G E O T E C H N I Q U E i n C A N A D A

A PERSONAL MEMOIR

by

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SPECIAL NOTE

... to the reader:

This record has been prepared on the basis of entries in my work diary (I have never kept a diary in the usual sense), early records of the Associate Committee, and privileged access for one or two matters to the wartime diaries of Dr. Mac kenzie, but chiefly upon my own memory, reactivated for this special task! It is as fallible as any other memory and so I apologise in advance for any unwitting inaccuracy, while asking all who note any inaccuracy (no matter how detailed) if they would be good enough to let me know of it, so that the final record may be as good as possible.

1 March 1983. R. F. L.

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A P O L O G I A

Appreciation must first be recorded to those responsible for conceiving and developing the idea of preparing a record of the development of Geotechnique in Canada. Having been involved, to a degree, in this interesting chapter in the history of civil engineering in Canada, I should have thought - long ago - of getting at least some of my recollections down on paper. This I have never done, counting myself always too busy looking ahead. I make this admission with regret and no little embarrassment in view of my own interest in, and writing about the history of engineering in this country. So it is that I welcomed cordially the invitation of Jack Clark and David Townsend to get my memories in this field on paper. Once I had seen the need for this (with some help!) I gave much thought to the matter. The more I thought, the more I was driven to the conclusion that the only possible way for me to make a useful contribution would necessitate writing a very personal memoir. Never having done anything like this before, it has not been easy! My friends know that I am "old-fashioned" enough ~~always~~ to write my technical papers always in the third person, so that to have to start writing "I" does not come naturally. But it must be done and so, with embarrassment, here goes.....

Early Training (1922-1929)

Reflecting upon my good fortune in participating in the formal start of Soil Mechanics as a discipline in civil engineering, a very small activity when it did start, caused me to realise how my early training had "conditioned me" for being receptive to any such move in connection with soils. I must, therefore, go back to my professional beginnings if what follows is to make sense.

Through an entirely unexpected scholarship, I was enabled to enter the University of Liverpool as an undergraduate in 1922 (instead of becoming a bank clerk!). Liverpool was then, and still is, one of the leading "Red-brick Universities" of England with fine standards. Because of my schooling, I was permitted to enter the second year at the end of which I elected to take civil engineering, without really knowing too much about it. Civil was a small department, distinguished by a fine lecturer in Structures and by the fact that, in order to take an honours degree, all civil students had to take a special minor-honours course in geology. This included short but good introductions to petrology, palaeontology and geological mapping, all with laboratory instruction as well as lectures, all supplementary to the main course of lectures on Geology for civil engineers by the head of the Department, Professor P. G. H. Boswell, the finest and most inspiring teacher I have ever had.

I still have my lecture notes from that course and they show what an admirable introduction to the subject "P.G.H." gave to us, including due reference to soils. His own first-world-war research had been in connection with sand; he retained his interest in soils

throughout his life. He was the Professor of Geology at Liverpool from 1917 to 1930 when he left to take up the Chair of Geology at Imperial College, London, an indication of his standing in the profession. But he was plagued by ill-health and had to retire in 1938. Cared for by his wife, he spent most of the remainder of his life in nursing homes but even here his active mind was always at work. Towards the end (he died in 1960), he wrote a series of essays, or short notes, on various aspects of soil and these were published (posthumously) as Muddy Sediments, a small book of 140 pages, published by W. Heffer & Sons Ltd., of Cambridge. It is not a very good book, containing some strange and even incorrect ideas. Boswell himself had no illusions about this, saying in his Preface that it was merely "Simple tales for simple souls", but I go into this detail since, to the best of my knowledge, it is the only book of recent decades about soils written by a hard-rock geologist.

The most remarkable thing about geology at Liverpool for civil engineers was that Professor Boswell was able to insist that all civil engineers taking the honours course must take a full week of geological field work in the Easter vacation. In my case, this was spent at the old town of Ledbury near the English-Welsh border. We stayed in an old inn, were out in the field by eight every morning, working until five, returning for dinner and then spending the evening plotting our results for the day. At the time, I remember so well, we "kicked like steers" (as students would) about this "slave-driving" but, looking back, I am quite sure that the week in the field, with Boswell and his two lecturers, was the most profitable week of my University training. We were trained in

the art of observation, by three expert field geologists and this, coupled with earlier training as a Boy Scout (and especially in Kim's Game), has been of inestimable benefit to me ever since. I only wish that there was some way of ensuring that every undergraduate in civil engineering could have similar training, in lecture-hall and in the field. I have always appreciated this training ~~since~~ from soon after graduation but only ~~as~~ since I was forced to look back, in thinking about Geotechnique in Canada, have I realised fully ~~the facts~~ that the seeds planted by Boswell in those impressionable years must have had some influence on what happened later.

Having always been interested in railways (I am still a "buff") I had hoped to get a job with one of the main British railways after graduation. This, however, was at a time of the great amalgamations; the railways had too many civil engineers; so I had to look at other fields. Water power engineering had also attracted me; I found that the largest water power ~~plant~~ project that will ever be built in the United Kingdom (located in the North of Scotland) was then being designed, and so I wrote to the consulting engineers in London. Invited to go for an interview, I made the "terrific" journey to London (all of 200 miles, but I was the first member of my family to wander so far afield) and was interviewed by Mr. W. T. Halcrow, ~~then~~ the head of an old firm of consultants, then known as C. S. Meik and Buchanan. (C. S. Meik had been one of the resident engineers on the building of the Forth Bridge). I got a letter offering me a pupilage with the firm i.e. I would be an assistant engineer for two years, would pay £300 for this privilege, and get it back in monthly payments.... and up to that time, this ~~is~~ was the only way in which one entered the profession of civil engineering in the United Kingdom, even with a degree.

I had to turn this offer down since I had to earn my own living but within a couple of weeks I got another letter, this time offering me a job as assistant engineer at the princely salary of £150 per year (on which I managed to live in London). Later, I found that I was the first such assistant ever to be paid in that office; pupils were still being recruited for some years after that! It was an old-fasioned office in which I learned much from a wise old chief draftsman but my main good fortune was in acting as Mr. Halcrow's "technical office boy" - doing calculations and trial designs to satisfy his thinking. One job stands out; the fact that I can remember it so clearly shows that it must have influenced my own thinking. In order to locate the Lochaber power house, at the end of large steel pipes coming from the Lochaber tunnel (see below), a large programme of test boring was carried out over an area of about half a mile square. Working to Mr.Halcrow's direction, I prepared and roughly costed sixteen separate schemes and saw these being compared, in a fine exercise of engineering economics, until a final selection was made. I have never forgotten the value of that intensive site investigation.

Mr. Halcrow was a quiet, kindly man; he later became Sir William and the same firm is today, as Sir William Halcrow & Partners, the largest firm of consultants in the United Kingdom. I suppose that I must have "made noises" about getting up to the job, now in full construction, since he allowed me to have a six-month period with the Resident Engineer; and, later, a further longer period as activity on the job peaked. The tunnel noted

~~was~~ is 79,000 ft long and 15 ft 6 in in diameter, going right under Ben Nevis, the hghest mountain in the British Islaes. Geology, therefore, dominated the job; all the resident staff were knowledgeable and some of this must have rubbed off on to me, especially my contacts with one of the two assistant ~~R. E's~~ Resident Engineers, Ben Peach. He was the son of the great Dr. Peach who first unravelled the geological structure of the Scottish Highlands. Himself a geologist of note, Ben Peach taught me a lot; his son, with the same name is now a Professor of Geology at Brock University! The geology along the tunnel route had been predicted by Dr. Bailey, later head of the British Geological Survey (I think); he and other geologists came up to see the tunnel work in progress. It went well, with no unusual difficulties, rock being as expected. Sundays, spent in the absolute quiet of the tunnel, doing essentiel survey work, provided an experience never to be forgotten. All in all, I was a fortunate youth to see such a job at first hand, every aspect of it confirming what P. G. H. Boswell had taught in a general way.

But I could see the end of the job coming, with the long tunnel holed through and the steel ~~pipes~~ Penstocks in place. I had caught the "construction bug" and could see that there would rarely, if ever, be as big a job as Lochaber again in the United Kingdom. So for reasons that I can not fully recall, and after finding out that New Zealand was, even then, so socialised a country that opportunities in construction were limited, I decided to come to Canada. Mr. Halcrow understood; he had gained overseas experience. One of his former colleagues who had gone to Canada and been very successful as a consultant (A. D. Swan) encouraged me when he came in to see the staff in the old office on Victoria Street, Westminster, and so I sailed from Liverpool in March 1929.

CANADA: PRE-CONFERENCE (1929-1936)

Arriving in Montreal (by way of Halifax and C.N.R.) in the early spring of 1929, six months before the depression broke over North America, was an exhilarating experience. There was a shortage of engineers and so one had a choice of jobs; I had five offers, all pressing. I elected to join the Power Corporation since they promised me construction experience. (In those days, the newly established Power Corporation was designing, building and financing new power plants for the power companies which it had bought up). Dominion Day therefore found me up in the Cobalt district of northern Ontario, as Resident Engineer and assistant to the Superintendent of the Upper Notch plant, to be built on the Montreal River for the Northern Canada Power Co. In slightly less than 18 months, from the first clearing on the site, we had the plant operating and turned over to the Power Company. The plant was then the largest completely automatic power plant in Canada but generated only 13,000 H.P. This was wonderful experience for me, in many ways; I came "out" of our camp in the bush only three times in the eighteen months. In this context, however, there are only two things to be related since the dam and power house were all founded on bedrock and, of concrete.

In our excavation for the power house, on the east bank of the Montreal River, I had my first encounter with glacial till, as tough as any I have ever seen since. I wondered how we were going to move this stuff on our tight schedule but I need not have worried. Our general foreman, a man who could neither read nor write, was wise indeed in the ways of construction. After he had examined the till and seen what it was like, he ordered a powerful pump and some

fire hose; as soon as he personally directed the first high pressure water jet at this rock-like material, I watched it disintegrate before my eyes and flow down the wooden trough below, the grades being suitable for gravity flow. In just a day or two, the excavation was complete, and I had learned another lesson about "soil".

Near its entry into Lake Temiskaming, the Montreal River then flowed through a very narrow gorge. This provided an "obvious" site for another water power development and so, in our second summer, we were asked to see what we could find out about foundation conditions at the gorge (called the Lower Notch). We had some of Canada's best "river-men" on the job; it did not take them long to get timber cribs firmly anchored in the turbulent water at the head of the gorge, from which diamond drilling was then to be carried out (a photo of the drill set-up being one that I have used several times to illustrate test drilling). I went down by canoe regularly to inspect the work and to "keep an eye" on it; in this way I learned much that was useful about rigging, river work, and diamond drilling. Our drilling started with wash boring ... and continued in the same way (in mid-river) since we never did reach "bottom" (although later work showed that we were only a few feet from the bottom of the gorge in our wash- boring through sand). It is an interesting commentary upon Canadian development to find that the Lower Notch plant has now been built (by Ontario Hydro), crest level being such that it has flooded out completely the (over) Upper Notch plant, the machinery in which was removed before all our good concrete was ~~was~~ submerged never, probably, to see the light of day again.

One "side-effect" of the Upper Notch job is of relevance. I had been introduced at 16 Victoria Street, as a part of my training, to the then current periodical literature of civil engineering, the two fine U. K. journals (now sadly changed) and Engineering News-Record. In those days it was a really professional journal, advertising front and back only, contents including full accounts of engineering projects and research. So I read and noted the famous 1925 articles by Terzaghi, his first North American publications. Somehow the editors of E.N.R. heard about the Upper Notch job, presumably since we were placing almost all the concrete under winter conditions. They wrote and asked for an article; the Chief asked me todo it; I did and the article was duly published. The associated correspondence included a friendly exchange with the Senior Editor, Van T. Boughton, who had worked in his early days on the other Montreal River of northern Ontario.

They remembered this since, in mid-1933, they wrote and asked if I would like to be their (part-time) engineering correspondent in Montreal. One did not refuse anything in those days and so I gladly accepted. My first short news note appeared on 14 December 1933; today, I am still privileged to be (now) a special correspondent to the Editor. In this way I learned how to read a newspaper so as to be able to pick out any item of engineering significance. More than this however, it led to close friendships with Van Boughton and Waldo Bowman, the two men who "ran the paper" in those years. And Van told me the story of the 1925 articles - how they came in to the Editor, F. E. S. Schmidt who found them so badly written that he put them in his waste-paper basket; Van rescued them from there, rewrote them, after seeing their value, and so helped tolaunch Terzaghi on his North American career.

Living in the bush prevented me from realising the full extent or meaning of the depression, despite the news that we did get. But it did not take long after "coming out" (at the end of 1931) (and getting married, in a quick visit to the United Kingdom) to realise, when living in Montreal, how very serious economic affairs were becoming. This is no place for any details of what those days were like so let me merely record that, after seeing that much of the work in the Power Corp. office was "made work", it was not too much of a surprise when, on a day in February 1932 (at the very depth of the depression) the Corporation discharged about thirty of us engineers, all married men, some with children, all having been promised "permanent" jobs. And at that time, one third of all the engineers of Canada were out of work.

I was very fortunate, yet again, for within a few weeks I was offered (and accepted!) a job with the Sun Life Insurance Company, to operate their new [illegible], the details of which are here irrelevant. But when unemployed one has time to think and so, with my wife, we did a lot of thinking and talking about the future, optimistic about the future in a way that now seems almost irrational! I had seen so clearly the fundamental importance of geology in civil engineering, and especially the geology of soils, that I was surprised at the almost complete neglect of the matter in the literature of civil engineering, with which I kept in touch. I wanted to talk with someone about this. It may have been through Dean Brown (Engineering, McGill, a fellow graduate of Liverpool) but somehow I was advised to ask Professor J. J. O'Neill, then Prof. of Geology at McGill (later to be Dean of Science, and then Vice Principal), if he would see me. He said he'd be glad to and so I went to his office at McGill on 10 March 1932 for a talk that would prove to be another turning point in my life. Happily, I was able to keep in touch with Dr. O'Neill until just before he died; his friendship was a rewarding experience; I think that he knew how I valued it.

I was then only 28, still naive in many ways, and I "walked in off the street" so to speak. And yet this great man gave me far more than an hour of his time and talked with me as if I were one of his prized students. I found that he had similar ideas to P. G. H. Boswell, in realising the place that geology should occupy in civil engineering; he knew, and regretted, the way in which it had been almsot forgotten, thought that "something should be done about it", was sure that this was a field of activty for the future, and finally told me that if you really wanted to learn about something, the best way was to try to write about it - this latter advice being something that I, in turn, have often tried to pass on. It finished up with him suggesting that I should write a paper on Geology and Engineering and try to get it accepted by the Engineering Institute of Canada, in those days at the peak of its professional service with a very active Montreal Branch.

So I got busy, keeping in touch with Dr. O'Neill who eventually read the paper that I prepared. When I had his approval, I submitted it to the E.I.C. They asked me to present it to a regular meeting of the Montreal Branch (which I did on 18 Jan. '34) and then asked for it for the Engineering Journal where it appeared in due course in print (1). Active discussion at the meeting led to new and rewarding friendships. A copy of the paper went, naturally, to P. G. H. Boswell who also approved, so much so that, to my amazement, he asked me to join with him as a co-author of a book on the subject that the well-known British publishers, Edward Arnold Ltd., had asked him to write. He was good enough to say that the paper was really a synopsis of the book he had in mind; would I please go ahead and prepare a detailed outline of what I thought the book should contain.

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(1) Legget R. F. (1934), "Geology and Civil Engineering; their relationship with reference to Canada", Engineering Journal, 17, 422-442.

Responding to this challenge occupied all my evenings for many weeks but eventually I had a complete outline of what I thought such a book should contain. I had separate sheets for each chapter, and headings for each section of every chapter. My new work, to be related shortly, took me to England in the summer of 1935. Professor Boswell invited me to luncheon at the Athaneum, a great thrill in itself for a youngster such as me; afterlunch we adjourned to a small room and there, without saying a word, he went right through the outline I had prepared. At the end, he looked up, made some appreciative comment to the effect that it was fine but added "I want you to change one word". When I asked which word he replied "My name; take it out. This is your book....." Argument was of no avail; he was too busy etc., and the outline was the book (I found out, much later, how right he was in this). He said that he would write a foreword - which he did - but insisted upon my getting busy and writing it. I relate this detail as further evidence of the greatness of this man to whom I owe so much. After my return to Canada, he agreed that it would be awkward for me to deal with a British publisher and fully agreed with my taking it to McGraw Hill in New York with what results will be related later. Here, however, I should note that the last chapter in the first (1939) edition of the book (Geology and Engineering) deals with "Soils and Soil Mechanics". The chapter was written after the 1936 Conference; the concept of a chapter on soils was there in the outline of 1935.

Now I must revert to 1932. In the early Fall, I was advised of an opening with a new small company just embarking on the sale of British steel sheet piling in Canada. This was close enough to

foundation engineering to be of interest, so I applied. I found, to my pleasure, that the little company was an offshoot of the British Steel Piling Company of England, which I had come to know before coming to Canada. They were suppliers of Larssen steel piling which I already knew to be in a class by itself because of the patented Larssen interlock. The little Canadian company was essentially a sales office but the British Company insisted that they must have an engineer quite divorced from sales, to assist engineers with design work and to inspect and assist with every installation of Larssen piling in Canada, to ensure that all were well done. This was the job; I was fortunate to be appointed and then had the benefit of a good training perid with one of the senior BSP enginners who was in Canada for this purpose. When he returned to England, I was on my own, apart from regular contact by mail; it was the beginning of four most interesting years.

The world-wide Larssen companies had pioneered in the use of steel sheet piling as a permanent unit in construction, especially for retaining walls. This fitted to the needs of Canada at the time, when much construction was government-sponsored and geared to local needs. Accordingly, I got to know all the district engineers of the Department of Public Works, Canada, in the eastern part of the country (as far west as Winnipeg), seeing what devoted public servants these men were, with wide areas of jurisdiction, small budgets but many difficult jobs. Keeping timber crib wharf and breakwater structures in repair was a widespread problem. Steel sheet piling provided a perfect solution since, with good design, it could be driven

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(2) For an introduction, see Legget R. F. (1936), "Steel Piling; Some notes on its development and use", Engineering Journal, 19, pp. 273-280 (given to Toronto Branch, E.I.C. on 6 March 1936).

all round an existing, deteriorating timber rock-filled crib without the necessity of removing the cribwork, always a difficult and costly job. All round the Atlantic coast line of Canada small structures reconstructed in this way may be seen today. When conditions were suitable, new structures could likewise be economically built with steel sheet piling; one of the best examples is the large Marginal Way coal dock at the east end of Toronto Harbour, an area of several acres, surrounded with Larssen piling, tied back to anchorages, the whole filled with sand dredged from the lake bottom.

The design of such structures was a nice problem in statics! The piling, when in place, was a ~~horizontal~~ vertical beam, fixed at one end (in the ground) and supported on a point support near the other (by tie rods). An elegant graphical solution to the problem had been developed in Germany by a Dr. Blum (I think), based on or related to work by Dr. Lohmeyer. None of this had been published in English, the information I got about it coming from BSP Ltd. I always intended to "write it up" and think that I still have my notes for this but, as will shortly be seen, time for this always eluded me (2). I mention it now since every case depended on the assumtions made about the soils involved, and upon the assumed distribution of soil pressure. It always surprised me that the great U. S. steel companies, or one of them, did not latch on to the Larssen patent so as to open up a whole new market for their output. Just before the war, when the name of Germany was in disrepute, they tried to break the Larssen patent and one of my most interesting assignments ~~ever~~ resulted - a day spent as an "expert witness" explaining to the Master of the U. S. Court of

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Claims, in Washington D.C., what the interlocked section modulus of steel sheet piling was, and why you could not use this with the interlocks on U.S. sheet piling. It took a whole day, but the Master, an outstanding lawyer, eventually got a clear understanding and the case was as good as won. But the war started before the judgement was given, and another small chapter in North American civil engineering was closed. Prior to this, an engineer named (I think) Benoyer had a series of articles in Civil Engineering - written on behalf of either U.S. Steel or Bethlehem Steel, - outlining design methods for steel sheet pile walls etc., lifted from German work to which either little or no acknowledgment was made. They had little effect on practice in North America, or rather in the U.S.A., because you ~~cannot~~ can not use the interlocked section modulus with U. S. Piling and so the economy of this method of construction becomes questionable. Z-shaped steel piles were introduced to try and get around the problem, but they proved ~~very~~ difficult to drive and so have not come into wide use. (There ~~is~~ also a small Canadian sequel; a record of that is for another day).

