whereby the University of British Columbia assumed the financial responsibility for the consortium legally sound? A lot of time was spent while the lawyers straightened this out, and in the meantime other factors had entered.

The most important of these related to the use of the large polishing machine. With the publication of the Phase I report, word spread quickly about the excellent telescope design, particularly of the interchangeable ends and of the novel mirror support system. The AURA group retained the firm of Dilworth, Secord, Meagher and Associates to assist in design changes for the 150-inch telescope on Kitt Peak and for a sister telescope being planned for Chile. The firm was also retained to do a complete redesign of the Kitt Peak concept for an Anglo-Australian telescope in Australia[100]. There were other possibilities, the CARSO telescope, a proposed 150-inch telescope in Saudi Arabia and numerous smaller instruments. Why should the Associates not form a consortium to bid on these telescopes?

They did so, being joined in it by Canadian Westinghouse, which had the expertise and facilities necessary for the heavy manufacture, and by Owens-Illinois, which already had a contract to supply the mirror blank for the Australian telescope, and had an excellent optical shop in which to polish it. As a first step the consortium would bid on the Anglo-Australian telescope. The only thing they lacked was a large polishing machine, and they applied to the Government[101], through the Department of Industry, to lease our machine. An early decision was needed; the deadline for bids was rapidly approaching, and unless a polishing machine was assured they could not bid. The Department of Industry urged that the request be accepted, thus placing itself in direct confrontation with the Department of Energy, Mines and Resources, which had agreed to sequester the assets for the WESTAR consortium. As a further complication AURA[102] applied to lease the machine to do their own grinding on their mirrors. All of these possibilities would involve transporting the machine to the United States.

Any such solution was inconsistent with our undertaking to WESTAR, which would need the machine to polish the Canadian mirror if they were successful in raising the necessary funds; furthermore their chance of raising those funds would be seriously reduced if the machine were sent out of the country for two or three years. An ideal solution would have been to construct the optical shop as planned and to contract the grinding of one or more of these mirrors while attempting to raise funds. There was a substantial body of support for doing this at government expense[103]. The commercial consortium would have none of it; it wanted to do its own work on its own terms[104].

There was an additional complication. If the DAO was not to have access to a new telescope, at least its existing equipment could be improved. Provision was made in the Observatory's 1969-1970 estimates for the purchase of a new mirror blank for the 72-inch telescope and for the construction of an optical shop in which to figure it. The smaller grinding machines and the machine shop equipment were transferred to the DAO to be used in this shop. Because the Observatory shop would be used in the provision of the WESTAR telescope, this did not constitute a change in that agreement.

THE END OF THE DOMINION OBSERVATORIES BRANCH

The Petch Committee quietly familiarized itself with the status of astronomy in Canada. It convened a series of meetings in Waterloo on July 9 and 10; university astronomers from the east, university astronomers from the west, and government astronomers appeared in separate sessions.

The committee report was submitted, on schedule, by the end of September, 1969. Its recommendations were unambiguous:

1. Canada should join the CARSO project, sharing expenses, responsibilities and observing time on a 50-50 basis; the Canadian contribution, estimated at $12,000,00 should be paid in 10 annual instalments and should consist principally of expenditures in Canada or in support of Canadian experts working in Chile; Canadian industry would share in the design and construction of the telescope to the maximum possible extent.

2. The optical shop should be built on the campus of the University of British Columbia and should remain the property of Canada, not of the CARSO project; its first responsibility should be to the 200-inch CARSO mirror; after that it might be used to grind the WESTAR mirror and to contract for other mirrors.

3. Government activities in astronomy should be centralized in the National Research Council.

These recommendations were accepted by the Cabinet Committee on Science Policy on October 31, 1969, and confirmed by the Cabinet on November 13. It was stipulated that the transfer of astronomy to the NRC should take effect on April 1, 1970, and that the areas of overlap with geophysics should be worked out in consultation with the NRC.

There was substantial opposition from western Members of the Liberal caucus to participation in the CARSO project; if Canada couldn't afford to complete the Queen Elizabeth II telescope in Western Canada, how could it afford to support a telescope in Chile? For this reason announcement of the decision was withheld pending a final agreement on the transfer of the Queen Elizabeth II assets to WESTAR. It was thought that a positive announcement on the latter would reduce the caucus objections to the former. It was not until April 1970 that the legal problems had been ironed out and that the joint announcement could be made: the assets would be transferred to WESTAR and NRC was instructed to negotiate an arrangement with CARSO, under carefully defined conditions. This decision having been made, the reports of the Working Group and of the Petch Committee were published together under the general title "Canada's Future in Astronomy[92].

The Honourable J.J. Greene made the formal presentation of the 157-inch mirror blank, the Hindlesphere and the large polishing machine to WESTAR in a ceremony at the University of British Columbia, on June 12, 1970.

The long delay in reaching the decision was unfortunate. By 1970, western resentment about the cancellation of the telescope, which WESTAR had expected to lead to generous support of its aims, had abated and the consortium was not able to raise any part of the necessary funds.

The Cabinet decision on the transfer of astronomy to the NRC, reached on November 13, 1969, was to take effect on April 1, 1970; we had a little over four months in which to make the necessary decisions. The first question was, what should be transferred? Clearly the DAO and the DRAO would transfer, but what about the work in Ottawa?

First, Positional Astronomy and the Time Service. As we saw in Chapter VIII, the caesium atom had replaced the rotating earth as the basis for correct time, and the NRC had replaced the Dominion Observatory as its source; the Time Service clearly should transfer. The vagaries of the earth's rotation, as defined by the Photographic Zenith Telescopes, were now a matter for geophysics; this work was retained as a Geodynamics Section within the Seismological Division.

The question of Meteor Research was a more difficult one. The observatories at Meanook and Newbrook was clearly astronomical, and should transfer; on the other hand the study of meteorite craters was a matter for the earth sciences and should remain. The MORP network was a problem; its techniques were astronomical, but Ian Halliday, who was responsible for the network, was also involved in the crater program. There was another complication: any meteorites which the network recovered would go to the National collection, maintained by the Geological Survey. An ideal solution would have been for the MORP network to be retained, and for the National collection to be transferred to us. When the Survey declined to accept this proposal, it was agreed, with Halliday's concurrence, that MORP, and he, should transfer to the NRC.

Solar Physics is a discipline bridging astronomy and geophysics. Because the sun is the only star whose surface can be observed in any detail, its study is important to astronomy, but because of the effects of solar processes on the earth it plays a fundamental role in geophysics. This complexity has made for organizational problems, both in international unions, and in the government. Half the resources of our Geomagnetic Division were committed to studies of geomagnetic variations originating in the sun, and both the NRC and the Defence Research Board had a variety of groups studying solar effects. We suggested that a study be made about the possibility of consolidating these many endeavours in a single organization. Because of the major engineering support that the group would need it was recognized that we could not provide a home for such a group. Reluctantly, and with Gaizauskas' concurrence, we agreed to his transfer.

During the last year substantial financial support was given to astronomy. The MORP network and the Solar Spar were completed, a new mirror blank was purchased for the Victoria telescope and an optical shop was built to polish it. Work on the synthesis radio telescope was begun. Much of this work was made possible by transfer of funds from the geophysics divisions. Since the proportion of the Observatory budget to be transferred to the NRC would be based on the distribution of funds in the 1969-1970 fiscal year, this transfer had to be done with care. It was a matter of great satisfaction to me that we sent our children out into the new world well provided for.