Readers of this note may think that I have forgotten my main thesis - the development of Geotechnique in Canada. Not so! I have quite deliberately allowed myself to go into this detail about the uses of steel sheet piling because, now looking back, I find myself convinced that it was the development of steel sheet piling in Europe, and especially in Germany, that was one of the major factors in the final recognition of Soil Mechanics as a discipline. The general explanation, which I have myself used, is that it was the attention given to landslides (on the Panama and Kiel Canals, and the Swedish State Railways) that sparked the

concerted interest in soils which, coupled with the great pioneer work of Terzaghi, led to the recognition of Soil Mechanics and the holding in 1936 of the first international conference at which the (somewhat unfortunate) name of "Soil Mechanics" received its formal recognition. But I am now as certain as I can be that it was the use of steel sheet piling as a permanent unit in civil engineering practice, due to the work in Germany which I have touched upon, consequent upon the development of the Larssen interlock, that was yet another influence in this development, although generally unrecognised.

Against this background, it was quite natural that I should have noticed in an issue of Engineering News-Record in the spring of 1936 a tiny item about a proposed conference on soils and "soil mechanics" to be held at Harvard University. I wrote to Arthur Casagrande, got a kindly reply, and duly registered for the meeting with the support of the Piling Company. I did not realise that it would take over 3,000 words to provide the background for my attendance at the Conference but I have not hesitated to give the foregoing explanation since I would not wish it to be thought that it was by some touch of inspiration "out of the blue" that led me to participate in this significant gathering. It can now be seen that, thanks to unusual good fortune in my early years in civil engineering, I had been conditioned to appreciate the importance of soils so that I was ready for the call when it came.

FIRST INTERNATIONAL CONFERENCE (1936)

Making every allowance for natural bias, my wife and I still think of this meeting as the most pleasant and rewarding that we have ever attended. There were only 206 present, just the "right number". The programme was well arranged with plenty of time for private talks. There was an atmosphere about the entire week that was exciting, as if we knew instinctively that this really was the start of a new chapter in civil engineering. We were all accomodated on Harvard Yard, and ate in University dining facilities (except for a few special functions). All this led to our making at least fifty good friends there, with whom we kept in touch for many years after (in most cases for the rest of their lives, most of them being older than we were). I am tempted to give the full list but this would not be too helpful; instead, let me give the names of those who became very close friends.

Dr. Terzaghi, President of the Conference, for some reason unknown always to us, gave us thegift of his friendship then, and for the rest of his life. Similarly, we got to know Arthur Casagrande and his brother Leo, then living in Germany. Phil Rutledge was one of Arthur's assistants at Harvard; later we saw much of him and his wife. Practicising engineers, already men of distinction, included Carlton S. Proctor, Admiral Bakenhus (rtd. U. S. Navy), Lazarus White and his two sons, Robert Ridgeway, F. T. Darrow from Nebraska, and H. A. Mohr of Boston. Yourger men included Earl Bennett, Spencer J. Buchanan (friendship with whom was broken only very recently by his death), Donald Burmister, A. E. Cummings, C. A. Hogentogler. E. J. Kilcawley, D. P. Krynine (at Yale), J. O. Osterberg,

M. G. Spangler, O. L. Stokstad, Donald Taylor and E. W. Vaughen of T.V.A. Many of these names will be recognised from their later contributions to the literature. One U. S. name I have left for special mention, Bill Housel; we "hit it off" straightaway with him and his wife but even at this meeting the antipathy between him and Arthur Casagrande - which was so unfortunate - was already evident; and yet Bill, despite some of his unusual ideas, was one of the soil pioneers of this continent.

From overseas there were Paul Raes of Gehnt, Belgium, (who stayed with us in Montreal after the meeting, to see how a bilingual society worked, and who became a very close friend), Rudolph Tillman the City Engineer of Vienna (who wanted to know what a bulldozer was for a dictionary one of his freinds was preparing; so I photographed him with one on the big field trip), Drs. Hanna and Tschebotareff then both from Egypt, Lan Cooling from the U. K., the start of a close link, Bas Steevens from the Netherlands East Indies (imprisoned, with all his family, in different camps for the whole of the war by the Japanese), and J. J. Bryan from the Straits Settlements, then under the U.K. From these short lists (and they are far from complete), it can be gathered that the United States contributed about two thirds of those attending. What of the Canadians?

Eight Canadians were listed, six certainly attended. From the Department of Public Works, Canada, came H. M. Davy long-time head of their test-drilling unit, and Jack Lucas at the start of his work in their testing laboratory; from Universities, Dr. C. R. Young, then head of the Department of Civil Engineering at the University of Toronto (later to be Dean) ~~and~~ Professor I. F. Morrison of the University of Alberta, and from Saskatchewan, Professor G. M. Williams.

(None of us ever saw Prof. Williams but he was a shy man and may have kept to himself; he maintained some sort of residence or link with the United States all the time he was at U. of S.). From construction came R. J. Mattson, a dour Scandinavian who was an office engineer with the Foundation Company of Canada, a quite delightful chap once you got to know him even though he had no sense of humour whatever and was the greatest "squirrel" for trade literature that I have ever met!!. He had been sent to the meeting by the President of his Company, "The Chief", Mr. R. E. Chadwick, one of Canada's all-time construction "greats." We both have the impression, still, that "The Chief" did turn up at the meeting, maybe for just a day, but his lively interest in Soil Mechanics was already there, in keeping with his forward-looking outlook. And, somewhat to our surprise, J. M. R. Fairbairn, the great Chief Engineer of the Canadian Pacific Railway, was listed as an attendee, even though neither of us can recall seeing him there. But his interst, expressed in his registration, is yet another indication of the truly great men who were the leaders of Canadian civil engineering in those days of such keen development. And we brought up the tail end. I had known Harold Davy, Jack Lucas and Matson previously, and had met "The Chief" and Mr. Fairbairn (both most kindly to me as a youngster) but the meeting gave me (I am almost sure) my first meeting with "C.R." Young, and certainly so with I. F. Morrison - so greatly to my lasting pleasure until his death.

The technical proceedings of the Conference are available for all to see in the three volumes of Proceedings (reprinted at Harvard in 1965) but it may be convenient if I include as an appendix to these notes a copy of the account I wrote fro the Engineering Journal; it will be found at the end. When I look at this today,

I am concerned to find no names of fellow Canadians listed; I feel sure that these were in my original account but it was "boiled down" somewhat by the Editor, as noted in the heading (3). This account gives (I think) a good general idea of the technical content of the Conference; let me add just a few notes about the more personal aspect of it.

The registration fee was only $10 for everything, accomodation in The Yard being gratis! The total cost of the meeting, including the initial printing of the Proceedings, was only $7028, of which sum Harvard Univeristy paid over $3,000 from their Tercentenray account, since the Conference was a part of the summer-long celebration of the 300th. anniversary of the great University. The initial notices said that foreign guests would be received in New York - so I naturally asked A.C. if we could be classed as Foreign guests! He agreed, for this part of the meeting only! So we travelled to New York, stayed at the Sheldon Hotel with all the others, were welcomed at a cocktail party on the 64th. floor of the R.C.A. Building, followed by a fine dinner in the Rainbrow Grill. On the day following (Saturday) we were all taken by bus on a fine tour climaxed by a visit to West Point, my wife being taken by Mrs. Robert Ridgeway in the lead car, following a motor-cycle escort which went through every red light, to the amazement of most of the visitors - even to us, it was a thrilling ride! On the Sunday, a special car had been arranged for on the 3.00 p.m. train to Boston on the New Haven line; we sat with the Bax-Steevens, and so began one of our valued friendships. The meeting started on the Monday and lasted all week, finishing with a fine field trip, all-day, to the Quabbin water supply scheme for Boston, then under construction. And the ending was a memorable dinner at Longfellow's Wayside Inn (since burned).

(with apologies for this slight confusion)

For Page 20

(3) Legget R. F. (1936), "The First International Conference on Soil Mechanics", Engineering Journal, 19, pp389-391.

For Page 21

please add at *

... adopted - unanimously, apart only from one opposing vote to the resolution about continuing the Conferences; for the life of me I can not now recall who this was!

and

(4) Legget R. F. (1936), "The Correlation of Soil Mechanics Studies with the design and construction of retaining walls", Proc. of the First Conference, vol. 1, pp. 207211 (Discussions noted are in vol. 3, pp. 142-143 and 160).

What did Canada contribute to the Conference? It is a measure of the stature of "C.R". Young that he was asked to preside over the final session at which resolutions - of thanks, and of a desire for a continuation of the conferences - were unanimously adopted.* (See p. 20A) In the third volume there are two contributions to the discussions - a listing of British publications on pile driving and the supporting power of piles, and another on British publications on lateral earth pressures, both of which I was asked to contribute. There is only one Canadian paper in the other two volumes, and I wish there wesn't! It was a summary of one I had prepared, noted below, but it is really a most amateurish effort; I was ashamed of it when I saw the calibre of some of the other papers. All I can say of it is that it did indicate Canadian interest, even if it also showed the abysmal ignorance of one Canadian.(4) I hope that no reader of these words will ever look at it.

DEVELOPMENTS IN EASTERN CANADA (1936-1939)

All of us came away inspired. Professor Morrison returned to the West; I am sure that Dr. Hardy will be recounting his early work. "C.R". Young returned to Toronto, with what results will shortly be seen. Matson reported to "The Chief" whose interest in the development of Soil Mechanics was thereby assured. The one blank in my memories is what happened in the Department of Public Works. Harold Davy continued with his test drilling work with little opportunity for "doing anything" about Soil Mechanics, although we certainly talked about it during my occasional meetings with him. I do not know whether Jack Lucas was able to start any laboratory work; he was in the R. C. A. F. during the war. Possibly

(4) — see p. 22a

his successor, Norman Laycroft, might know if he could be located. (The D. P. W. Laboratory was abolished a few years ago and Laycraft retired. All my own contacts in D.P.W. have now retired or died and so I have no leads that I can follow up. It might be worth digginginto this, although my impression is that J. W. L. did not do very much in the way of soil studies).

In my own case, I returned to my work in C S. P. with renewed vigour, grateful for being at work in a field of such interest; but long-held thoughts of getting intothe teaching of civil engineering were revived. The idea had crossed my mind after some years of experience (after I had come to Canada). Professor O'Neill mentioned it as one way of "sowing seeds" about geology in civil engineering. P. G. H. Boswell added his imprimatur when I asked him on one of my visits to the U. K. And an old, wise friend of my Father endorsed the idea also, but got me to promise him that I would not apply for a University post until I had at least ten years of practical experience under my belt. How wise that advice was I found just as soon as I did start at a University; ten years may be a bit much, but I often wish that I could be a dictator and make sure that no University teacher was ever appointed (in any subject) until he has been at work in the "real world" for at least five years.

With this advice behind me, I did not do anything until the spring of 1935 (I graduated in July 1925). I then wrote to all the Deans of Engineering at Canadian Universities and got back kindly replies, all to the same effect - that their University was not yet recruiting any new staff, following the depression years. Some asked me to see them; I recall a delightful talk with Dean Featherstonehaugh ("Feathers") of the University of Manitoba in his brother's architectural

office in Montreal. I knew Dean Brown of McGill, and had been privileged to meet Dr. Sexton, the first President of Nova Scotia "Tech". I think that I had also met, by then, Professor Earl Turner, and the famous "Blinky" Stevens, at U.N.B. No luck, so I put the idea out of mind, and "get on with the job".

But after the Conference, we thought it worth trying again, so I wrote again to the ten or eleven engineering schools of Canada; replies were all considerate and kindly but the answer was the same again - No Vacancy. When all the replies were in, my wife and I went for a walk one September evening (in 1930), discussed the future, and decided to put behind us all idea of a University post, starting to plan a future in other directions. We got back to the house and there was a telegram from Dean Clarke of Queen's asking me to get into touch with him. (I had been fortunate enough to meet this fine man during the year). When I called, he told me that they had been inundated with an unexpectedly large second year in engineering and could handle it only if the great Professor "Sandy" Macphail had an assistant. If I was interested, would I come up to Kingston for an interview? I was there the next day (I think); was interviewed by Dr. R. C. Wallace as (I believe) his first official duty as the new Principal, and as a result was offerred a job as lecturer at a salary of $1,200 a year, a reflection of the tight control exercised on all Queen's monies by the Treasurer, Dr. McNeill (later a good friend). I said that since I was married with a young son, I could not live on that and so it was graciously increased to $1,800 a year which I accepted (with my wife's kindly agreement); it was just 50 per cent of my salary from the piling company!

Then followed some hectic days, the term at Queen's about to start, my presence being urgently needed. The President of the piling company was cooperative and let me go with my promise to serve the Company as consultant in what time I could spare; to the day of his death he regarded me as half-crazy to have given up my good job with him! We found an apartment and within about a week were resident in Kingston. At that time, the Department of Civil Engineering was unique, staffed by four full professors and one lecturer! There was an historical explanation of this but I was soon introduced to, and became friends with, Professors ~~McNeil~~ Macneil, Wilgar, Ellis and Malcolm, characters all, with Sandy ~~McNeil~~ Macneil one of the greatest characters Queen's has ever known, a C.M.G., friend of Rudyard Kipling, editor of the Queen's Quarterly, and the only Professor of General Engineering in Canada! (Even mention of his name makes me want to digress; for any interested, I have written up this one episode of my life (5)). Doug Ellis went on to be Dean, always interested in hydraulics. Bill Wilgar retired early (I think), but the soil story takes up again with Lindsay Malcolm.

This fine man entered upon a new lease of life after tragically losing his wife; he became a "workaholic", taking his Ph.D. at Cornell University in Sanitary Engineering at the age of 55, and bestirring the Queen's department in a way never known before. He got funds for building a full-scale sanitary plant on the waterfront at Kingston, as a research laboratory. He was just setting this up when I arrived. When he heard of my new interest in soils, he generously set aside one room for my use, and this was my first soil mechanics laboratory - started in 1937. It was a slow start, but by early 1938 I was equipped for the simple soil tests and had drawings etc. ready for building equipment necessary for the more important tests already

(5) Legget R. F. (1971), "Sandy Macphail", The Queen's Review

developed such as soil compaction, and direct shear.

Not too long after I had joined the staff at Queen's I received a very kind letter from C. R. Young, at Toronto, saying how interested he was that I had entered University work and yet how sorry that he had not been able to entice me to Toronto; an opening was coming up etc.. ..Two letters followed during 1937, in each there was a firm offer of a job in C.R's department; to each I replied that I was happy at Queen's etc., with my thanks for the offer. I said nothing to my senior colleagues. Early in 1938, however, a third offer came from C.R., this one most pressing, with the offer of an Assistant Professorship, responsibility for the course in foundations etc..... a real "carrot". I was indeed happy at Queen's, already looking forward to staying there a long time; but this letter from C.R. was such that I had to discuss it with Sandy MacPhail, telling him (naturally) that it was the third. Looking over his half-moon spectacles, Sandy said, in his own inimitable manner ; "Well, Legget, you don't know how good one Universty is until you've seen another; I think you should take it. Go to Toronto and see what the place is like - but be <u>sure</u> to come back." So I accepted, although when I saw Dean Clarke's disappointment, I felt badly about my decision.

I had naturally discussed the matter with Lindsay Malcolm; he listened attentively but said little. When, finally, I told him my decision, he surprised me by saying "Now let me tell you something." He had been offered the position of head of the Department of Civil Engineering at Cornell University at the same

time as I had received my offer from Toronto. Since by this time he knew me, he decided to postpone his decision until I had made mine since, as he said, he knew that if I knew he were leaving, I would not leave at the same time because of all that had gone into that laboratory. I relate this incident to indicate a measure of the man who was such a good friend but, as I look back, I realise now that it was another of those turning points in my life when the fates were indeed kind to me (and to my interest in Geotechnique).

While all this was going on, I was faced with my first consulting job, a critical redesign of a cofferdam which had failed, killing four men, at Port Stanley. Late one evening, one of the very fine early contractors of eastern Canada, William Bermingham (a man of whom it was said that you never needed a written contract with him; his word was his bond) called on me and most earnestly entreated me to help him; this was the first fatal accident he had ever had in his life and he was determined to see that the job was finished with no more trouble. Only because of my respect for him did I accept. I visited the site; prepared a design (in steel sheet piling); had this checked; and the job went ahead again - but I was not there when they drove the piling, being busy with my teaching duties. We got an alarming message - that the cofferdam had failed again. I went up with Bill Bermingham (the son - a traumatic journey which I can still remember), and we were soon on the job. It was not a failure of the cofferdam but distortion of some piles. The Superintendent, even though instructed what he had to do, thought that he knew best and did not follow the specific instructions written large on the cofferdam plan. It was Larssen

piling in use; the interlocks were tight in a way the Superintendent had never seen before; this had caused the trouble. It was soon corrected. The Super. learned a lesson; so did I - that no civil engineer should ever accept responsibility for a design unless he is on the job, or represented, to see it built ... a lesson still to be learned by so many.

We moved up to Toronto in the late summer of 1938 and began what was to be a period of nine wonderful years, despite all the stress of war. C.R. was another of the great men with whom I have worked and who must have influenced my own progress. He was a wonderful teacher, a kindly man, highly respected in the profession, and, in keeping with his kindness, he told me when I arrived (in a round-about sort of way) that soil mechanics was already being taught in the Department by a long-time member of staff who gave it in his highway engineering course. But, he added, he wanted me to develop a proper soil mechanics laboratory and with his strong support, I did so in a basement room of the (old) Electrical Building, then used also by the Civil Department. Using the drawings of a direct shear box that I had brought from Queen's (with Sandy's permission), Bill Kubinga, the excellent man in charge of the machine shop, made a shear box and stand ~~were made~~ and these were ready to be used early in 1939.

C. R. had very firm views on the place that consulting work should occupy in his department. He encouraged his staff to do this provided that (1) it was in their own field of study; (2) it did not in any way compete with regular consultants; (3) it did not interfere in the slightest with University duties; and (4) when possible, it would result in a research publication.

He was meticulous in following these guide lines himself. From the number of visitors to his office, his servics must have been much in demand but he accepted very few assignments. He kindly passed on to me one or two smaller inquiries, of no great significance here even though they usuelly involved soils. One request that he did not turn down, however, was from Dr. H. G. Acres for assistance with the Shand Dam.

This dam was to be built in the headwaters of the Grand River of south-western Ontario by the Grand River Conservation Commission - the first structure to be built for conserving the waters and soil of south-western Ontario, now a splendid movement covering all of the area through river conservation Authorities. Prime objective of the Shand Dam was to control flooding on the Grand River i.e. to undo what man had done to the Grand River valley by clearing it during the last century. It was on 4 March 1938 that Andy MacQueen, one of Dr. Acres' associates, called on C.R., to discuss the project for which they were to be the consultants. They had the idea that, apart from the necessary concrete spillway, the dam might be built of soil. C. R. discussed the matter with me after Andy's visit. I said that I thought I would have the shear box in action within a week or two, being already equipped for simpler tests such as for soil compaction and permeability. Accordingly, MacQueen and Bob McKordie came up to spend most of a day with me discussing the whole idea, the design of earth dams, the necessary soil testing etc., this being on 31 March. It was agreed that soil test pits should be put down around the dam as a first step. These showed glacial till over a wide area, examined by a group of us on 5 May; at about that time, the bold decision was taken by Acres to proceed with an earth dam.

The Shand Dam was, I think, a pioneer structure for North America. The two main embankments are built entirely of glacial till, with no core wall but with suitable drainage features. Fortunately a good contractor won the job and followed instructions (from Acres) very well, even though compacting till in layers at proper moisture content had not been seen before in eastern Canada. The job was finished in the summer of 1939 and the dam has performed well since. (I paid a brief visit to it again in 1980 and was delighted at how well it looked). Fortunately, this pioneer work in soil mechanics was well written up by the two men mainly responsible in a "key" paper published in the Engineering Journal, then still the main Canadian outlet for such professional papers (6). All the soil testing was done in my lab at the University of Toronto so it was a busy place so early in its history, with several student assistants helping me. I got more and more baffled by the variations in the properties of the till; if I had then known more about the geology of tills, the paper I wrote summarising our work (another very "amateurish" effort) would have been very different (7). But before there was any oportunity to follow up some of the many questions raised by all the work on the Shand samples, the war loomed up as a possibility and, finally in the early Fall of 1939, as an actuality.

There is one footnote to be added about the Shand Dam. Right from the start, the Acres office appreciated the importance of the site geology. They approached the Geological Survey of Canada and the officer in charge of their work in S.W. Ontario became a member of the Shand team in an informal way - Dr. John F. Caley. He became a close friend of mine, the start of a happy liaison with the Survey.

(footnotes over)

(These footnotes should have gone at the foot of p. 29; I apologise again for this slip in typing - showing how unaccustomed I am to the use of footnotes!)

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(6) McQueen A. W. F., and R. C. McMordie (1940), "Soil Mechanics at the Shand Dam", Engineering Journal, 23, p 161.

(7) Legget R. F. (1942), "An Engineering Study of Glacial Drift for an Earth Dam, near Fergus, Ontario", Economic Geology 37, p. 531; reprinted in Engineering Journal, 26, pp502-508 (1943)

THE WAR YEARS (1939-1945)

The first impact of the war on work at the University of Toronto, and specifically in the Department of Civil Engineering, was a visit I had in the Fall of 1939 from a courteous gentleman named Roewade who explained that he was a Danish-Canadian, a brewmaster with one of the big breweries! I wondered what this had got to do with me but he went on to explain that a nephew of his, a brilliant young Danish engineer, had won a Danish State scholarship after some years with Christiania and Neilson, the big contractors, for graduate study at the University of London (England). He had given up his job but when he got to London, they had just had to decide to cancel all graduate study because of the incidence of war. They recommended that he should apply to Toronto since, in some way I never did fathom, they knew that I was there and had started work in soil mechanics which the young man was anxious to study. Would I take him as a graduate student if he came to Canada? To that, said C. R., there is only one answer and so, towards the end of the year, Mr. Per Hall arrived to be my first graduate student.

It was a pleasure working with Per Hall, his practical experience very quickly putting us "on the same wave length", as also his wife who came to Canada with him, herself a civil engineer. After a few weeks of preliminary discussions and study, we both found ourselves attracted by the significance of Professor I. F. Morrison's paper on pile fundamentals(3) and so, with C. R's agreement, we decided on the correlation of soil properties with the supporting power of bearing piles as the subject of Per Hall's thesis. Not many days after deciding this, he appeared in my office one morning with the tragic news that Denmark had been over-run by German troops, cutting him off from his family, his Government, his funds. All else was put aside while we helped him to try to join the R.C.A.F. but his Danish citizenship stood in the way of this, naturally for that time. Disappointed, he accepted the inevitable and joined the Aluminum Company of Canada, his knowledge of Danish enabling him to make a significant contribution to the war effort of this company, especially in the Virgin Islands; later he transferred to the Foundation Company and eventually set up his own office. Now I wonder if this was the start of graduate work in Geotechnique in Canada.

1940 was the time of the "phony war" but by this time we, at the University, knew that the Government had decided that no University staff (at least in engineering) would be permitted to join the armed forces. This was for me a keen disappointment but the decision had to be accepted and I can now see (although not at the time!)

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(3) Morrison I. F., (1939), "The Fundamentals of Pile Foundations", Engineering Journal, 22, p. 431.

that the decision was a wide one and essential for the national war effort. Our energies had, therefore, to be turned in other directions, once essential University duties were done. The latter grew in intensity - with classes to be taught Statics numbering well over 300 students etc. - and so all University research came quickly to a standstill. Again, I was most fortunate in the "outside" demands made upon me. The first, early in the summer of 1940, was to assist a great friend (the Superintendent of the Upper Notch job, of 1929-1930) who had been charged by the Hudson Bay Company with getting their Mackenzie River Transport organisation shipshape; it had just grown "like Topsy" and really had no organisation to speak of! Behind this was, of course, the El Gorado mine on Great Bear Lake and the need for getting their ore out from the North; at the time this was not known or even suspected.

It is a complicated story, of no special importance here but I spent the whole summer at Waterways, Alberta, the headquarters of the Mackenzie River Transport, then a tiny settlemet of 150 people! In the course of my travels, I visited Fort Smith in the Northwest Territories, and went even further North [thus being] introduced to Permafrost, although the name had not then been coined. This new aspect of soils fascinated me and I found out all I could about what was then known. This resulted in another very amateurish paper, of significance (perhaps) only because of the date when it was published (9). Little did I then think that I would come to know so much more of the North, and Permafrost.

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(9) Legget R. F. (1941), "Construction North of 54°N", Engineering Journal, 24, pp. 346-348 (paper given to London Branch of E.I.C. in 1940)

On my journey out from the North (I think) I called on Professor Morrison in his (then) tiny office on the campus of the University of Alberta and gained more insight into his sound thinking. I do not recall meeting anyone else during that visit; maybe Bob Hardy can correct me on this. Then I came east by way of Purdue University where there was held the second major soil mechanics conference on this continent. My main memories of the meeting are personal, apart from the diatribe of Arthur Casagrande against all forms of shear test except the tri-axial! In his usual enthusiastic way he "went overboard" about this new form of test. On the urging of other friends, notably Bill Housel, I was put up to answer him with amusing results, far too detailed for this record. My main memento from the meeting is a snap of D. P. Krynine and G. P. Tscheboteroff standing together, probably the only such record in existence since (although I did not know it when I asked them to stand for me) they were bitter enemies and never spoke to one another! (I have not looked again at the Conference Proceedings but will do so when time permits; this may permit me to add a bit more about this excellent meeting, the memories of which were soon clouded over by the war.)

Back, then, to Toronto to a very busy winter with University work while the tempo of the war got steadily worse; noone could see what was going to happen. It was not too great a surprise, therefore, when Andy McQueen (of the H. G. Acres office) called on me in February 1941, to tell me privately that there was a possibility of the long-planned and great Shipshaw power

project going ahead for the Aluminum Company of Canada, the metal already one of the critical materials in the war effort. He wanted to know if I would be willing to help them with the necessary foundation studies if and when a start was made. Again, there was only one answer to be made. We kept in touch in the weeks following. This was the year when Dean Mitchell persuaded the Faculty of Applied Science at U. of T., to grant all students their degrees, or year-end results, on the basis of their work during the term i.e.without writing any examinations, and so the term's work ended early. (The action was a profound mistake; and graduates were among the first to realise this when they faced up to urgent problems in their first jobs; three of them told me this, individually, during the summer when we were all living together at Arvida.) As a result of this, we were all free of University duties earlier than usual. I went over to Niagara Falls for a briefing on 5 May (1941) and left for the Lake St. John country on 8 May, staying up there on the Shipshaw job until 12 September when I had to return to Toronto for the winter's work at U. of T.

It is difficult to write briefly about this remarkable job - one of Canada's greatest civilian wartime projects - but I will be as brief as possible. When the initial development of the Saguenay River was carried out (by the Duke Power Company, I think) a dam and power house were built at Chute a Caron on the main river to provide power for the initial aluminum smelter at Arvida. It was then intended, however, that when more power was needed, the Chute a Caron Dam would serve to divert the whole flow of the Saguenay into a depression in the local Precambrian rock to the north

where, by means of a number of "saddle dams", a vast reservoir or headpond could be created from which the whole flow of the river could be led to a power house with its tailrace level almost at sea level, thus gaining the extra head in the rapids below Chute a Caron in addition to the head there. The result was a power house developing 1,200,000 h.p., all the works for which could be constructed "in the dry" while the river flow continued to go through the Chute a Caron plant and its associated spillway. This was the Shipshaw project, in brief. Once the decision to proceed had been made, the whole job was completed in eighteen months, despite one of the most severe winters exer experienced in the area - one of the greatest construction feats ever carried out in Canada and yet still virtually unknown. It was described in a series of papers by one of the Aluminum Company's executives, (Dr. H. G. Acres and Brig. V. G. Younghusband of the Aluminium Company, the main contractors (lo).

In May, the decision to proceed had not yet been made but, such was the critical urgency of all wartime efforts related to aluminum, it was decided to get ready with plans; the foundation studies had therefore to proceed at top speed. Two (or three?) drill rigs were on the site when I got up there, being supervised by Don Miller, a mining engineer who had previously worked for Acres. We were soon "in harness", advised in a wonderful way by Dr. E. C. Harder, chief geologist of Aluminium Laboratories who visited us regularly. Five retaining dams had to be built across soil-filled valleys; exploration, therefore, involved soil sampling and diamond

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(lo) DuBose McN. (1944), "The Engineering History of Shipshaw", Engineering Journal 27, pp. 194, with companion papers on Design by H. G. Acres, and Construction by V. G. Younghusband, extending to p. 249.

drilling at all five dam sites as well as test pit exploration, and diamond drilling and a test shaft in the bedrock at the power house and penstock locations. It is, today, difficult to convey the pressure underwhich we worked, pressure that must excuse the inadequacies and limitations of the geotechnical work I was able to "squeeze in". We worked all day and all evening six days a week, relaxation being on Sunday evenings, such was the challenge ahead of us. * (Please add here the material shown on p. 33)

Two aspects of our work are of significance for this record. Since the soils we encountered were not going to be used, but just removed from above the bedrock used for all foundation beds, there was no need for anything other than identification tests. With the help of the Chief Chemist of Aluminium Laboratories (who had a large research lab at Arvida) I was provided with a tiny lab and there assembed the necessary equipment (borrowed, I think, from Toronto) for simple soil tests. Much of the excavation was to be in a gray clay, the properties of which soon had me baffled. All my tests showed that the natural moisture content was higher than the liquid limit. I decided that I must be a poor technician. There was nobody knowledgeable ~~that~~ whom I could consult. Mr. Rimmer (Chief Chemist) was interested and helpful, offering to do the tests himself as a check. He got exactly the same results. I could see the effect of this i.e. that if the soil was disturbed, it would flow, without knowing why. So I told the contractor, after work had started, and was politely laughed at .... they wanted none of that academic nonesense on a priority job like this. I confirmed my advice in writing since theywere disposing of excavation in locomotive-hauled dumpcars on temporary track.

Late one Friday night, one of the dump trains, locomotive and cars, went over the edge as the large pile of clay started to flow down to the Shipshaw River. I was hauled out of bed around midnight and very politely asked what it was I had said about this ..... soil! After that, my relations with the men in charge of excavation was all that could be wished for; and I had had my introduction to Leda Clay. In our test borings at Dam site No. 5, we spotted some organic matter about half way down to bedrock, with clay above and below it. This was very puzzling since the layer was about twelve inches thick and even from our sampling tools we got specimens of identifiable wood (cedar being one I can recall). So again I warned the cnntractors and again was politely ribbed for my crazy ideas. But when the large excavator at the site got down to the level I had indicated, there was a bed of organic matter stretching right across the site. It included stumps of trees, some with beaver marks on them, others still with the smell of burning on them. Excavation was difficult but I watched it carefully and got some good samples to take back to Toronto. There I cnnsulted a new friend (who had done graduate work at Glasgow under an old friend of mine) who was in the Department of Botany with special interest in the very erudite field of palaeobotany. He was fascinated by the samples; we decided that when the war was over we would publish joint papers on this strange occurrence. And this was my introduction to Dr. N. W. Radforth, and the real start of muskeg research in Canada.

For convenience, I have given these detailed notes about the significant parts of my Shipshaw experience. Let me now quickly summarise the main events of that summer. We pressed on with our

work and managed to keep ahead of the design team at Niagara Falls, having regular visits from Dr. Acres and his three senior colleagues. They kept us advised of policy developments and so we heard, about mid-summer, that the job was to go ahead, with the highest possible priority, an overall contract having been negotiated with the Foundation Company of Canada Ltd. Bert Younghusband was to be in charge for them, a fine man with whom it was a pleasure to work. Speed was evrything, expense secondary. To one like myself trained in good economic construction, this was difficult (at first) to accept but the imperative of war soon corrected this! Some time later, I found that Dr. Acres had gone to Dr. Young at U. of T., to ask him to grant me leave of absence so that I could be the chief Resident Engineer for him on the job but C. R. refused, saying that I was needed at the University. I was a bit "miffed" when I heard this but again, as happened so often with me, I came to see that it was the right decision, I went back to the job in the summers of 1942 and 1943 to see the final stages of construction, and the splendid finished job. Even today, I look at it in unbelief, as I recall the hectic days of construction. One slight break was a talk I find that I gave, on Soils and the Engineer, to the local (Saguenay) Branch of the E. I. C. on 25 July 1941!

As the war neared its close, Bill Radforth and I took out our Shipshaw notes and prepared two papers for publication. In those days, the only possible outlet in Canada ~~(or elsewhere)~~ for geotechnical papers was through the Royal Society of Canada. One had to have a paper sponsored by a Fellow at an annual meeting; if acceptable, it might then be published in the Society's Transactions. (This will read strangely to younger workers of today, but this was the situation!) And so our two papers were presented in Kingston at the 1945 annual meeting of the Royal Society, and later published

in the Transactions of the Society (1) (2). Since I was so uncertain of my laboratory results, I decided not to include them in the paper; I can now see, of course, that this was a mistake. Fortunately, I had the opportunity of correcting this omission because of another error in our papers. None of those with whom we consulted could suggest why we found the bed of organic matter "sandwiched" between two strata of Leda clay, the idea of a landslide being apparently out of the question because of the surrounding topography. More than a quarter of a century later, Pierre LaSalle (of the Quebec Department of Natural Resources) found evidence of an immense landslide in this region. I was able to tell him the exact location of the bed of organic matter, which he then uncovered in a test pit and was able to study in detail. We then combined to prepare a paper correcting the mistakes of the earlier one, and including the results of his more recent work (3). These three papers form, therefore, a somewhat unusual early contribution to Geotechnique in Canada.

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(1) Legget R. F. (1945), "Pleistocene deposits in the Shipshaw area, Québec", Transactions of the Royal Society of Canada, Series 3, Section IV, 39, p. 27-39.

(2) Radforth N. W. (1945), "Report on the spore and pollen constituents of peat bed in the Shipshaw area, Québec," Transactions of the Royal Society of Canada, Series 3, SectionIV, 39, pp. 131-142.

(3) Legget R. F., and P. LaSalle (1978), "Soil Studies at Shipshaw, Quebéc: 1941 and 1969", Canadian Geotechnical Journal, 15, pp. 556-564.

Throughout the war years I was asked to undertake a number of smaller consultancies, consistent with the prior claims of universuty work. Some requests came through C.R. Young, others directly to me. All involved soil problems in some way but there is no point in attempting to list them since they were all relatively simple problems. I did my best to keep good notes and, after the experience of the Shipshaw paper to the Royal Society and the welcome that it got in then (then) Geological Section, I wrote up two short papers after the war about two of these jobs since they seemed to be useful geological contributions (14) and (15). I list them here since the Transactions of the Royal Society of Canada would not be thought of as containing any early geotechnical contributions! As the war neared its end, Mr. Chadwick, of the Foundation Company (see p.13) wanted Canada to be ready in this new field and so invited Dr. Terzaghi to come and spend a day in Montreal, as his guest, and to present a paper to the Montreal Branch of the Engineering Institute. When his invitation was accepted and the day fixed, "The Chief" got in touch with me and asked me to come to Montreal as his guest also, in order to keep company with Dr. Terzaghi throughout the day. This was on 12 October 1944. It started with lunch at the

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(14) Legget R. F. (1946), "A note on Plesitocene deposits of the Sarnia district", Transactions of the Royal Society of Canada, Series 3, Section IV, 40, pp. 33-40.

(15) Legget R. F. (1948), "A note on Plesitocene deposits near Three Rivers, Québec, " Transactions of the Royal Society of Canada, Series 3, Section IV, 42, pp.55-60.

St. James' Club; there was a fine dinner at the ~~Royal Montreal Golf Club~~ Windsor Hotel (to which The Chief invited Mrs. Legget and one or two other ladies) - preceding the E.I.C. meeting. During the long afetrnoon, I had Dr. Terzaghi to myself. We walked up Mount Royal, by which he was delighted, and spent some time at the famous lookout, just talking. It was then that, in effect, he gave me the challenge of my life, as related in my Terzaghi Lecture to A.S.C.E. (16). Naturally, it was a day never to be forgotten; much of what I was able to do in this field after that was due to the inspiration of that being with him then gave me.

There was another by-product of the Shipshaw job. Dr. Acres asked me to go and have a look at the La Tuque development of the (then) Shawinigan Power Company, on the St. Maurice River, since he had heard that they had an unusual groundwater situation there and he wanted to know about it, just in case we ran into anything similar at Shipshw. There was indeed an unusual groundwater situation at La Tuque, on which I duly reported to Dr. Acres; we did not have any comparable situation at Shipshaw, but the visit opened my eyes to the significance of groundwater in all geotechnical site studies. This was reinforced by a minor but comparable problem on the Forestville project, for which I was consultant to the Anglo-Canadian Pulp and Paper Company. Fortunately, I was able to keep good notes so that many years later, after finding again and again how groundwater was neglected, I wrote them up into a paper for A.S.C.E. (17).

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(16) Legget R. F. (1979), "Geology and Geotechnical Engineering", Proc. Am. Soc. Civil Engs., 105, GT3, PP.339-391.

(17) Legget R. F. (1962), "Experiences with Groundwater on Construction" Proc. Am. Soc. Civil Engs., 88 SM2, pp 1-17

This was, I think, the only geotechnical paper I had published which elicited no official discussion at all - confirming just what I thought was the general neglect of groundwater. I did get one or two personal letters commending the paper, one man saying he wished that he had ~~wrote~~ written it, but it really did not "make a ripple on the waters" and groundwater continues to be (in my opinion) the Cinderella of Geotechnique.

Two major wartime jobs finally call for mention; both of them went on after the war, the second for many years, but they started under the pressure of the wartime effort and so call for mention here. The first was the construction of the Polymer synthetic rubber plant at Sarnia, another superb Canadian wartime project which was never written up (as it should have been) since all of those closely connected with it went on to other high-pressure jobs. When the Japanese over-ran the Far East, supplies of rubber were cut off from the allied forces. Manufacture of artificial rubber immediately took on the highest possible priority. The United States planned to build all six emergency (Synthetic rubber) plants themselves until Rt. Hon. C. D. Howe (as he once confirmed to me) made a dramatic move in Washington, ~~which~~ This won for Canada the very grudging consent of the U. S. wartime production chief for the privilege of building the sixth plant, the only one to make rubber by the two known methods, instead of by just one method as all the five U. S. plants. Canada started last, therefore. Canadians generally should know that the Polymer plant was constructed so well and so quickly (Operating within eleven months) that the first carload of Styrene made there left (in a great hurry) to help the first U. S. plant start up its production. Behind that achievement there naturally stands a superb job of organisation and cooperative engineering and construction.

To direct this great construction enterprise, Mr. Howe borrowed the services of Mr. R. L. Hearn (now Dr.), then Chief Engineer of Ontario Hydro. At the peak of construction, Mr. Hearn had working on the one-mile square site at Sarnia about thirty contractors and something like a dozen consulting engineers. All the plant designs came from the U. S., but the site preparation and all services including roads, a large steam power house, pumping plant etc., were designed by the Acres office. Work went on steadily six days a week, twenty four hours a day; Sundays were to be taken off by everyone, except the maintenance gangs who, in this way, kept all construction plant at top efficiency. To achieve ths result, Mr. Hearn had six special assistants, all engineers, one mechanical, one electrical, one chemical, one petroleum, and one civil, the sixth being a sort of "engineering office-boy" to do jobs that fell outside of the fields of the other five. It was my privilege to occupy this lowly position, working most weekends, and nights when necessary, while still carrying on with University work. And early Geotechnique was there! The civil assistant to Mr. Hearn was Jim Knight, staff engineer with the company that then supplied calcium chloride; Jim had made a real study of soil stabilisation using CaCl. Mr. Hearn's vision and standing as an engineer is shown by the fact that, after briefing by Jim, he laid down as a firm rule that, despite the intense urgency of the job, nothing should be done on the site until all construction roads had been laid out, and constructed, using stabilised soil, under Jim Knight's direction. Well do I recall the criticism to which R.L.H. was subjected by impatient contractors but he stuck to his guns, and all construction roads were finished before access to the site was granted; in the result, not five minutes was ever lost on the job because of road failures despite the heavy construction traffic

(I am quite sure, in memory, that Jim Knight wrote a little paper on this road work and I think that it was published either in The Canadian Engineer if this had not, by that time split up, or in one of its sections called Roads and Bridges. Unfortunately, my notes do not contain this reference and, if I still have a copy, it has disappeared into my wondrous filing system, so far unretrieved! But in any bibliography that may be prepared for the start of Geotechnique in Canada, this paper should have an honoured place.)