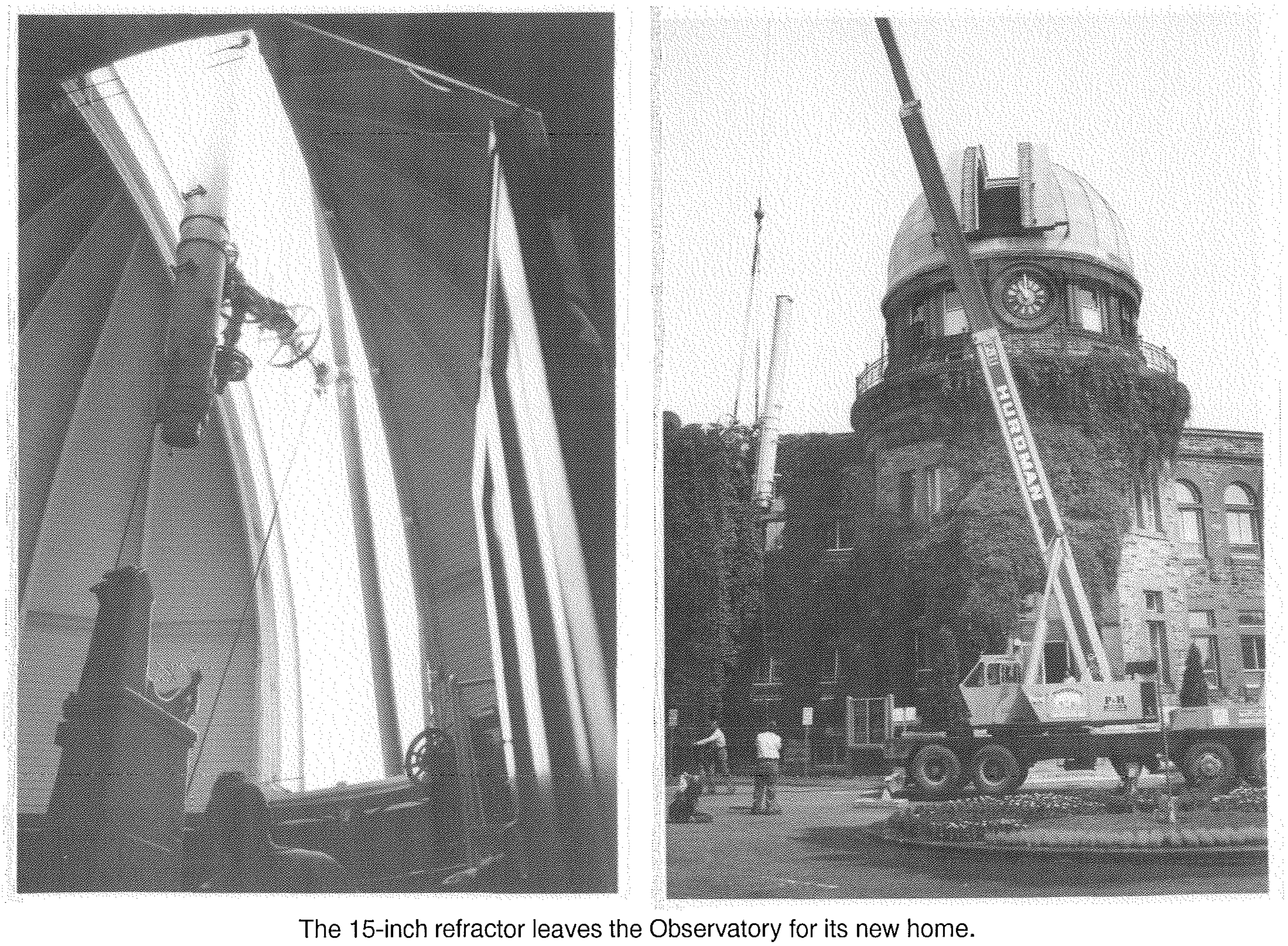
One problem remained. What would be done with the 15-inch equatorial telescope? As we saw in Chapter 6 the public use of this telescope increased over the years to the point that an astronomer, Mary Grey, had been made responsible for the program on a full-time basis. Could this service be taken over by the Museum of Science and Technology? To go a step further, could the Museum take over the Dominion Observatory building as a branch museum dedicated to astronomical and allied displays? A.E. Covington, our colleague in solar radio-astronomy, thought it could be, and enlisted the support of Heritage Ottawa in a campaign to "save" the telescope and the building.

D.M. Baird, Director of the Museum of Science and Technology, was enthusiastic about continuing the astronomical program, but he was not interested in having a museum of astronomy separate from the central museum. Instead a new building, with dome, would be constructed on the Museum grounds and the telescope would be moved, refurbished, and installed in this new home. Until the building was ready the existing observing program would be continued at the Museum's expense. Mary Grey was eventually transferred to the Museum staff, and supervised the program.

The telescope was moved in July 1974 and the installation was completed by May 1975. Since the telescope was once again in use Heritage Ottawa and Covington were content on that score. They had also been worried about the building but it has survived unchanged.

And what should the remaining organization be called? We proposed "Geophysical Sciences Branch" or "Geophysical Branch", but Harrison objected that geophysics had a much wider connotation than the proposed scope of our Branch. This would have been less true if the suggested consolidation of all solar studies in the Branch had been accepted. Harrison suggested "Earth Physics Branch", translated into French as "Direction de la Physique du Globe", and this was adopted[105].

It was Harrison's idea that farewell parties should be held, in Victoria and in Ottawa, to mark the transfer. The reception in Victoria was held in a downtown hotel. Harrison was not able to attend the party and I acted as host. Everyone on the Observatory staff, as well many others with close connections to the Observatory, were invited, and we all had a fine time.



The 15-inch refractor leaves the Observatory for its new home.

A few days later a similar party, at which Harrison could be present, was held in Ottawa. The astronomers were well launched in their new orbit!

This was not, for me, the end of the Dominion Observatory. That moment came at 1 P.M. Eastern Standard Time on April 1, 1970, when the CBC announced "*The National Research Council* official time signals: the beginning of the long dash - - - ."

. . . . .

Victorian writers frequently supplied an addendum to their novels in which the subsequent history of their characters was outlined for the edification of their readers. Our story, which would otherwise have an unhappy ending, calls out for such a feature[106].