The geology of the site (part of the old Indian reservation at Sarnia) was straightforward so that all foundations, I think, were on the local clay-till. I recall doing some soil tests of this for Acres but they were responsible for all foundation designs, including that of the power house, the largest steam plant in Canada. After much discussion, and partly because of necessary speed, it was founded on a "floating foundation" which must have been one of the first, if not the first large foundation structure of this type in Canada. Memory tells me that there was a paper written on this part of the job by an engineer named Hvilivitsky, a fine member of the Acres team who died young, I think. I will see if I can locate this and if so, will footnote it as a further early paper of importance, even though I recall being a bit dubious about the settlement observations that it contained. (12) The power house is still there, although now oil (or gas?) fired instead of with coal, as originally built; it might be a job worth following up if someone is looking for a graduate thesis subject!

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(12) Ings J. H. and J. Hvilivitsky (1944), "Some Structural Features of the Polymer Corporation Steam Power Plant," Engineering Journal, 44, pp. 394-399, July 1944.

The coal supply, of 600,000 tons per year, raised one of the biggest policy questions in design, solved (to cut a long story short) by making it the first steam plant on the Great Lakes to have its entire coal supply handled by earth moving equipment without of the usual travelling gantry etc. equipment, a development which I wrote up for A. S. M. E. but which is hardly relevent here. What is a part of the geotechnical studies was the plugging of the large coal bunkers in the power house. After a man's life was tragically lost through this (and not following instructions), I was asked to look into the problem. I set up a small experiemtal model plant in the U. of T. soil mechanics lab., and eventually found the cause (moisture content) and so was able to suggest a solution.(19) In this work I was helped by a graduate assistant whose name was Donald H. Macdonald - the beginning of a lasting and valued friendship!

"Polymer", therefore, has an important place in this story, as also does Steep Rock. This was the name of a high quality iron ore deposit found, by geophysical methods, beneathe the bed of Steel Rock Lake, near Atikokan in western Ontario. In 1943, with supplies of iron ore in a critical state, the decision was made, jointly by the Canadian and U. S. Governments, to devlope this valuable deposit even though it meant draining the Lake. Diversion works were necessary (a tunnel and big cut) before both ends of the Lake could be sealed off; these were undertaken in 1943 (Acres being the consulting engineers) so that the very large floating pumping plant started operation in the late winter of 1944, while the surface of the Lake was still frozen. Water level had been dropped about 75 feet when the spring thaw started and then things began to happen. I got a frantic telephone call on Good Friday 1944, asking me to get up to the Mine as quickly as I could.

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(19) Legget R. F. (1947), "Clogging of Bituminous Coal in Bunkers," Transactions of the American Society of Mechanical Engineers, 69 pp. 525-533.

When I got there, I was treated to one of the most remarkable sights I have ever seen in my life. It was a lovely spring day when we got to the south end of the Lake, the ice cover glistening in the bright sunshine, trickles of water all around testifying to the thaw that was then well under weigh. Due to the drop in water level, the ice-sheet was broken all around the edge of the Lake, exposing the lake-bed sedimemts. As these thawed, and because of their unstable condition, they slid into the lake - so I saw landslides innumerable, all round the Lake as far as the eye could see. I recall one very large one (200,000 to 300,000 cubic yards) taking place close to us, going out in a few seconds with an awe-inspiring "shoosh". Quite naturally, there was panic among the mining engineers who, in the general pattern of mining work, had just regarded soil as a nuisance. It took quite a time to persuade them that the slides were a perfectly natural phenomenon and must be allowed to take their course. I need not go into any detail but an incident with ground water, which I had asked them to cntrol at one critical spot on the edge of the Lake but which they neglected, finally showed them that this was soil that had to be respected. After that, cooperation was all that could be desired. They followed every suggestion I made, the climax coming when I had to move to Ottawa (in 1947) and they asked me please to continue to use their soil as a field laboratory for any research work I wanted to do - and this we did. Since my connection with Steep Rock lasted, as indicated, until after my move to Ottawa, this was one job that I did manage to "write up" (20)

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(20) Legget R. F. (1958), "Soil Engineering at Steep Rock Iron Mines, Ontario, Canada," Proc. Institution of Civil Engineers, 11, p. 169; discussion in 13 (1959) pp. 93- 117.

After the open pit mining operation was in successful operation, the Steep Rock Company leased the north-east part of the deposit to the Caland Company for whom Dr. R. M. Hardy acted as consultant. By chance, but unfortunately, Bob Hardy and I never met at Steep rock but he contributed notably to the discussion of the paper just noted, and we naturally had private talks about the complexities of the job.* This was, probably, the first linking of geotecnical work in eastern and western Canada. It may not be amiss to record that I was given to understand, privately, that in both cases, the U. S. partners wanted to bring in U. S. soil consultants (in what may still be called the usual way) but they were advised that there were Canadians who could serve as needed.

It will readily be appreciated that the transition from wartime work to peacetme activities was a gradual process with much overlapping. Some indication has been given of the imperative of war in the jobs described, Shipshaw perhaps most of all, where completing the job in the shortest possible time was the prime objective, over-riding all else. The biggest change to peace-time work was the removal of this imperative, and a chance to look for the best solution, rather than the quickest. And this did not happen overnight since, until 1946 the threat of war was still in the air, as the record of the Associate Committee will demonstrate. This part of the record has been kept for separate treatment, even though, chronologically, it is interspersed with some of the jobs already described. For convenience, therefore, the war-time record will stop here, and the beginnings of peace-time geotechnical work, as I saw it, briefly related until I get to the time when others can continue the record.

At a later date, we did author a short joint paper (to the Geological Society of America) on a boulder which was found embedded in the varved clay found beneath Steep Rock Lake. Unfortunately, I cannot find the reference to this; possibly Bob Hardy will be able to add it to the record. But we were joint authors of a much more significant paper which concluded the volume on Soils in Canada, the record of what was probably the first general symposium on Soils in Canada, held at the annual meeting of the Royal Society in Kingston in June 1960 (1). Published as Special Publication No. 3 by the Royal Society in 1961; a revised edition was published in 1965, and this was reprinted in 1968 and 1971, making it one of the most successful volumes ever to be published by the Society.

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(1) Legget R. F. and R. M. Hardy, (1961), "Engineering Significance of Soils in Canada", in Soils in Canada, edited by R. F. Legget, Special Publication No;3 of the Royal Society of Canada (240pp), pp. 218-229).

THE POST-WAR YEARS (1945 to 1947)

As the war neared its end, there was a good deal of sound thinking and planning for the future in Canada, largely stimulated by Rt. Hon. C. D. Howe whose early appointment as Minister of Reconstruction (amongst other things) was one of the Government's wisest moves. Reconstruction Committees at the top level were established at the national level under Principal James (of McGill University) and Principal Wallace (of Queen's) for Ontario. I think that the record would show that the Ontario Committee was the more effective. In keeping with these moves of a major nature, I was approached by some of the engineers who had seen, during war jobs, what Soil Mechanics (as it was then called) had to offer. Dr. R. I. Hearn was one, I know, but there were others. Their request boiled down to the same general message - when they graduated Soil Mechanics had not been thought of; they could see its value and wanted tobe able to use this new approach to soils; would I not put on a Short Course at the University so that they could at least learn the elements.

As can be well imagined, I was not looking for things to do but the requests were so insistent and from such notable men that I discussed the matter with C. R. Young, still my mentor even though (I think) he was now Dean; the Department had a head who had been away for much of the war and who was not interested in the practice of civil engineering. C.R. agreed that it really should be done so I got busy and planned a five-day (week-long) course of lectures, with simple laboratory sessions in the evenings. When I had this ready, and in conjunction with the University's Extension Department, we placed a note about it in the Daily Commercial News,

the excellent construction newspaper published in Toronto. Within 24 hours, the course was sold out, to our amazement. We had had to limit the number of registrations to something like ~~125~~ 150, the capacity of the largest lecture room in the Electrical Building (in which the Dept. of Civil Eng. was located). I had grave doubts about reserving such a large room, so modest were my expectations. I need not have worried. What was even more surprising was the calibre of the registrants - Dr. Hearn's was one of the first to be received and he was then General Manager of Ontario Hydro at the start of its great era of expansion; Otto Holden, the Assistant G.M., was another; heads of several consulting firms ... so it was going to be a meeting of friends. It was held from 14 to 18 May 1945.

I had invited one or two close friends to do individual lectures - Lyman Chapman was one, I think, from the Ontario Research Foundation. But here, in matters of detail, my memory fails me and, as I must record with great embarrassment, such was my disregard for the history of Geotechnique in Canada or, at least, about my part in it, that some years ago when the Norwegian Geotechnical Institute issued a call for early records of Geotechnique for their Terzaghi Library, I was stupid enough to send over to them my complete notebook for this course. This contains all my lecture notes (simple as they were) copies of the publicity and registration forms etc., with (I think) a list of registrants. I am so mad with myself for having done this that, after this has been read and if others think it worthwhile, I will gladly write to N. G. I. and ask for my notebook back; let's hope they still have it. In summary, all I can record is that the week was, as far as I could see, a success, judging by the comments I received; certainly I was encouraged, attendance staying at almost 100 per cent to the last hour on the Friday.

As a necessary aside, but to show how thinking was developing, on 13 March 1945 I had an interview with J. P. Miller, the Deputy Minister of the Ontario Department of Highways. With the full approval of C.R. Young, I placed before Mr. Miller the idea of a post-war joint University-Department Highway research unit, similar to the pioneer ~~station~~ Project so successfully operated by Purdue University and the Indiana Highway Department. The Purdue Joint Project was a real pioneer in the highway research field. I had seen it in 1940 and kept in touch with its work, the beginning of a long friendship with Professor K. B. Woods. Miller turned me down flat, even going so far as to say that he was not interested in "messing about with mud" when I tried to tell him what Soil Mechanics could then do in Highway design and construction. So that was that; I have often wondered what might have developed had there been someone ~~else~~ for me to talk with other than Mr. Miller. For entirely different reaons, Mr. Miller ceased to be the Deputy Minister not too long after this visit. A new Deputy Minister was appointed and the Department started on its post-war programme which was carried out with such distinction - and with all the aid that Soil Mechanics could give, as I watched with unusual interest from afar in Ottawa!

Meantime, other things were happening as hopes about the end of the fighting escalated. Late in 1944 I got a telephone call from Major H. W. Tate, assistant General Manager of the Toronto Transportation Commission (as it then was), an almost mythical character and a great man. He was reputed to know the name of every employee of the T. T. C.; certainly when I went round with him, he would speak to all the men we saw by name - and they worshipped him! He was a man of vision; happily he lived to a great age and so was able to see more than the fulfillment of some of his dreams. He asked me to come and talk to him, which I did, finding that the Commission, under his urging, had agreed to start studies for a subway. I said that I thought that Toronto was not yet ready for such a development; he said that it would be ten years before any subway would be ready (right on the nose; it was exactly ten years from the day of our talk) and that, by then, Toronto would be ready. He was absolutely right; I was equally and completely wrong!

So "H. W." wanted to know how you started to find out about ground conditions in which the subway would be built, ending up by asking if I would be consultant to the T. T.C. for the study. C. R. Young agreed and so began one of my most rewarding consulting jobs. Being so close to the University, it was easy to fit in with my University duties, necessitating no absences from classes. I went first to the City Hall and asked if I could see their records of the urban geology. They did not know what I was talking about and had precisely nothing. So began my interest in Urban Geology, still developing so many years later! I had to get busy and study the few reports available from the Ontario Department of Mines, notably by the great Dr. A. P. Coleman; then I went round to all interested

consulting engineers and solicited their help, always gladly given. I discovered that one consultant, Gordon Wallce (later the firm of Wallace and Caruthers), in order to keep his small staff busy during the depression, had kept them busy studying old city records and plans, on the basis of which the office had prepared a fine map of all the ravines that once interlaced the whole area now occupied by the city. The main ravines are still there; the smaller ones had all been covered up. This was a real find and most helpful. (It has not yet been published but has been used - I think - by Owen White and his staff in the Ontario Geological Survey.)

The overall pattern of the underlying geology gradually developed but the Commission accepted my recommendation that, despite this, test borings must be put down to confirm what we thought the geology was. There were no test boring companies at that time but my friends in the Department of Public Works, Canada, very kindly arranged for one of their rigs to be rented to the Commission for this clearly urgent work. In locating the test holes, I made another "discovery" - of the consultative committee that maintains the detailed records of all underground utilities in Toronto, meticulously kept and, as we discovered, accurate to within an inch or ~~two~~. The borings were put down, confirming the overall picture; one was drilled deep enough to tap sub-artesian water that had caused "quicksand" in one of the deep foundations of which I had been told; A few were cased and groundwater levels read regularly for over a year, gving absolutely vital information. (In one we found gasoline, to general consternation, until it was traced to a leaking tank at a filling station). Samples of all soils encountered were carefully kept and cores of the underlying shale.

The Chief Engineer to the Commission for the subway work accepted my suggestion that not only should essential subsurface information be fully integrated into the contract drawings and documents, but that all the information we had obtained should be made available to all those tendering on the work. At the time, this seemed to me to be just plain common sense but we soon discovered (from the comments of tenderers) that it was so rare a procedure as to be phenomenal! Today it is fairly regular practice - but the efficacy of this early example was shown when the tenders came in, all very close to one another and to the engineer's estimate; as also by the final settlement of the contract, with no difficulties about major claims, the entire matter settled in one happy session in the Chief Engineer's office between him and the General Manager of the contractors' consortium. The material was very well displayed in a separate room set aside for this purpose in the old T. T. C. head office building. (I was then in Ottawa and so had nothing to do with the final arrangements).

I venture to give these details because of another real "blind spot" of mine. At the time, I gave no thought to the fact that this was pioneer work, and that the example of what we did for the first Toronto subway might be of help and service to other Canadian cities when they came to build their subways ... and so I did not "write it up" at the time. I can offer many excuses, the main one being that I was so very busy - with University work and the Ottawa research, yet to be mentioned - but the fact remains that this was an opportunity missed. Another factor in the overall picture was that the Advisory Geological Committee, to which the T. T. C. willingly agreed, did not function quite as effectively as we had hoped. One key man died

tragically while work was in progress; another proved to be quite uncooperative, to our total surprise. Some geological papers did result from the work but not the series of papers which the Advisory Committee had originally envisaged. The concept of the Advisory Committe remains (in my view) a sound one, well worth adopting on other major comparable construction jobs.

To this I should hasten to add that, when excavation did start, we made certain that regular suites of soil samples (at every 50 feet, I think) were taken and carefully encased, being eventually passed to the Royal Ontario Museum for safe keeping, and to be available for future workers on the Plesitocene Geology of Toronto. I think (and hope) that they are still there. This job was supervised by W. R. (Bill) Schriever, of the newly formed Division of Building Research, N.R.C., who was assigned as subway research officer, resident in Toronto, throughout all the excavation part of the first contract. This followed my departure for Ottawa in the early summer of 1947. When I came to advise the T. T. C. about the necessary break in my connection with them, they urged me to keep in close touch with the work and offered any facilities I wanted when I had got the new Division launched. Thus it came about that having a research officer right on the subway work was to the pleasure of the T. T. C., and in keeping with my resolve to develop building research in Canada in the closest possible liaison with construction operations as well as with design professionals. Towards the end of the 1950s, with Bill Schriever back in Ottawa, I finally realised my mistake in not having written up the T. T. C. work and so we put together a summary paper (23) There was still no Canadian outlet for such papers and so I made another error, by sending it to a British trade journal, the Editor

(23) Legget R.F. and W.R. Schriever (1958) 'Site Investigations for Subway Floor

of which had been "after me" for some years for a paper on some aspect of the work of DBR/NRC. Even though a summary paper only, it should have been published in Canada; that it was not has long been a matter of regret to me. But we had to wait until 1965 before we had our own journal.

It will have been obvious that my connection with the Toronto subway overlapped my move to Ottawa but I have included it here for convenience. Work at the University of Tronto continued to occupy almost all of my time. We had the first classes of ex-service men in 1944 (what a joy it was to lecture to them). Then came the huge influx, the start of the AJAX satellite campus, journeys to which necessitated 7.00 a.m. starts, but it was an exciting and challenging time. It left little opportunity for any outside activity, but two more special consultancies must be in this record.

The end of the war saw the buiding of the first new hospitals in Canada for many years. One of these was the first (main) section of the new Sick Children's Hospital of Toronto on University Avenue. The architect was "Jimmy" Govan, a small Scots Canadian with real expertise in hospital design, and a delightful man. He was anxious, as always, to do a first class job. I can not recall who was doing the structural design for the building (possibly in his own office; memory fails me) but there was the usual talk about piles. Mr. Govan had a hunch that this was wrong and so asked me to help. It did not take long to find that the site was underlain by till, nor to have this checked by one or two test holes; even they confirmed the presence of boulders. I did some shear tests on the till and was able to show that a floating concrete slab foundation would be perfectly safe. "Never done before in Toronto" was the immediate

reaction; Mr. Govan had to take a lot of "flack" but he stuck to his guns, designs were completed and the contract let. When the Hospital's Board of Directors (mainly financial men from "down town" because of the big fund-raising effort) heard that the building was to "float" and not be firmly founded on piles, there was consternation. Over Mr. Govan's head, they consulted Stone and Webster who, with their financial connections, had just established their Canadian office. They had to refer the matter to their Boston head office; in due course I had to waste a lot of my time telling men from the S & W office the basis of the design. Well after a year since they were consulted, they submitted their report, which Mr. Govan and I did not see but which, apparently, confirmed all aspects of the design since nothing more was heard about piles.

The building was, I think, the first major building in Toronto to be so founded; it was soon followed by others including (I think) the Mount Sinai Hospital on the other side of University Avenue, as one of the first field investigations by the new Division. The results were well written up by Carl Crawford and Ken Burn, and in their paper, they refer briefly to the similar studies at the Sick Children's Hospital (24). I am fairly sure that stainless steel plugs will be found embedded in the "Sick Kids" basement columns, if ever anyone has the time and inclination to look!

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(24) Crawford C. B., and K. N. Burn (1962), "Settlement Studies on the Mount Sinai Hospital, Toronto, " Engineering Journal 45, p. 31-37, December 1962.

From the numerous other small consultancies - all involving just my advice and nothing in the way of design, in keeping with C.R's guide-lines - one only can be mentioned since it was an unique assignment. Just after the end of the war, Ontario Hydro were able to complete two long-planned diversions of water from the Nelson River watershed, draining into Hudson Bay, into Lake Superior. One of these (the Long Lac diversion, I think) increased the flow of the Aguasabon River, entering Lake Superior at Terrace Bay, to such an extent that it was economical to develop power at the ideal site near the river's mouth. By constructing a small dam in a rocky gorge, water level of the impounded reservoir was such that it flooded through a narrow defile into a huge area, the southern edge of which was only about half a mile (or less) from the shore of the Lake. A concrete intake structure could be built on bedrock from which a tunnel in rock could be driven to near the Lake shore where short penstocks would lead to a small power house, discharging almost directly into the Lake. It really was, and is, a beautiful little water power project. It was planned, and construction was well started while Hydro, with their usual care, carried out a massive programme of test drilling along the ridge which separated the reservoir from the Lake, on which was located the main line of the Canadian Pacific Railway (and now the Trans-Canada highway). Test drilling was difficult through the sand and gravel because of the presence of boulders, but the work was pressed forward.

First one, then several holes were completed, all in sand and gravel, but they went well below the level of the Lake without striking bedrock. Reaction of Hydro managers can be imagined; there was fear that, despite all the work completed, the entire

project might have to be abandoned. Dr. Hearn was General Manager. One day he called me and asked if I would go and have a look at the site to see what I thought. (I was told, later, that this was the first time in then recent history that Hydro had gone outside their own staff for engineering advice). I went up to Aguasabon and had a very good look round the entire site, ably guided by John Gorman. It seemed pretty hopeless but after "sleeping on it" I had another look and asked myself why there was a small lagoon ("Blue Jay Lake") in the centre of the depression to be filled by the reservoir. It was not very large, or deep, being surrounded by marshy ground. Then I recalled the changing level of Lake Superior since the ice receded, so well shown by the terraces after which the Bay is named, so I wondered if beneath Blue Jay Lake there might not be the bed of an old glacial lake. It did not take us long to find out that there was - blue clay-silt, clearly impermeable, all round the little lake. Auger borings showed it to be two or three metres deep; later exploration, with all holes very carefully plugged up, confirmed its existence over the whole area to be occupied by the impounded water, just as if Nature had arranged it specially. Nobody could be absolutely sure that the clay lining (so to speak) was continuous but it was decided to take a chance and carry on. I shall never forget the worry of waiting for the first reports of observations in the wells, specially cased, as the water level of the reservoir rose. There was a slight rise of groundwater level some 100m beneath the reservoir, but this soon stabilised and the plant has been in operation ever since. And this time, so unusual was the case, I did write it up, but again, of necessity, in an overseas journal (22). (24)

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(24) (22) Legget R. F. (1953), "A Perched Reservoir in Northern Ontario, Canada", Geotechnique, 3, pp. 259-265.

OTTAWA (1947 and on)

Building Research must have come first to my attention through meeting Cooling at the 1936 Conference. The Building Research Station (of Great Britain) was then well established and continued as the only national building research organisation in the world until 1946-7 i.e. after the second world war. I must have seen pre-war references to "B.R.S" and also to Dr Stradling (later Sir Reginald) but I did not know (naturally) that he had paid two pre-war visits to Canada to discuss with General McNaughton, then President of the National Research Council, the possibility of Canada aiding B. R. S. with its cold weather work! Preparation of the first National Building Code of Canada, started in 1939, naturally drew attention to problems in building that needed investigation but, although the Code was finished (in 1941), the war naturally clouded all such extraneous possibilities.

Accordingly when, in the spring of 1946, I was called upon in connection with [illegible] to make an official visit to Switzerland, travelling by way of London, Dr. Mackenzie, the Acting President of N. R. C., knowing of the contacts I had in England, asked me to look into the "building research situation" in Great Britain with special reference to their post-war plans and to report to him on my return. This I did, being introduced to Sir Reginald Stradling by one of my former lecturers at the University of Liverpool, now one of his chief aides, ~~and he~~ Sir Reginald gave me a wonderful over-view of the history and future of building research in the U.K. On the basis of this, and other information which I was able to pick up in my few days in London, I was able to

prepare a Report for Dr. Mackenzie within a few weeks of my return to Canada. He told me later that, deliberately, he did not read it before he himself went on a visit to the United Kingdom when he made his own inquiries. When he did read my report, on his return, he was good enough to tell me that he was grateful for it, since it saved him from writing a report of his own, his conclusions being the same as mine. Essentially these were that the twenty-one years experience of B.R.S. had proved the value of building research beyond doubt; that all such research must be carried out in the closest liaison with the construction industry and design professions; that with the prospective post-war "building boom" in Canada even then being envisaged, some sort of research in the building field was desirable for Canada; and that, if the National Building Code was to be kept up to date, then building research must somehow be started here.

Later that year (1946) a large meeting was convened in Ottawa, under the auspices of the Department of Reconstruction (I think) to discuss the specific research needs of post-war housing in Canada, then already seen to be a matter of high priority. I was invited and was an interested observer to the jockeying for position on the part of representatives of intterested agencies (especially the Mines Branch and the Forest Products Laboratories). I kept quiet, especially when the discussion got round to the idea of an inter-departmental committee on housing research since, even then, I had seen some of the difficulties of actually doing research under the aegis of a committee. In all this, never once did the very idea of my having anything to do with building research in Canada cross my mind; I was far too interested (as can be imagined) at the possibilities I could see opening up for Soil Mechanics (Geotechnical) research at the University of Toronto.

A little time after this meeting, I was in Ottawa again on Associate Committee work. After I had made my regular report to Dr. Mackenzie on the progress of the track research, he surprised me by asking what I thought of the housing research meeting and the conclusion of the meeting. I indicated my reservations about the idea of a committee, no matter how good; he agreed and told me that he had already taken to the Council the concept of setting up a new Division of Building Research within the NRC organisation. Later, he told me the idea had been approved by Mr. Howe who was then engaged in setting up Central Mortgage and Housing Corporation, and saw that a research organisation for building within NRC would be a fitting complement. (Being the man he was C. D. Howe knew instinctively that you can not really do research within an operating organisation; others in Ottawa had to make this discovery the hard way). When the Council had made its decision, then Dr. Mackenzie asked me to help him to find the right man to be the first Director. I have to smile when I think of the names we discussed so privately and so frankly. I was, however, completey unprepared for what happened on 7 Jan 1948 when "C. J." got up from his desk, walked to the window (I can see him now) turned round and said "Legget, we're wasting our time; you should be the Director". Never having dreamed of this, I gave him, off the top of my head, all the reasons I could then think of as to why I should not be this pioneer, one being that I was anxious to get on with geotechnical research. "That's part of building research," was his answer to that! He told me to go away and think about it which I did, with my wife; it was an agonizing decision to have to reach but after several weeks we decided to accept.

Dean Young (as he then was) was most understanding; the letter he sent me as I left is one of my treasures. But leaving the University, our friends and the lovely house we had in Rosedale was not easy. It was done, however, in rather hectic weeks and, after a short holiday in the United Kingdom (when I spent a day at B.R.S. and my long and close friendship with their new Director Dr. (now Sir Frederick) Lea was firmly established), I reported for duty at the Sussex Drive building of the Council on Monday 1 August 1947. Those who have managed to read this far will probably think tthat it is just another of "Legget's tall stories" when I state that, ~~whtin~~ within two hours of entering the doors of N.R.C., I was handed the first enquiry to DBR/NRC - and it was a geotechnical problem! It was a foundation problem at Chalk River and I had to go up there about it within a day or two, so I had my "baptism of fire" ... and it never stopped.

Inquiries soon were coming in at an increasing rate, once the existence of the Division was known, eventually to the extent of several thousand every year. (The Division used to get more mail than all the other Council Divisions and activities at the Montreal Road Laboratories). I mention this for a good reason. If it not been possible to "sit back and think" a bit about the course to be charted, I would have been sunk in a mass of paper. So in the course of the first year, after visiting all the interested organisations in ~~Ottawa~~ Canada and the U.S.A. and starting what proved to be most happy liaisons, a plan of action was hammered out, with Dr. Mackenzie "looking over my shoulder"; he never interfered but, at first, I reported to him about once a week, while he gradually pushed me off on my own.

Through knowing Fred Lea, and following an early visit by the newly appointed Director of B.R. for South Africa, as well as a visit of mine to Washington where the National Bureau of Standards started their Division of B.R. in the same month as did Canada, it became very clear that Building Research, to be fully effective, had to be developed internationally. This was done, through regular five-yearly meetings of the Directors of B.R. in the English-speaking world (all personal friends) and, on the broader canvas, through C.I.B., (le Conseil International du Bâtiment). Even today, I think that it can still be said (as it certainly could in my day) that no branch of applied research is so truly international in coverage, and in integrated activity, than is building research.

This looks like growing into a history of DBR/NRC: not so! The foregoing explanation, however, is necessary to explain why it was that, in hammering out the overall policy for DBR, it was possible to concentrate upon only those branches of work that must be done in Canada, leaving to other countries problems which, although relevant to Canada, could better be done elsewhere e.g. problems of buildings in hot climates, whereas all agreed that Canada must take on building problems of cold climates. This resulted in a policy of having six major branches of activity - Foundations and Soils (since you can't study Canadian soils in South Africa), Snow and Ice (under Dr. Mackenzie's urging, since no research in this vital field was then even contemplated elsewhere in Canada), and Building problems of the North (the North of Canada being one of this country's unique responsibilities) ... all geotechnical, and not because of my own personal interests! The other three?

Fire research (Canada's fire record being a national disgrace), the whole matter of building materials in our climate, and an over-riding concern for the enclosure of space, again in the Canadian cliamte (and this included housing research, roofs, insulation etc. .. the sort of things that come first to mind when building research is mentioned). In all these fields, the objective was the same - to deal with immediate problems and, by developing extertise in these fields, to act as a catylist in getting work started elsewhere in Canada, especially at Universities.

The work of DBR/NRC has, fortunately, been written up, supplementing its own long list of publications, starting with Ten Years of Building Research. (We "had no time" before 1957 to contemplate putting together such a record.) Accordingly, in this record it is necessary for me only to touch upon some of the more personal aspects of each of the four main branches of Geotechnique as they were tackled in the Division. Muskeg received the least attention, in view of the extensive programme of Dr. Radforth, first at McMaster University and then U.N.B., but Ivan MacFarlane was the one research officer who, in the (old) Soil Mechanics section did excellent work on muskeg and produced some often-cited publications before he went on to other work. Snow and ice research was slow in starting, really requiring the cold-room facilities of the BR Centre (opened in 1952) before laboratory work could commence. But the visit, for one year, of Dr. de Quervain of Switzerland, as a guest worker with O&C, produced a masterly report on what Canada should be doing in these fields that is still, in a way, the "Bible" for this important activity! (25) D. C. Pearce was the first research officer, followed by I. W. Gold, now the Associate Director of the Division.

M. R. de Quervain (1950), "Snow and Ice Problems in Canada and the United States", DBR/NRC Technical Report No. 15.

Very shortly after I arrived in Ottawa, I met Dr. O. M. Solandt, then Chairman of the newly formed Defence Research Board (an offshoot from NRC in the post-war reorganisation). We hit it off and so had a number of useful talks about our respective spheres of action and immediate plans. The North of Canada naturally came into our talks even though Exercise Muskox was then the only National Defence northern activity (I think), and there had been practically no civilian activity other than the normal work of the R.C.M.P. and Hudson's Bay Company, and the few small mines down the Mackenzie. We both agreed that, taking the long view, this was an area that must be looked into and Dr. Solandt encouraged me to get busy on finding out what we could about building in the North and especially about permafrost, the word then just coming into use to describe the perennially frozen condition of the ground in the North. That was the start of the still-continuing northern programme of DBR, this in 1947.

In my hunt for staff, I was lucky enough to come across John Pihlainen, a Finnish-Canadian civil engineering graduate of McGill who had spent two summers in Labrador (I think) and so had got the "Northern bug", a reflection possibly of his own Finnish background. He joined the Division in 1949 and by the summer of 1950 it proved possible to organise an expedition, jointly with DND, down the Mackenzie River to the Arctic coast for a study of every building in the Mackenzie Valley in order to determine what were the

secial building problems of the North. The result was a useful report, jokingly known within DBR as "The Doomsday Book of the North"(25).

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(25) J. A. Pihlainen (1951), "Building Foundations on Permafrost" DBR Technical Paper No. 8, DBR No. 22, 42 pp.

That was the beginning of permafrost research in Canada, now often forgotten by new workers in the field. A small field station was established in 1952 using ramshackle left-overs from the Canol project at Norman Wells, N.W.T. A well built station followed, being officially opened in 1956 by Dr. E. W. R. Steacie, President of the Council, during the course of the first tour of the North ever made by members of the Council, the opening ceremony being high-lighted by one of the best of the many humourous stories that developed in the early history of DBR..

I have left the start of Soil Mechanics research to the last since it was allied, through the people involved, with the Associate Committee. On the staff of the Division of Mechanical Engineering, when I arrived in August 1947, was F. I. Peckover who had been a student with me at the University of Toronto. I think (but FLP can check me on this) that Mr. J. H. Parkin, Director of the Division of Mechanical Engineering, had some idea of starting soil mechanics research in his Division before it was known that DBR was to be established. There was no difficulty in having "Peck" transferred to the staff of the new Division, of which he therefore became the first Research Officer. Mr. Parkin kindly made space available for the start of a soil mechanics laboratory in his Hydraulics Building and here the first laboratory of the Division was established by early 1948. The winter

of 1947-48 was one of the most severe Ottawa has ever had and so we were called upon for help by the City Waterworks Engineer. It was also the first winter during whuch snow had been cleared from city streets and so there was much trouble with frozen pipes, leading us into a study of soil temperatures, at that time almost a virgin field for research! The first published research paper of the Division was one written jointly by FLP and RFL which was presented to the 29th. meeting of the (U.S.) Highway Research Board on "Soil Temperature studies", the beginning of what is now a long series of notable research papers in the field of Soil Mechanics.(*) By a strange coincidence, I had called on Dr. John Paterson, Head of Canada's Weather Service, in Toronto, on20 February 1945 since I had seen by then that soil temperature studied would be vital in Northern research. They had some very early records

(*) R. F. Legget and F. L. Peckover (1949), "Soil temperature studies - a progress report", Proc. Highway Research Board, 29th. meeting, Washington, pp. 434-445.

After getting the Soil Mechanics lab well set up and the section well organised, and following notable service as the first Secretary of the Associate Committee, F.I.P. felt the need for actual experience in the field and so, by mutual consent, left the Division in order to accept the position (to my delight) of Chief Soils Engineer on the construction of the St. Lewrence Seaway. His work here was notable and he published a splendid paper summarising this experience before joining Canadian National Railways, where he served until his early retirement. He was succeeded as head of the Division's Soil Mechanics section by Carl B. Crawford, now the Director of the Division, the record of whose fine work is well documented in DBR reports. I know that he would join with me in this brief tribute to Peckover's pioneer work as one of the very earliest Canadian workers in the geotechnical research field.

Finally a very brief personal note, since here is where I really bowed out of active participation in geotechnical research. When I was appointed, Dr. Mackenzie got me to promise him that, despite all the claims that administering the new Division would make on me, I would never let a day go by without at least brief contact with my main personal interest, geotechnical research; he had seen "too many" Directors lose all touch with active research. I kept my promise, although with great difficulty in those early hectic months, my "contact" being merely keeping in touch with what literature there then was. I looked forward to doing at least a little experimental work when the Division was organised, staff recruited and the Building Research Centre in full use. So in the planning of the building, I asked the Architect to include a small laboratory bench with sink in the small working office for me which he included in his plans just off the Board Room, the Director's "official office". We moved into the building on schedule; in due course the organisation of the Division was developed but the pressures got worse (if possible) and so continued until my retirement. I think I used the little lab. bench for one experiment! The bench was used mainly for holding some of the piles of "paper" which had to be dealt with. And so, reluctantly, I had to accept the fact that my research days were over, apart from keeping in as close touch as possible with the work that Carl Crawford and his colleagues were doing, occasionally joining in as a joint author, but only when I had had some definite part in the work being reported, generally in discussion of ideas or in more general field studies. The research achievements of the Division were all the work of others, work which gave me increasing delight as I watched over it and did my best to get the necessary support. And chairmanship of the Associate Committee kept me in touch with the broader field. To the story of the [illegible]

THE ASSOCIATE COMMITTEE

The Associate Committee on Soil and Snow Mechanics (now Geotechnical Research) was established on 20 April 1945. On that day the first meeting of the newly formed group was held in the Sussex Drive Building of the National Research Council. It was a short meeting (two hours) but it launched the Committee on its urgent wartime task with an indication that, in due course, it would be transformed into an "ordinary" Associate Committee for the purpose indicated by its name. There is, in the office of the Committee of today, a complete set of Minutes of meetings of the Committee, and of its publications which may therefore be consulted for any gaps that may have to be filled. It seems desirable that I should try to record the things about the Committee, such as its initiation, that are not in the Minutes. Again, I regret that I did not have enough sense to keep a good record of these developments at the time but from my work diary, aided by a reasonable memory, I believe the following ~~record~~ to be a "true and correct record". (F. I. Peckover will be able to check at least parts of it.) I was much too close to the operation of the Committee to be able to assess its full impact on the Geotechnical scene in Canada, but I believe that it was a helpful influence. Dr. N. B. Hutcheon watched the work of the Committee from "the outside"; he is a mechanical engineer and was Assistant Director of DBR throughout most of the period of the Committee's peacetime work (under my Chairmanship). Somewhere he wrote an appreciation of the Committee which would be a helpful part of this record since it was by an independent observer unconnected with Geotechnical

work; if I can locate it, I will add it to this memoir as an Appendix. Accordingly, what now follows is a factual record of the initiation of the Committee with some notes on its work etc. to supplement the official Minutes of its proceedings. After that, I will include some general notes about Associate Committees, in the light of our early experience with the one now under review.

We must start at the beginning of the year 1945, a time that can only be described by that over-worked word "hectic" - the days were that indeed. The Allied forces had landed in France on 6 June 1944 (D-day) and had made such fine progress that many thought that the war in Europe would be over by the end of the year. This did not happen, the German counter-offensive starting in the Ardennes on 16 December leading to some of the most bitter fighting of the war. Canada was fully geared up to its maximum war effort, overseas and here in Canada; the demands of war dominated everything but there was hope in the air as the tide of war could be seen to be finally turning, in Europe and in the Far East. Normal annual functions still took place, but on a muted scale, and so the annual meeting of the Engineering Institute of Canada (then a really great engineering meeting, sometimes attended by more than a thousand) was held in Winnipeg on 9-10 February (1945). One session was devoted to papers on some aspects of Soil Mechanics; I believe that this was the first such meeting ever held in Canada. Joint chairmen were Prof. A. E. Macdonald (of the University of Manitoba) and me. The two papers were by Bob Peterson and Gerry Williams, on the use of SoilMechanics by P.F.R.A., and the Manitoba Department of Highways. They aroused much interest and good discussion, both at the meeting and long into the evening privately. This must have been my first meeting with Bob P. and Gerry W., but I had met AEM previously. I think that Bob Hardy was at the meeting. And I am under the impression that either one or both of the papers were later published in the Engineering Journal, in which case they will be two more of the pioneer papers.

~~By chance,~~ Dr. C. J. Mackenzie (President NRC) was then a past President of the Institute and so ~~(I think)~~ he was at Winnipeg; ~~if not, he must have got a full report on the meeting~~ because, when I got back to Toronto, after a visit to Steep Rock following Winnipeg , I found a message asking me to telephone him in Ottawa. This I did, and he asked me to prepare for him a brief review of the "state of the art" in Soil Mechanics, without explaining what this was for; this was on Monday 19 February 1945. I sent a draft of this statement (of which I now regret I have no copy) the next day. When I heard from C J M in reply, he asked me to come up to Ottawa as soon as I could; this I did on Saturday 3 March. ~~I must have been away because~~ I saw Dr. D. C. Rose (a senior scientist in the Division of Physics) who explained to me, in the greatest confidence, the problem that had arisen with tracked vehicles, about which we had a good talk. During the morning I saw Dr. Mackenzie also but I have no record of my talk with him; later ~~Later~~ I saw Professor R. E. Jamieson and Colonel G. L. Letson, (of whom more later).

On 12 April 1945 President Roosevelt died. The shock of this news went all round the free world; those of us who can remember it will recall it as one of the ~~moments~~ things we shall never forget. The media had covered up his serious illness and so we were all unprepared for the news. Nothing had appeared in print, or almost nothing, about his Vice President, and so successor. I was in Hart House, at the University of Toronto, just coming out from a dinner when a man, coming the other way, told me. It was as if "the end of the world had come" since Roosevelt and Churchill had, between them, built up an image of confident fortitude that was a sort of life-line in the bad

days of war news. We gathered in small groups, all else being forgotten, to discuss this dread event, and what was now to happen. Someone thought that the Vice President was some miserable little ex-haberdasher from Missouri, a machine politician, whom "nobody" had ever heard of, and so worry and uncertainty about the future deepened. How utterly wrong we were, although it was a year or more before we began to find out! It was Roosevelt who had been the weak man, in his final decline in health, and President Truman gradually showed himself in his true colours, one of the strongest and greatest Presidents that the United States has ever had.

This may sound like a diversion, couched in unusual language for me - but what I say reflects the general feeling at the time, and this event had a profound effect upon the early work of the Associate Committee. After a day or two the shock of the news began to wear off, and everyone buckled down to wartime work again with a will, the more urgent since nobody knew what would not happen; our confidence in President Truman was a long way in the future. And so, when I was asked to go to Ottawa again, to get started at the tracked vehicles study, it was a welcome call to action. I went upon the night train and was soon at the Sussex Drive building of N.R.C. to preside, at Dr. Mackenzie's insistence, as Chairman of the new Committee. In addition to Dr. Mackenzie himself, who sat through the whole meeting, (and me) there were present Colonel John Tuzo Wilson, then Director of Operational Research for the Canadian Army, Colonel G. M. Letson, the Director of Engineer Development for the Army, Professor R. E. Jamieson of McGill University, on loan to the Departments of Munitions and Supply as Director of Engineering Services, and from the National Research Council, Dr. D. C. Rose of the Division of

Physics, George Klein from Mechanical Engineering (because of his work on snow in connection with aircraft skiis) and F. I. Peckover of the same Division to serve as Secretary. The meeting lasted two hours; a little later, I joined Dr. Mackenzie for lunch at the Rideau Club and heard in more detail what he had told the Committee, being given my "marching orders" in his own inimitable low-key style, with promise of his full support.