Our astronomers were initially amalgamated into an Astrophysics Branch of the Radio and Electrical Engineering Division, NRC. In 1975 there was a further reorganization into the Herzberg Institute of Astrophysics under the direction of our erstwhile colleague J.L. Locke. The National Committee of the IAU was transferred to the Council and established as an Associate Committee on Astronomy.

I have not been able to find any evidence that the NRC moved on the Petch Committee recommendation with respect to the CARSO offer. Other exciting possibilities were developing. Shortly after the cancellation of the Queen Elizabeth telescope Graham Odgers had been granted sabbatical leave, to be spent in France. There he met a group of astronomers, led by Charles Fehrenbach and Roger Cayrel, who were planning for a large telescope in the northern hemisphere and were seeking a partner. The triumvirate approached the National Research Councils of France and Canada with the proposal that the two countries should join in the construction of a 3.6 m (142-inch) telescope on Mauna Kea, on the island of Hawaii. The proposal was ultimately accepted; the University of Hawaii became a partner in the project, supplying and maintaining the site and the approaches to it. The Canada France Hawaii Telescope (CFHT) Corporation was set up in 1974. Cayrel was named Project Director, with Odgers as Associate Director. Construction costs were divided equally between Canada and France; observing time is divided between the three partners C/F/H in the ratios 42.5:42.5:15.

France was responsible for the telescope, its drive gears and most of its instrumentation; Canada would polish the mirrors, supply the dome, the telescope drive and control system, the workshop equipment and some of the instrumentation. A CerVit mirror, already purchased by France, would be used.

The Optical Shop, which we had financed in the final year of the Observatories Branch, now proved its worth. The grinding of the new 72-inch mirror for the Victoria telescope and of the 142-inch for the CFHT went forward in record time under the supervision of Dancey. The opening ceremonies for the new telescope were held on September 29, 1979, although the secondary mirrors had not yet been completed. The telescope was equipped for prime focus, Cassegrain and coudé operation. Because of the more southern latitude the telescope had a yoke, rather than fork, mount, but it did incorporate the exchangeable upper end design of the Queen Elizabeth II telescope.

The observatory, and its telescope, has been a tremendous success. At an elevation of 4200m (13,800′) it enjoys clear sky 85% of the time; of this, 75% is photometric. The diameter of the seeing disk is normally less than one second of arc and for appreciable periods is as small as 0.5 second of arc.

So, there's our happy ending!

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ABBREVIATIONS USED IN THE TEXT

AFOSR – Air Force Office of Scientific Research
AURA – Association of Universities for Research Astronomy
CARSO – Carnegie Southern Telescope
CFHT – Canada France Hawaii Telescope
CSAGI – Comité Spécial de l'Année Géophysique
IASPEI – International Association of Seismology and Physics of the Earth's Interior
IAG – International Association of Geodesy
IAGA – International Association of Geomagnetism and Aeronomy
IAU – International Astronomical Union
IGY – International Geophysical Year
ISC – International Seismological Centre
ISS – International Seismological Service
IUGG – International Union of Geodesy and Geophysics
IUGS – International Union of Geological Sciences
MORP – Meteor Observation and Recovery Program
NRC – National Research Council
PCSP – Polar Continental Shelf Project
PZT – Photographic Zenith Tube
UMP – Upper Mantle Project
USC&GS – United States Coast and Geodetic Survey
WWSSN – World Wide Standard Seismograph Network

THE HEAVENS ABOVE AND THE EARTH BENEATH

VOLUME 2

CORRECTIONS TO FINAL TEXT

Page 7, LH coloumn, Line 10. For "page 8" read "page 5".

Page 8, LH column, Line 1. Delete "committee assuming honorary".

Page 25, LH column, Line 16. For "page 6" read "page 4".

Page 70, RH column, figure caption of small flare. For "page 70" read "page 68".

Page 91, The photograph of the New Quebec crater has been printed upside down.

Page 95, LH column, third paragraph, second line. The subscript 6 on "photographs$_6$" should be a superscript^6 reference number.

Page 102, RH column, third paragraph from bottom, first line. For "the figure on p. 105" read "the figure on p. 103".

Page 191, LH column, Line 6. For "on this page" read "on the facing page".

Page 195, LH column, Line 10. For "below" read "on the facing page".

Page 204, LH column, Lines 6 and 7. For "in the left-hand diagram of the figure below" read "in the figure below".

Page 204, LH column, Line 11. For "as shown in the diagram on the right" read "as shown in the left top diagram on page 205".

Page 205, RH column, Line 11 and 12. For "the figure on the next page (left column) shows" read "The figures at the top of this column and on the next page (left column) show" the same fault-plane solution in the two projections.