In summary, Dr. Mackenzie explained to us that, when the Allied Forces went into France with the best assembly of military vehicles that the world had probably ever seen, their only real difficulty was that many of these splendid engines of war bogged down in the "mud" of northern France. This was never admitted or even mentioned in wartime news-reports, but he told us that it was so serious a problem that at the very top policy level (to which he was always Canada's senior adviser) it had been agreed that it was a problem that must be solved, since nobody knew when the war would end, or what new turns it might take, Hitler now acting like a mad-men. The British were to concentrate on operational research, to find out just what did go wrong; the United States, in the tradition of the automobile industry (in which research means building a full scale prototype and trying it out), were already embarked upon full scale testing, having taken over two agricultural experimental stations which had long test bins for trying out machinery. Canada had no such facilities as these and so it had apparently been agreed that we should look at the theory of operation of tracked vehicles, to see if this would suggest desirable improvements - in other words, as Dr. Mackenzie put it, we were to use our brains, as Canada had already done in other wartime fields. I hesitate to use that word

"brains" in this most personal narrative so let me hasten to add that my job was to find the brains, mobilise them and guide them in the work to be done. The Army would try to get the best possible personnel; Dr. Mackenzie had already arranged for an initial budget of $15,000; we would have the entry to all U. S. and British work, and were urged to maintain liaison with them, especially the U.S.; and if, as our work developed, I wanted for anything, I was just to ask for it.

Finally, Dr. Mackenzie explained that the steering committee had to have a name, and a name which would be a complete camouflage since at first the work was classified (I think) as Top Secret. He therefore suggested - and this was entirely his own idea - that we should call ourselves the Associate Committee on Soil and Snow Mechanics The word snow was there since there was already quite serious talk of a winter invasion of Norway (how strange that idea now sounds!) in which Canada would have a leading role to play. Over lunch, "C.J." explained to me the operation of Associate Committees, and showed me by examples how successful they had been. He had been following, despite all the claims of war upon him, developments in Soil Mechanics and had been impressed by the proceedings of the Winnipeg EIC meeting, and so he was certain that, when peace came, Canada must have a medium for supporting the develoment of Soil Mechanics in peacetime use ... and also snow mechanics/something nobody had then even heard of but, possibly because of his many years at Dean of Engineering at the University of Saskatchewan, he knew that snow must be studied in exactly the same way as soil. This, then, was the beginning and as will be seen, the real founder of "organised soil mechanics in Canada" (if it may be so expressed) was Dr. C. J. Mackenzie. Happily he was able to follow, always with close interest, the steady

development of Geotechnique in Canada. He gave me the inestimable gift of his friendship, more particularly after my retirement in 1969, and so in our well-nigh weekly talks, I told him of significant advances in Canadian work, always reporting fully on the annual conferences. He even came, when in his mid-eighties, to the lamentably disastrous twenty-fifth anniversary dinner, staying right to the end of that awful evening. He is still alive as I write this, but now not well, although we had our usual talks until mid-1982.

Despite all the challenge ahead, I had to go back to Toronto, to get ready for the special "extension" course already arranged for and booked solid; as already noted this went well, but as soon as it was over, and after conducting business in the meantime generally by telephone, I was back in Ottawa again, soon becoming a "commuter". I had managed to spend four days up there at the very end of April, having useful discussions with some of those who were going to help, notably Colonel Wilson, visiting the Vehicle Proving Grounds at Orleans, and the Soil Service of the Dept. of Agriculture since even then I could see the value of a link between Soil Mechanics and their work. This gave me my ~~first~~ initial meeting with Alf. Leahey who was at first surprised by this first approach from anyone outside agricultural soils work, but who welcomed the overture and became a tower of strength in mutual developments.

"V/E" Day" was on Tuesday 8 May. Reading this today, one might think that all need for the new committee automatically disappeared but this was not the case at all. There was still the fighting in the Far East ("V/J Day" coming on 2 September). Even after that, the loss of President Roosevelt still being an influence, there was such uncertainty and distrust of the Soviet Union that warlike

activity continued generally through 1946. Armies were gradually demobilised and there was much conversion of wartime facilities to peacetime use but the continuance of what may best be called the "armed truce" is, perhaps, best typified by the fact the the United States conducted its first atomic bomb tests (in the Pacific) in June 1946. The work of the Associate Committee therefore continued unabated, before it began to be transformed into the Associate Committee we know today, the concept of the "spaced link track", based on our research work had been developed, so the original job was done. How best to summarise those busy days?

I think it will be desirable for me to concentrate first on the Committee as such; by listing its meetings, the tempo of its work will be well reflected. The initial meetings of the Associate Committee were, therefore:

Ist.	20 April 1945	
2nd.	15 June	"
3rd.	24 August	"
4th.	29 September	"
5th.	7 December	" (in Toronto) (five in seven months)
6th.	9 February 1946	(in Montreal)
7th.	16 March	"
8th.	30 May	"
9th.	5 September	" (four in twelve months)
10th.	29 March 1947	
11th.	16 September	"
12th.	11 December	" (three in twelve Months)

.... and thereafter what became a normal pattern of two meetings (generally) each year.

Dr. Mackenzie only did not attend a single meeting after the first but he kept in close touch with the Committee and its work through my reports to him, and the written records he had time to examine. Don Rose dropped out after that first meeting, having done the job that the President (whose special assistant he was) had asked him to do. But the other participants (Wilson, Letson, Jamieson, Kline and Peckover) were most faithful attendants, only very rarely missing a meeting. They were a fine team to work with.* Colonel Wilson stayed on the Committee even after he gave up his military position to become a member of the teaching staff of the University of Toronto (as will shortly be related). Colonel Letson stayed active until he, too, left the Army and returned to his home in Vancouver, after the seventh meeting. Bert Jamieson went back to McGill at about the same time, his place from his Department being taken by N. C. Millman (on loan from General Motors, Oshawa) who continued the liaison as long as the Department of Munitions and Supply existed. Bert Jamieson, however, remained on the Committee, so interested was he in its work and future potential, retiring only in June 1952. Peckover passed the Secretaryship on to Don Nazzer, also of the Division of Mechanical Engineering, N.R.C., after the eighth meeting, while he took a year's study leave for a graduate course in Soil Mechanics at Harvard University; he rejoined the Committee as Secretary in . The first Army officer assigned to the Committee was a Captain "Jake" Kastner but for reasons which I do not now recall, he did not stay long, being replaced by Captain M. G. Becker of the Royal Canadian Engineers; he came, first, to the fourth meeting. Indicative of the continuing "pressure" on the Committee was the (welcome) presence at the fifth meeting (December 1945) of the Master General of the Ordnance of the Canadian Army, General McNaughton, and his Deputy, Brigadier Morrison, who came to be briefed personally on what we were up to!

(See p 76A)

* Please note — Sirius II

It may be worth recording (and I think FLP will be able to confirm this) that the early meetings of the Committee were a splendid example of the well-known fact that a well-chosen small committee can achieve very much more than would be achieved by the summation of individual contributions of its members, acting separately, no matter how good they may be. Once we started to discussd the track problem, Tuzo Wilson's brilliant mind would be in full actiom, ideas coming out just like sparks off an emery wheel; Bert Jamieson would sit there, smoking his pipe but saying nothing until he sensed an idea had merit when the pipe would be laid down and he would say quietly "I think we should discuss that"; Colone Letson, the perfect military man, always precise, quiet in speech, keeping our feet on the ground and never letting us forget that it was an urgent military problem that we were discussing; George Kline, with his quite phenomenal one-track mind, listening carefully, making the odd comment, but the leader as soon as we got on to snow. Peck and I? We were just the Secretary and Chairman.

Colonel Wilson ...

This meeting was held in Toronto, at the University, so that the Committee and the visitors could see the research work being done, in my laboratory in the Electrical Building, on the track problem; photographs of one of the experiments accompanied the Minutes of the meeting. To spread the load a bit, a Subcommittee on Track studies was set up but did not become too active even though interest in track studies continued into 1949. Major Becker then got an appointment at the Stevens Institute of Technology (New Jersey) and so faded from the scene. (For any who wish a more detailed account than this, there is a useful history of the early days of the Committee as an Appendix to the Minutes of the 18th. meeting, held on 21 April 1951).

The transition from the wartime committee to the Associate Committee as it is today was not a sudden change; it was rather, and to general agreement, a gradual shift in emphasis. Dr. Mackenzie made quite clear that this was what he wished. As early as the 4th. meeting, I find that I mentioned the idea of sub-committees to be responsible for each branch of Geotechnique when the Committee was able to turn its attenton to peacetime tasks. At its 7th. meeting (March 1946) the Committee approved the preparation of a report on Soil Studies in Canada, required for a forthcoming Commonwealth Scientific Conference in London, but directed that this should be submitted through the National Research Council and not by the Department of Agricuture, as had been suggested, an interesting indication of the ignorance, even at that time, of engineering soil studies.

The first significant change was at the 5th. meeting (in December 1945) when Dr. Norman McLeod joined the Committee as the

first representative (so to speak, but not officially) of civilian Soil Mechanics. He was then on leave from Imperial Oil Ltd., to the Department of Transport, engaged on his notable work on airport runway design. He contributed much to the Committe, ebeing eventually succeeded by John Walter of the Ontario Department of Highways, another pioneer worker. Dr. N. W. Radforth first came to a meeting of the Committee (the 10th.) in March 1947, as a guest, although the Committee had been supporting his muskeg research financially. At this same meeting, the Committee approved of the holding of the first Soil Mechanics conference, to which later reference will be made. Dr. P. O. Ripley came to the 13th. meeting (in March 1949), the beginning of the long-continuing and valued link between the Committee and pedological soil studies in Canada. Minutes of the 18th. meeting (27 April 1951) were the first to be issued with no classification attached to them, clear indication that the Associate Committee had assumed its peacetime role fully. This role had already so increased the workload of the Committee and its Secretary that Miss Margaret Gerrard was engaged in 1948 by the Division of Building Research to serve as Assistant Secretary to Mr. Peckover, But the work of the Division was expanding rapidly and so, at the 21st. meeting (in March 1953), Mr. W. J. Eden took office as Secretary giving devoted service until he in turn retired in 1967; he now serves the Committee as its Technical Advisor. Mr. Peckover left for his work on the St. Lawrence Seaway. Miss Gerard left her position in order to take charge of the Publication programme of DBR/NRC, to what good effect is now widely known. She was succeeded by Miss Gloria Zuane (1952-57) as Assistant Secretary; she then took on other duties in the Division and is now a member of the staff of the Canada Council. Mrs. Audrey Roper followed Miss Zuane (1957-59) but after a short period she was appointed as the Director's Secretary, a position she occupied under successive Directors until her own retirement in June 1982, a fact which can be left to speak for itself. (The Director of DBR/NRC had to persuade the Chairman of ACSSM to let Mrs. Roper leave.) Miss Joy Butler then started her long service with the Committee (1959 to date) first as Assistant Secretary with Mr. Eden and then, from 1967, as Secretary of the Committee, having been Mrs. Joy Curran since 1971.

I wish to record my special appreciation of what these early (and, in the case of WJE and JC, continuing) workers have contributed to the Committee since it has been upon the basis of their work that the Committee was able to advance as it did. When my own three-year terms of office came up for renewal, I "made noises" in appropriate quarters about being relieved of this pleasant but demanding task. There was always evidenced such strong feeling (writing and otherwise) that, since I was also the Director of DBR, this was a link for the Committee through its Chairman that was too valuable to be lost. And so my protests were "shot down in flames" until 1967 when I held the ace of spades, if I may mix my metaphors, and was able to pass on the chairmanship, with pleasure and confidence, to Carl Crawford.

Here I can stop and leave any more details of the Committee that may be needed for others to relate. I should, however, add brief notes about each of the civilian branches of work to which the Committee turned, and their initiation, as well as about the track studies.

Track Studies:

This should have been one of the highlights of the "Committee story" but, through a most strange development, it did not turn out that way. Memory being the wonderful phenomenon that it is, I have long since forced myself to forget much of the unpleasant part of this story, while up to this point memories have been almost always crystal clear. Nothing is now to be gained by going into the mattter in any detail and so I will give the most concise summary possible of what was one of the happiest research experiences I have had, spoiled only in its closing phase.

The Canadian Army managed to find, as successor to Capt. Kästner, a Captain M. G. Beckker to work with me on the track problem. Bekker was a Polish military engineer who had had a remarkable escape from Poland at the start of the war, finishing up in Ottawa. He had worked, in Poland, on military vehicle design and was very bright; so he was a "natural" for this job. We hit it off immediately and became close friends, our families also. He started with desk-top experiments in a glass-sided sand box to see just what did happen under loaded grousers. This proved so rewarding that the Committee authorised us to build a proper lab. set-up in the Soil Laboratory at U. of T., properly instrumented; we were given, for a time, the servces of Lieuts. Hamblin and Belford (two former civil students of mine at U. of T.). While experimental work continued, Bekker and I worked on the theoretical explanation. Looking back, it was not too difficult a problem; nobody had thought before us (apparently) of investigating the failure of soil under the combined horizontal and vertical loading upon a grouser. Theory agreed with experiment and so ultimately we were able to predict what the resistance to a grouser would be. When two or more grousers were used (as in a real track) it was easy to see that there was a critical spacing between grousers which should be observed if the action under one grouser was not to interfere with the resistance of the adjacent one. So arose the concept of the "spaced-link track". All experiments were initially in ~~soil~~ sand but eventually Bob Peterson (of P.F.R.A.) was asked to ship to Toronto a good block sample of really sticky gumbo clay. Here memoray fails me; I have no recollection of experiments in clay, but some may have been done. Reports were made regularly to the Committee; they acted as a most helpful advisory group.

Results were recorded in the following Technical Memoranda issued by the Associate Committee, of which copies were distributed, without delay, to British and U. S. authorities - so the work was known in appropriate quarters:-

T.M. #1	Proposed Field Soil Testing Device - August 1945
T.M. #2	Ground Failure Under the Action of a Track Grouser - September 1945
T.M. #3	The Interrelation of Soil Mechanics and the Design and Operation of Vehicles (CAORG Report No. 43) - November 1945
T.M. #4	Soil Survey of Vehicle Proving Establishment, Ottawa - October 1945
T.M. #5	Method of Measuring the Significant Characteristics of a Snow-Cover (N.R.C. Report No. MM-192) - November 1946
T.M. #6	Fundamentals of Soil Action Under Vehicles (Part One) by Robert F. Legget and M.G. Bekker - November 1946
T.M. #7	Preliminary Notes on "Muskeg" from Churchill by Norman W. Radforth - March 1947
T.M. #8	Fundamentals of Soil Action Under Vehicles (Part Two) by M.G. Bekker, Directorate of Vehicle Development, Department of National Defence - June 1947

T.M #6 was the vital document. Bekker and I wished that we could have published the contents of T.M. #1: it might have been useful.

All these were naturally classified before publication. Despite all efforts, long after the imperative of war made the classification understandable, the Committee was unable to get them de-classified, the most extreme example of bureaucratic inertia I have ever experienced. Finally, (14 October 1948) the Committee got fed up and at its 14th. meeting they instructed me to prepare, jointly with Bekker, a "civilian paper" on the work, for presentation to the Society of Automotive Engineers for their

next annual meeting.

Bekker had been spending a lot of time in the United States, first (I think) with General Motors, then at the Aberdeen Proving Ground. So, in concert with him by correspondence, and to his complete agreement, I prepared the joint paper (really a civilian version of T.M.6) and submitted this to SAE with a copy to Bekker. After he got his copy, he sent a telegram to NRC (whether to Dr. Mackenzie or me, I don't remember; I think to C.J.M.) saying that he would take legal action against the Council if the paper was presented (and/or published) with my name on since his professional integrity was affected etc. etc. I had not previously encountered the "Polish persecution complex"; all concerned agreed that this was an extreme example. I was (naturelly) so annoyed that I sent a telegram to SAE withdrawing the paper entirely. It was a joint contribution from NRC and DND and so I should not have done this without consulting DND. And so I was given a polite official reprimand, but by a gentleman with such a delightful twinkle in his eye (and voice!) that I knew that I had taken the action that all concerned (except Bekker) approved of fully.

I do not think that I have had any communication with Bekker since that time. He was soon permanently in the U.S.A. but now he got out of his obligation to the Canadian Army, I do not know. He took the view that all further work was mechanical engineering, with soil mechanics aspects of the problem solved. He went on to gain a reputation in this field, through his books. I know of these but have never looked at them; friends have told me that there is no acknowledgment of the NRC contribution. I was so busy with the development of DBR that I never bothered to follow up the matter; possibly I should have done so, but the days were too short! And

so the "Track Studies" have remained yet another minor Canadian research contribution to the wartime effort of Canada, unrecorded and so unknown.

Snow Studies

These were recognised from the very start of the work of the Committee, if only because of the notable work done by George Klein (DLE/NRC) on snow in relation toe aircraft skiis. He designed the "Snow Kit" which the Committee sponsored, and of which quite a number were distributed for studying the quality of snow in a quantative way. Apart from Klein's work, we were really in the dark out somehow I heard that there was work going on in Switzerland. Through contacts in the Canadian Met. Service, I heard that the head of the Swiss Meteorological service was to be in Canada in late 1945. I went to Montreal to meet him and found Dr. Lugeon a charming man, interested and anxious to help. He told me of the laboratory at the top of the Weissfluhjoch (mainly meteorological) and of the Swiss Institute for Snow and Avalanche Research at Davos, urging me to visit both. Such was the urgency then attached to snow studies that the Committee told me to arrange the earliest possible visit, with Bekker, adding that I should go in uniform since I would be one of the first Canadian visitors to post-war Switzerland. This was done, and Bekker and I spent a memorable visit in April 1946, being received by the Chief of the Swiss Defence staff at Davos, where we were shown all the Swiss Army's snow-experiments and at the Institute their laboratory work, meeting their Chief Scientist, Dr. Marcel de Quervain. There is a summary account as an Appendix to the Minutes of the 9th. meeting of the Committee (5 Sept. 1946).

Meantime, the Canadian Army, under Col. Wilson's inspired leadership, had organised exercise Muskox for the early spring of 1946.

Leader in the field was Lt. Col. P. D. (Pat) Baird, known to the Committee. Colonel Wilson kept the Committee informed of progress, even to the extent of having one of the progress telegrams delivered to him during the 7th. meeting on 16 March 1946! A later military exercise in the North was Flight Coriberg in which a North Star, four-engined plane flew over Arctic Canada for the first time. This was a well-planned exercise, the only landing fields being at Churchill, Fort St. John and Goose Bay, but despite this the whole of the land area of the North was safely traversed, and most useful observations made on snow coverage and on ice conditions in Hudson Bay (28).

I had come back from the Swiss trip, during which I also met in London Gerald Seligman, founder of what is now the International Glaciological Society and author of the first real book on Snow (Snow Structure and Ski Fields), quite inspired as to what Canada should be doing in this field. But there were no laboratory facilities to speak of, and so active research work had to await the completion of the Building Research Centre in Ottawa. Colonel Wilson had left the Army in 1947 to take up a full Professorship (of Geophysics) at the University of Toronto. Almost at the same time another Associate Committee was organised, under J.T.W's chairmanship - on Geophysics and Geodesy. J. T. W. and I were therefore able to team up, being by this time close friends, with the result that we agreed to establish a sub-committe on Snow and Ice under the joint sponsorshp of the two Associate Committees. There is an amusing appendix to the Minutes of the 11th. meeting of ACSSR, showing by means of a complex chart all the "snow snakes" bodies of which we knew, and the relation of the joint sub-committee to them. I have to smile when I looked at this again - a perfect example of how not to do things, as JTW and

(28) P&... 84a

(28) R. F. Legget (1950), "Flight Cariberg", The Beaver, Outfit (vol.) 281, September 1950, pp. 30-34.

Soon found out and
Ackowledged (jointly!) at the 14th. meeting of ACSSM on 14 October 1948; thereafter, snow research was under the aegis of "our" Associate Committee. One thing that was done, however, by the two Associate Committees was the holding of a two-day conference (in Ottawa) on Snow and Ice Research in Canada (on 17-18 Sept, 1947) one of the most interesting and useful gatherings that I can now recall from those interesting days. It was enlivened and assisted by the presence of Sir Charles Wright, one of the few surviving members of the Scott Antarctic Expedition of 1910, and joint author of the famous book on Glaciology (Wright and Priestley). Sir Charles, a resident of British Columbia in his final years, was active in research until well into his eighties. He was a regular visitor to DBR and followed with lively interest the development of the Division's Snow and Ice Research work when, eventually and initially under D. C. Pearce, but generally under L. W. Gold, it did get started.

Muskeg Studies

Muskeg was also recognised early in the history of the Associate Committee as a special responsibility of Canadian geotechnical research. None of us knew very much about it so the first action taken was the carrying out of a small military exercise by DND, under the personal command of Colonel Letson, around north western Quebec and North eastern Ontario, to see what muskeg really was. I can recall so well Colonel Letson reporting, rather sadly, that everybody with whom they spoke in the northern parts of the two provinces had a separate and quite individual explanation of muskeg! Somehow that triggered my memory and I recalled Dr. Radforth and his interest in "palaeovegetography" (Radforth's word). He came as a guest to the next Committee meeting, and so started on his notable

work in muskeg research, the record of which others will be able to prepare. I hope that some of the "Redforth stories" will be used to enliven the record; they are legion and delightful, especially the one about his journey to the Falkland Islands, today so topical. I must include one here since NWR and I are the only ones who know it.

The first research grant made by the Associate Committee was to Dr. Redforth, to help him get started with his work. This financial suport was continued year by year, supplemented by further grants from the Defence Research Board when it was formed (at the request of N.R.C.) to take over from the Council all its continuing defence research responsibilities. The Committee got very brief reports of progress but finally, and naturally, asked about publications. With the natural reaction of the "pure scientist" (as he then was), N.W.R. explained that there were some things he just had to investigate before he could venture into publication. This continued; the patience of the Committee was finally exhausted. As Chairman, I was deputed to tell N.W.R. that he would get no more funding until the Committee and D.R.B. had his first published paper. I thought that the best way of doing this was in an informal atmosphere and so, one evening after dinner at my home, Bill and I had a nice quiet talk. I told him, in effect, "Not one red cent more until his first paper was on my desk". I wish that I had had a tape recorder! A valued friendship was in danger of shipwreck - but finally Bill saw that I was serious and, very reluctantly he agreed to put work aside and prepare his first muskeg paper. This was for submission to the Engineering Institute of Canada for their annual meeting in Vancouver (I think). Being N.W.R. he lived up to his word; the paper one day came to my desk and was submitted; I reported this to the Committee; funding was renewed; and that was

(as he himself admits) the turning point in his career. Within a year, he had a major contribution in NATURE and his work was soon known internationally. It was a matter of special pleasure to me when Radforth's work was supplemented by the work of Ivan MacFarlane, in DBR/NRC, on the more practical/engineering end. He soon had some valuable publications, which I still see referenced, before he went on to other duties.

Permafrost Studies

Problems with permafrost (the name just then coming into use to designate perennially frozen ground) were mentioned early in the Committee's deliberations but it was not until the eighth meeting (30 May 1946) that a formal report on the matter was made, and then by Colonel Wilson. At the tenth meeting (29 March 1947) Pat Baird mentioned a study group that had been set up within DND to look into these problems. Then came the establishment of DRB and DBR/NRC and my discussions with Dr. Solandt (Chairman DRB) when we agreed that, since permafrost would be of greatest significance in civilian developments in the North, DBR/NRC should be responsible. Later a subcommittee on Permafrost was established under the initial chairmanship of Captain (later Major) Scott Lynn R.C.E., a happy linking of the two interests. The valuable work of this sub-group of the Associate Committee formed a helpful parallel with the actual field and laboratory research work of the Division, to which it served as a useful advisory group .. as did the corresponding subcommittees on Snow and Ice, and Muskeg, as I should have made clear. The national Conferences which each of the subcommittees organised were excellent examples of the effective work of Associate Committees in general.

Soil Mechanics Studies

What, in this context, I can call "Civilian Soil Mechanics" were implicit even in the main wartime task of track studies. We applied what we could find from then existing knowledge in the field to the grouser problem. In the visits which Bekker and I (and, I think, Peckover on at least one occasion) paid to U. S. installations and offices, concerned with track studies, we picked up miscellaneous information about current Soil Mechanics developments. All of this helped to demonstrate the need for a Canadian forum on the subject and the first move in this direction was the membership of Norman McLeod on the Committee. Prior to this, however, a Mobile Soil Mechanics laboratory had been developed at the Committee's request, by the Army and to my direction, for use in a soil survey of the Vehicle Proving Grounds at Orleans, east of Ottawa. It was crude but must have been one of the first of such mobile labs to be used. When the Survey was done, the Lab. was left at the Proving Ground and I do not know what its ultimate disposition was - this because there was no civilian agency to which it could be passed on.

At the tenth meeting of the Committee (29 March 1947), it was reported that L. F. Cooling of the British Building Research Station, accompanied by Dr. G. G. Meyerhof (also then of BRS) were to pay a visit to North America. The Committee jumped at the oportunity that this visit would provide and authorised the holding of a two-day meeting in Ottawa, with the visitors, to which all known workers in Soil Mechanics in Canada were to be invited. The Conference was duly held, on 28, 29 April 1947 and it proved to be the first of the now regular Canadian Soil Mechanics Conferences. We did not realise at the time what a pioneering venture this was to

prove to be since, apparently, it was the first such national Soil Mechanics gathering anywhere. It has been a special pleasure to follow the development of these annual meetings, all but one of which I was able to attend. Due, I think, to absence in Europe on building research matters, I could not attend the second Conference held in Lethbridge, a location chosen (I think) so that a field trip could conveniently be arranged to some P. F. R. A. projects. Others will be able to take up the story from there on; we have to it that a book record of each Conference was published.

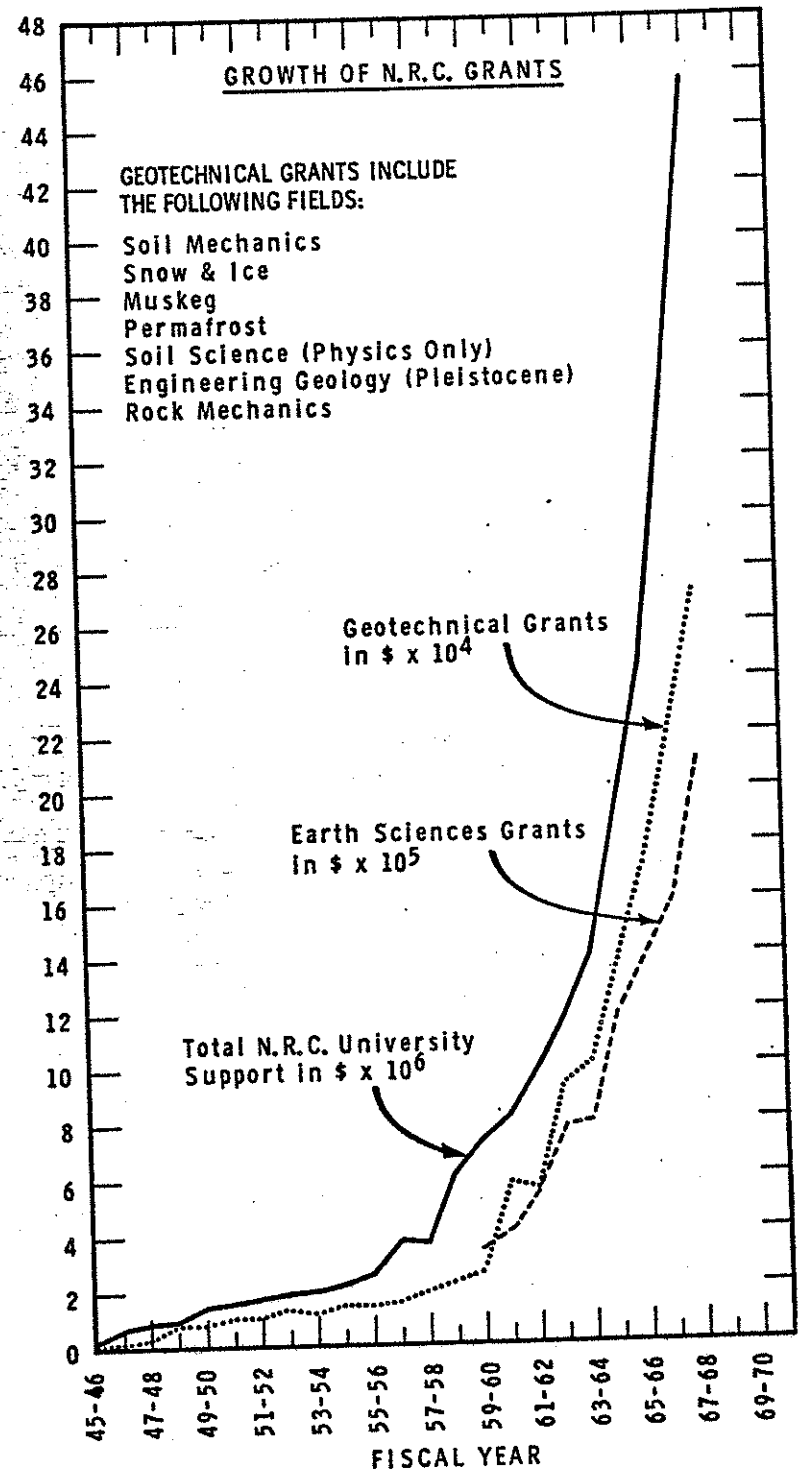
Associate Committee; Operations

As I have reviewed the records of the Associate Committee, I have seen that there are a few notes on "Operations", supplementary to the Minutes, that may be useful. The first relates to the special efforts made, from the start of the peacetime activity of the Committe, to make the Committee itself truly inter-disciplinary. Mention has already been made of the ready cooperation of soil scientists from the agricultural field. Membership was so arranged that there was always one pedologist on the Committee; I recall, appreciatively, the help of Drs. P. C. Ripley and Alf. Leahey. Correspondingly, there was always at least one geologist on the Committe, this with the hearty support of Dr. J. M. Harrison when Director of the Geological Survey of Canada, and his immediate successors. This was, to me, always one of the most satisfactory aspects of the Committee's work. To us, it seemed a perfectly natural and sensible way to operate but we soon found out that other countries looked at this Canadian practice with some envy.

Younger readers of this note will be surprised to know that the first Research Grants in Canada for geotechnical research were made by the Associate Committee. The first application came at the fifth meeting (7 December 1945) from a certain Dr. R. M. Hardy, requesting support for a study of foundations in Edmonton. Sympathetic as was the Committee, there was then available no money for such purposes, so the request was passed to Mr. J. Lorne Grey, then executive assistant to Dr. Mackenzie. By the 13th. meeting (March 1948) there was a little money available to support Dr. Hardy in attending the second International Soil Mechanics Conference, and Pat Baird a Snow and Ice meeting in Oslo. This resulted from the first research grant, made at the 10th. meeting (March 1947) to Dr. Radforth, for his muskeg research. After the establishment of DBR/NRC, my new colleagues and I, in our journeys throughout Canada, tried to encourage a start at geotechnical research at universities, seemingly with little success. Money for research grants was always available, amounting to $20,000 out of a Committee budget of $30,000 by 1959. In that year, applications exceeded the money available for the first time, and so I had to approach Dr. Steacie (then President of NRC) about getting some more money. That precipitated one of my interesting "confrontations" with my revered Chief, conducted in his own inimitable way, but the upshot was that thereafter no Associate Committee was ever allowed to make grants, all being handled by the Council's Grants Committees. Up to that time, these had been restricted to the fields of Chemistry, Physics and Biology (possibly a few in Medicine, but I am not sure). The real "break-through" (a term I use but rarely; here is it correct) was the establishment, by order of Dr. Steacie, of an Earth Sciences Grants Committee, with Dr. D. C. Rose and I as joint convenors. The name was carefully chosen to include all of Geotechnique (as we now know it) and Geology. The Geological

Survey of Canada had been making very small grants for geological field research, also just starting in Canada. Dr. J. M. Harrison and I discussed the whole picture (we did many things together) and when we put the broader concept to Dr. Steacie, he supported it fully. And after that, "applied science" was finally given full recognition in the NRC Grants programme. The result is best shown in the chart which follows. All the early missionary work began to pay off. It could well be that the initiation of the research grants programme was one of the most important things that the Associate Committee did - but that is for others to judge.

It was an application for a research grant from Queen's University that was the start of the second of the three (only) jarring notes in this record. Again, memory mercifully refuses to recall all the details, even the year in which this application was received! Fortunately, therefore, I can not give any details or names. The application was a very strangely worded one for work in the field of Rock Mechanics, so strange that it was decided that I had better go to Queen's and see the applicant. This I did, having one of the very few distasteful interviews that I have had the misfortune to hold. I was told, in no uncertain terms, that rock mechanics had nothing to do with soil mechanics; that "Terzghi doesn't know what he is talking about" when discussing rock mechanics (and those exact words I do recall); and a lot more along the same lines. I got precisely nowhere. I was not too surprised, therefore, when the gentleman in question moved on to another University shortly after our talk ... but that was the start of the separatist Rock Mechanics Committee (under the auspices of whatever the "Department of Mines" was then called), a development that



This record of the start of Research Grants for Geotechnical work in Canada is taken from "Geotechnique and National Development" by R. F. L., contained in The Earth Sciences in Canada: A centennial Appraisal and Forecast, edited by E. R. W. Neals, Special Publication No. 11 of the Royal Society of Canada, 1968 pp. 186 -202, the volume being another useful source of information.

caused widespread regret but which had to be accepted. (The divorce of soil and rock mechanics is found in other countries also, stemming from the similar international situation - and always due to people). Overtures were made from time to time but, when the rock mechanics committee got into its stride, this meant dealing with another remarkable and gifted man who was a real "loner" and with whom dialogue was impossible. It has been, therefore, with the very greatest pleasure that I have followed recent developments which give promise of healing this breach, now that both the men I have had to mention have passed on.

The third disappointment arose in connection with the local Geotechnical Groups, the formation and development of which gave to the Associate Committee such special pleasure. Having had the pleasure of speaking with all the Groups (possibly with one exception), I know well how much they have done in promoting the proper appreciation of Geotechnique across the country; they are a significant part of the geotechnical scene. But I have had nothing to do with their development directly; others can tell their tale.

Just before the 1969 annual Soil Mechanics Conference at Kingston, we heard that in Montreal a very small group of engineering geologists were going to form a Branch of the Association of Engineering Geologists. This fine body started in the 1950s as the California Association of Engineering Geologists, as a result of legislation passed by Los Angeles County - I think - regarding engineering geological services. It soon dropped the "California" from its title and added "American". Being guided, for a time, by good men who had no knowledge of international affairs, they soon started calling themselves an international body, promoting the establishment of overseas Branches. There is one in South Africa and one in the United Kingdom, each filling a gap. It has,

however, been a firm policy for well ~~over fifty years~~ of all the main scientific and engineering societies of the USA - with one strange and unusual exception, (IEEE) - to welcome Canadian members but never to form branches or sections in Canada, in consideration of Canadian susceptibilities. ASCE and GSA are prime examples; Canadian members are included in the contiguous US regions; Canadians have been Presidents of each body. But AEG, being a young ~~one-country provincial body~~ -still "geared to the West"- knew nothing of this and so welcomed the overture from Montreal. ~~The officers had so little appreciation of the international facie society organization that, until the last year or so, they refused to have anything to do with the International Association of Engineering Geology, on the grounds that they were the international body for all engineering geologists, just by saying so!~~ ~~Things are gradually changing but~~, in loyalty (maybe misguided) to my fellow Canadians, I could never bring myself to tell my good friends the successive officers of AEG to "lay off" Montreal. I should, perhaps, add that since they honoured me by making me an Honorary Member, I ~~was~~ was in a slightly invidious position!

I feel it necessary to make this long diversion since this is apart of what I carefully explained to Hugh Grice, one of the prime movers of the Montreal AEG unit, outside the Holiday Inn in Kingston at the time of the 1969 Conference. This was, naturally, a civilised dialogue as compared with my "rock mechanics talk". Hugh knew that this was the first, and still the only, unilateral such movement in the entire history of Geotechnique in Canada ... but nothing that I said would move him. The unit was established, Marc Boyer being (I think) the moving spirit. It is often hailed as proving how "international" AEG really is. But it remains a very small group,

with no more than twenty members, so I have been told. The climax was the so-called joint meeting in Montreal in 1982, joint as a result of a diplomatic gesture (I believe) on the part of the C.G.S. But it was not a joint meeting at all; some AEG members came away from Montreal with no knowledge of C.G.S.! And the very idea of the two small societies having their annual dinners in adjecent rooms in the same hotel on the same evening is so Gilbertian a situation that I would not have believed it, had I not been in the embarrassing position of being expected to attend both dinners!

This whole lamentable business must be due to some failure of mine; I still hope that I can help in correcting it. One glimmer of hope is that Pierre Crepeau (AEG) did attend the business meeting of the Engineering Geology Division of CGS and was much impressed. I do so hope that the Montreal Group, aided perhaps by the Eng. Geology Division, can persude the AEG unit to "get together" with CGS. I am ready and anxious to do what I can to help, even so late in the day; I am already dropping hints to AEG officers, on the basis of Montreal, which makes it now a little easier for me to speak.

This was a failure, minor perhaps, but still something that I regret. I feel sure that there must have been other failures or omissions in the thirty five year history of "organised Geotechnique" in Canada for which I must accept some if not most responsibility. Let me mention two of which I am aware. Tunnels constitute the major Geotechnical projects in any country. Quite some years ago, Don Macdonald and I talked about this and decided that the first step to take in "doing something about it" would be to compile a Directory of Tunnels in Canada. We did, I think, make a start but I got side-

tracked at a very busy period in the Division's history. When we were ready to pick up the threads again, so to speak, we found that a Tunnelling Office had been established within the Department of Energy, Mines and Resources (I think the new name had then come into use), and so we let the matter drop. Readers may judge my surprise when I found that nobody knew anything about such an Inventory when I came to inquire during the course of writing an Introduction for the pending Tunnelling Manual. Accordingly, I am just delighted to know that the ~~Committee~~ Society now has a tunnelling Committee, working in concert with the newly formed Tunnelling Association of Canada, a useful and natural extension of geotechnical activity. Landslides, which have caused so much trouble in Canada, constitute another problem area to which we might well have given more attention. I have a vague idea that, in the very early days, the Committee did have a subcommittee on Landslides (WJE will know) but it could not have done very much since it did not continue. In so, again, I am delighted to know that the fourth International Landslide Symposium is to be held in Toronto in 1984. Possibly this will reactivate interest in the subject in Canada to which I wish the Associate Committee had been able, years ago, to address itself more effectively.

After these gloomy thoughts about "things left undone which I ought to have done", let me turn to two other things that we did do. The matter of publishing results of research arose at the very start of the Committee's work, I believe at its second meeting. We followed an established NRC practice of using mimeographed, well bound Technical Memoranda; the list of these committee publications is now an impressive one. They were satisfactory for recording the early work, and records of useful meetings, but something more, and

better, was needed. This became a regular matter of discussion at the semi-annual meetings of the Committee but the subject was a complex one and so no action resulted in the 1950s. Much interest in the subject was evident in the Toronto Group, (the late) Larry Soderman and Vic Lilligan (I think) being prime movers.

To explain the complexity of the situation, I must again diverge to record that the same problem was being faced in the Division of Building Research. Dr. Hutcheon (Asst. Director) and I had many talks on the matter. We were getting papers published on building rseaearch in reputable U.S. journals but, naturally, wished that there was a Canadian medium that we could use. The Council itself was well launched with its Canadian Journal of Research - but the six sections in which this was then published were all in the field of "pure" science (Physics, Chemistry, Biochemistry, Physiology and Pharmacology, Botany and Zoology). There was, in addition, a Canadian Journal of Technology which was a sort of catch-all for papers that did not fit into the other journals. We never did find out who had started it, or who had chosen the regrettable name of "Technology" (then, and possibly still, looked upon by "pure" scientists as a minor form of plumbing (and equally disdained by engineers)). We discussed the situation with Dr. Sheacie. He was sympathetic and finally agreed with me that we could "take over" the C.J.T. and convert it into a Canadian Journal of Applied Science. Once well established, it would be a natural development for specialist fields in applied science to "hive off" with their own journals, one field being (naturally) Geotechnique. This seemed to be the answer but, to our amazement, when we came to take the first steps, we found that C.J.T. had been given lock, stock and barrel to the Canadian Institute of Chemistry to develop as they

pleased, independent of the Council. This step was taken by Dr. Leo Marion, the Director of the Division of Chemistry and Editor-in-Chief of the Journals, apparently without the knowledge of Dr. Steacie. We were most annoyed, but there was nothing to be done but to accept the situation and start all over again.

I am not exactly sure of the detailed steps that led to the start of the Canadian Geotechnical Journal but I have a vivid recollection of Larry Soderman coming to a meeting of the Committee, with his usual "divine" impatience over the lack of action by the Committee, only to find that we were all thinking along the same lines. I had further talks with Dr. Steacie, just before his lamentable terminal illness (he died, to universal regret, in 1962) and we argued (as we always did) about the best way to proceed. He would not agree to start a new section of the Canadian Journals of Research (maybe the memory of the disposal of CJT was still too vivid) but he did agree with the Associate Committee starting a journal, to the same high standards, if we could finance it ourselves, and if I would guarantee to him that we had enough papers in view for two whole years before we launched the first issue! (What a man he was! I argued strongly against the latter provision, but he was right, so right, as other fledgling journals have discovered). This was reported to the Committee and then things moved. Here memory is a bit hazy but I feel sure that we were indebted to the Toronto Group for much, if not all of the effort which went into the start of our Journal. The first issue appeared in September 1963, graced with a Foreword by Dr. Terzaghi; volume 2 started with the first issue of 1965 and thereafter volumes coincided with years. Not until volume 6 did the editor's name appear - and this

(I think)
was in the first volume to be published directly by the Council, as a part of C.J.R., since our Journal had by then "won its spurs". Fred DeLory was the editor so named but I am almost sure that Vic Milligan served as the founding editor, assisted by Fred DeLory, Pierre LaRochelle, Carl Crawford and Don Bezett, these being the names inscribed in the specially bound copy of the first volume that was so kindly presented to me on 5 March 1965. Others can give the full story of the Journal but I must record my great pleasure at the splendid job done by successive editors in developing its present international reputation. In several countries other than Canada I have heard its praises sung, several leaders in Geotechnique telling me that it is now the only geotechnical journal they read, since it has so well kept a balance between theory and practice, other journals having become almost entirely theoretical. Long may it continue to do so

The annual conferences call for some further comment, even though I have already mentioned them briefly. The tenth such meeting seemed to us, at the time, a great milestone! And so, greatly daring, we invited Dr. Terzaghi to come and this he did, to our great pleasure. The dinner was held on 17 December 1956 in the Chateau Laurier and it proved to be a delightful affair, graced by the presence of the Honorable Robert Winters, then Minister of Public Works, Dr. Steacie as President of the Council, Dean Henri Gaudefroy then (I think) still at Ecole Polytechnique, and Dr. Terzaghi. Our guest spoke for an hour and three quarters, without a note, on geotechnical aspects of the Assouan High Dam, one of the finest technical addresses I have ever heard. It was a notable occasion in itself but notable also in that this was one of the first "exposures" Dr. Steacie had to applied science at its best and I am now convonced that it proved of

(Please start new paragraph,
after".... March 1965").

Once again, as was inevitable I suppose, we did not realise what a significant step we were taking. The Canadian Geotechnical Journal was, I think, the first journal to be published in Canada in the field of applied science. During the planning stages, I kept in close touch with Dr. J. M. Harrison, Director of the Geological Survey of Canada. Together, I think, we had discussions with Dr. Steacie who agreed that the "pure" Earth Sciences could have a new Journal as a part of the Council's CJR series. It was therefore agreed, between JMH and me, that the Canadian Journal of Earth Sciences and the Canadian Geotechnical Journal would be closely complementary, the one taking purely scientific papers, the other papers in all applied fields of all the Earth Sciences. This is clearly stated on the first page of the first issue of the Geotechnical Journal but I have found recently that it is quite unknown to those responsible for the Journals today. The link should be so stated on both title pages. (I propose to follow this up.) The Geotechnical Journal was first in print by a few months. It was the first NRC journal to use our second language, and in its title, too. It was the first NRC journal (since the Associate Committee was at first alone responsible) to include discussions of papers, something previously unknown. It also published Book Reviews. After some discussion, these "novel" features were continued when the Journal was taken into the CJR fold. Having made these really great advances, we had to "back off" from publishing Obituaries (such as for Bob Peterson) and, to my profound regret, this alienated Dr. Hugh Golder, but there was nothing I could

real assistance to him. When he was appointed President, following Dr. Mackenzie's departure (at Mr. Howe's behest) to be President of Atomic Energy of Canada Ltd., E.W.R.S. was a pure scientist of the pure scientists. I even heard him "glory" in the fact that he "never attended committee meetings". Within five years, this very great man was the acknowledged leader of all the scientists of Canada - as I was told by another leader in the field in a moving telephone call just after Steacie's death was announced. Despite his having told me that Building Research was merely glorified plumbing (in the provocative way in which he used to talk with me) and his initial disdain for "messing about with mud" (he used the expression too), it was not long before he became the strongest supporter that DBR/NRC could possibly have wished to have, and equally so of the Associate Committee and its work. I am confident that the tenth anniversary dinner helped.

Correspondingly, we were given full support when the time came (in or around 1962) that the annual conferences had become so successful and so well attended that it was clear they must be passed on to some other body. This was the start of the Canadian Geotechnical Society, another rewarding development to watch, the success of which is for all to see - with almost fifty percent of its members now attending its annual conference on occasion, something unknown to me in any other society. Again, I was an observer to all this but I must record that the two-year transition period for the annual conferences to get ready to "stand on their own feet", and the associated establishment of C.G.S., were entirely in the tradition of the Council's operations. There were some, who did not know the Council well, who thought at the time that NRC (and maybe DBR) was

just "washing its hands" of what had become a burden. Exactly the opposite was the truth; it was all done with full support from the Council. This was, I think, the first such development in a field of applied science, but both the Defence Research Board and the Medical Research Council, not to mention Atomic Energy of Canada Ltd., had all "hived off" NRC in the same way, to the Council's pleasure, and in keeping with its modus operandi of stimulating research in Canada. The one thing we could not have foreseen was the way in which the Engineering Institute of Canada would change ... but that is another story which has not effected the steady and fine progress of the Canadian Geotechnical Society, the culmination of the development from such small beginnings that has been so roughly sketched in these pages.

Associate Committees in General

Now that the Associate Committee is again "just a committee" (a lamentable expression but for which I can not think of a better alternative), a final word about Associate Committees in general may be of service. From time to time attacks are made upon this unique committee structure; I have even had to listen to a former member of our own Associate Committee make such an attack in public. All the committees are reviewed from time to time, as they should be, and their continued existence considered. Such sensitivity is all to the good and is one of the strengths that makes NRC Associate Committees, as a whole, quite the most remarkable and useful agency of their kind that I have ever encountered in any country, so much so that I earnestly hope that they long continue to provide the national service that they do.

In the course of my duties for the Council it was my good fortune to visit many countries overseas, as well as the United States quite regularly, and not only to make such visits but to come to know leaders of research in many lands. Again and again I have been asked how Canada managed to achieve so much in research with so small a population (relatively) occupying such a vast territory, questioners comparing their more compact spheres of action with ours. It was in this way that I came to see the special value of the Associate Committee arrangement to Canada, being led to explain its salient features in answer to probing questions. The first Associate Committees were established by NRC before the second world war and some did useful work. It was, however, during the years of the 1939-1945 war that they really came into their own, under the watchful eye, and with the full support of Rt. Hon. C. D. Howe, as Dr. Mackenzie told me on more than one occasion. The basic concept of getting together in the same room the best brains in the country in any one specific field, to discuss problems from the national point of view, is so simple that it was strange to have to explain it to others. By the appointment of members to committees only as individuals, and never as representatives, was one of the keys to success. Payment of travel expenses, to well defined and sensible guide lines, conquered our geographical limitations. Service on the committees voluntarily, and with no payment for time - despite the great value of the time given by every members -was another basic assurance of impartial and good judgement on the problems addressed. And regular rotation of membership gradually but steadily developed a nation-wide fraternity (and I chose that word) in each particular field. If ever an example is needed of all that is best in NRC Associate Committees, AOSSM will be well to the fore in any selection.

L ' E N V O I

It will have been very obvious to any reader who has managed to get this far that the simple request of Jack Clarke and Dave Townsend opened up for me a Pandora's box of memories - so much so that I now feel that, perhaps, I have included too much of a personal nature about the early years, even though it seemed to be relevant in expleining how fortunate I was to be associated with the start of Geotechnique in Canada. This was no "inspiration" on my part but just my good fortune to be in the right place at the right time and to have been privileged to be aided by wonderful friends, as I hope I have made crystal clear. And it is of friends that I think as I close off this memoir, most of them still active, some who have passed on, leaving rewarding and treasured memories. For all these friendships, I am truly grateful.

Despite a few difficulties which I have not hesitated to record, it has been a wonderful experience. Were there any highlights? There were indeed .....the first Soil Mechanics meeting in the Meritimes, at Fredericton in April 1954, attracting twice as many as we had expected, including some very practical construction men who kept our feet on the ground, with the St. John River in flood and, despite this, the Premier of the Province (Hugh John Fleming) coming to our luncheon and being really interested, so much so that he remembered the occasion in his final years when he had become a friend; ..... the wonderful team work and team spirit that gradually developed among the whole Canadian geotechnical fraternity (a word I use with meaning and with pleasure), demonstrated in full measure

when, as chairman of the Organising Committee for the SIXTH International Conference in Montreal (the record of which I leave others to relate), I had to leave for meetings in Europe just six weeks before the start of the great meeting; things probably went better with me away, but only Carl Crawford (probably) knew the depth of my concern but he knows also the imperative of my being at the C.I.B. meetings that took me away, now from his own experience; .....the evening on The Mountain in Montreal during the Conference when the Mayor opened up part of his wonderful wine cellar for our buffet supper et the Lookout on that perfect summer evening, the moon coming up just as (at the Mayor's behest) all the lights suddenly came on in every tall building below us and a hush fell upon the crowd, a hush that told us that all our worries were over ("How do you Canadians arrange evenings like this?" is a query I still treasure!):..... the dinner at the 1968 annual conference when I was asked to describe my experiences just a few week before when I had lived through the invasion of the lovely city of Prague by Soviet forces, at the start of the International Geological Congress; never in my life have I felt such a glow of friendship and mutual concern as seemed to fill that crowded, hot room on that memorable evening:..... and, naturally, the annual dinner in Kingston in 1969 when my friends marked my retirement as Director of DBR/NRC and my earlier retirement as Chairman of ACGR, by announcing to my bewilderment the establishment of the first Canadian geotechnical award to which, in their kindness, they attached my name.

My feelings about that recognition are still, today, far deeper than words can express and so all I shall allow myself to say in conclusion is that I hope I have done just a little to be worthy of that most gracious honour, that my appreciation of all that I have gained by being a member of the Canadian geotechnical team is at least indicated in part by what has gone into the writing of this memoir, and that the end is not yet; I still hope to be of some minor service.

Foundation Engineering

Cambridge, Mass., June 22-26, 1936

APPENDIX "A"

(Abstract of a report contributed by R. F. Legget, A.M.E.I.C.)

The first International Conference on Soil Mechanics and Foundation Engineering, held at Harvard University, Cambridge, Mass., U.S.A., from June 22nd to 26th, 1936, was attended by about two hundred and fifty engineers and research workers, representing almost twenty different countries The conference assembled for the first time the results of soil research work at present proceeding in many parts of the world, and correlated at least to some extent the studies of soils necessitated in many of the various branches of civil engineering work.

The President of the conference was Dr. Karl von Terzaghi, of the Technische Hochschule in Vienna, Austria, and presently a visiting professor at Harvard University, to whom is due very largely the progress which has already been made in the scientific study of soils. In his addresses, Dr. Terzaghi sketched the development of what has now generally become known as the science of Soil Mechanics, using the word in its true geological sense, as denoting all unconsolidated materials found as constituents of the earth's crust. It was recalled that about twenty-five years ago, in three different countries, the attention of engineers was forcibly directed to close study of sands and clays by reason of certain disastrous failures. In America, the Panama canal slides; in Sweden, the severe slides on the state railroad system which resulted in the appointment of the Royal Swedish Geotechnical Commission of Landslides; and in Germany, the construction of the Kiel canal led to such serious difficulties that careful studies were necessarily initiated. The incidence of the war impeded progress, but in the years immediately prior to 1920 it was finally demonstrated that geological observation of such phenomena was not enough, and physical soil tests, planned in a general manner, were soon begun, although with primitive apparatus. Thereafter, soil research work commenced in many laboratories and universities throughout the world.

Studies of soil problems had been made from the start of modern engineering, and due tribute was paid to the many records of observed data in connection with such engineering works which are to be found in older engineering publications, especially those of Great Britain, dating up to the year 1880. Subsequently, empirical rules, based on these observations, seemed to become generally accepted and records gradually became more fragmentary. This condition persisted until more scientific study of soils began, when these older rules were often found wanting. Although at the start of recent investigations it had been hoped to develop a tool as definite as (say) structural analysis, such hopes had been abandoned, but gradually a new mass of evidence was being assembled, and on this foundation it was being found possible to erect the framework of a useful science.

Chairmen of national committees had been appointed prior to the conference, in addition to four vice-presidents, one Italian, one English, and two American, and the resulting preliminary work led to nineteen countries being represented at the proceedings. American engineers naturally predominated in number, but both Mexico and Germany sent relatively large delegations, and six engineers were present from Canada. Two engineers travelled to the conference from the East Indies, one from Java, and the other from the Federated Malay States. A member of the staff of the Building Research Station of the British Government represented recent British research work.

The free interchange of ideas between engineers of so many nations, both in formal discussion and outside the conference rooms, was perhaps the most notable feature of the conference. Eight half-day technical sessions were held, each starting with either one or two illustrated lectures on engineering work in one of half a dozen countries.

The papers submitted to the conference number over one hundred and fifty. All were printed either in full or in abstract form and distributed to participating members prior to the opening session. A volume, to be issued later in the year, will contain a record of the proceedings and will present an invaluable collection of data of actual records from construction, and of theoretical analysis.

Contributions were classified under fifteen sections, to which reference is made in the following notes.

Section A. Reports from Soil Mechanics Laboratories.

Twenty five papers were presented, describing the equipment and research work of twenty soil mechanics laboratories in America, Europe, and Japan. Members of the conference were enabled, in the evening sessions, to visit the soil mechanics laboratories of Harvard University and of the Massachusetts Institute of Technology, seeing there several of the types of testing equipment described in the papers. Humid rooms to assist in the correct determination of moisture content of clays etc., are a standard feature of the larger laboratories. Grain size is usually determined by means of standard sieves and the hydrometer method developed by Casagrande. Compression, consolidation and permeability are mechanical properties the study of which is now almost standardized. Several different methods were described for the testing of the fourth fundamental mechanical property of soils and their shear strength.

Section B. Exploration of Soil Conditions and Sampling Operations.

The eight papers in this section gave many useful references to standard types of sampling equipment, the development of which has been mainly the work of engineers who have had to adapt standard drilling methods so that, with little trouble or expense, relatively "undisturbed" samples of soils can be obtained from any desired depth, below ground level. The basic idea of all the main types sampler is to drill first a fairly large diameter drill hole, case it necessary, and then insert a special sampling tool which is forced in the ground at the bottom of the hole, rotated to isolate the core, ar then withdrawn, the sample obtained being sealed, with either a pa of the metal casing left in place, or with paraffin wax, to preve change of moisture content, etc. The developments of this idea a generally designed to reduce the necessary disturbance to a minimu Examples were given of the evident distortion, within the samplii tool, of various sizes of samples, due to the friction between the met of the sampler and the soil. Examples were also given of the swelli of even small clay samples obtained from great depths. The use "undisturbed" samples has been generally confined to those of clay strata, cohesionless materials such as sands and gravels being usual tested by means of loose disturbed samples.

Section C. Regional Soil Studies for Engineering Purposes.

A group of six papers dealing with regional soil surveys demo strated the extent to which soil studies have been applied to prelimina civil engineering work. Of special interest was a description of a cor plete soil survey of an area of 900 acres near New York, to be us initially for the 1939 World's Fair, and ultimately as a public par but at present low lying marsh and swamp land. By means of extensi sampling and subsequent laboratory work, a complete knowledge subsurface conditions has been obtained from which predictio regarding probable foundation settlements, data for foundation desig and methods for filling procedure have been worked out.

Section D. Soil Properties.

This section was concerned with general considerations presente in thirteen widely different papers. Work at present being carried o at the Building Research Station of the British Government on t shearing strength of clays, following up Professor Jenkin's initi studies of granular materials, was described by L. F. Cooling ar D. B. Smith, the former amplifying the paper in discussion by der onstrating the utility of the shear strength of clays at zero norm load as a means of obtaining the constant in the basic shear equatio Research work carried out in France on the effect of the speed of loadi on soil properties was described in another paper. The importa influence of the chemical nature of clays on their physical and mecha ical properties was demonstrated in a paper by K. Endell and Hoffman (Germany). This fundamental aspect of the subject w reflected in a paper describing Russian research work on the influen of scale-like particles (such as mica) on soil properties.

In general discussion, little attention was devoted to the pro erties of granular materials (sands and gravels) as there appears to general agreement on at least their main characteristics. With rega to clays and cohesive soils, it was generally admitted that researc on the shear strength of such materials is still incomplete.

Section E. Stress Distribution in Soils.

This branch of study is complicated by the fact that althou theoretical analyses can be made of stress distribution in homogeneo materials, soils as encountered in practice are always far from uniforr The classical theory of Boussinesq therefore formed the basis of much the analytical presentation and in some of the eleven papers included this section, was in sharp contrast with records obtained from actu observations. Of special interest to practising engineers is a paper (E by G. M. Rapp and A. H. Baker) describing the measurement of s pressures on the lining of the new mid-town tunnel under the Hudsc river, at present under construction in New York, in which details the gauges used are given and a summary of the records so f obtained.

Section F. Settlement of Structures.

In this section, the explanation of and the predeterminatic of the settlement of structures attracted much attention. Fifte papers were presented, many of these being actual records of settlemen and one of the illustrated lectures was given by Dr. Hanna and M Tschebotareff, amplifying data they had given in papers regarding t settlement of structures in Cairo, Egypt. The poor subsoil cond tions there encountered were contrasted with the underground form tion of volcanic ash and fine sand found at Mexico City, which in t past has caused serious settlement of many large buildings ther Other papers gave data on settlements from Shanghai, Vienna, ar Texas, as well as from three American bridges. One of the latter w the new bridge over the Mississippi river at New Orleans, which h to be founded on unconsolidated strata over 2,000 feet thick. Su surface exploration was very carefully carried out and settleme calculations made in connection with all pier design; these were check as construction of the piers proceeded, and as a result certain valuab modifications were introduced into the completed bridge design. A interesting observation made was that during flood period on the rive with a rise of water level of 15 feet, a temporary settlement of one and half inches took place, which disappeared on resumption of norm water level.

In discussion it was stressed that the main purpose of settleme studies is to enable foundations to be so designed that settlemen can be predicted with a fair degree of accuracy. Small scale loadi tests may be potentially dangerous unless carefully correlated wi

A P P E N D I X " B "

The Associate Committee; an Outside View.

Extract from a paper by Dr. N. B. Hutcheon, when Director, Division of Building Research, National Research Council, on "Geotechnical Research in Canada", presented to the Seminar on Guidelines for Scientific Activites in Northern Canada 1972, held at Mont Gabriel, Quebec, and published in Science and the North, Information Canada, Ottawa 1973.

P. 232:

"A decision was made at the time the Division of Building research was created to continue the war-time studies of terrain problems under the direction of an associate committee of the National Research Council. This associate committee, now known as the Associate Committee on Geotechnical Research, recognized four main subject areas of interest, namely, soil mechanics, snow and ice mechanics, organic terrain, and permafrost, and served these interests for many years through four active subcommittees. The study of permafrost and the promotion of an understanding of it has been the special objective over the years of the Sub-Committee on Permafrost of the Associate Committee on Geotechnical Research.

In a report to that Sub- Committee made by Harwood and Brown in October, 1968, it was noted that there were only five full-time research workers on permafrost problems in the federal government. In addition, there were up to fifteen academics and students interested in these problems and perhaps five consultants having competence in terrain engineering. This is not much

national capability to support a multi-billion dollar construction program.

More recently another sub-committee, on Pipeline and Land Use Technology in Northern Terrain, has been established. The deliberations of this new sub-committee led to the organisation of a Canadian Northern Pipeline Research Conference, attended by more than five hundred delegates, in February 1972.

The success of these ventures has been remarkable, and constitutes an outstanding example of the way in which government, industry, university, and professional engineering research and associated interests can be encouraged and made highly inter-active with great mutual benefit. As a consequence, there is now a closely-knit, highly responsive group of geotechnical specialists in Canada who are well-informed and sensitive to developing needs. Links between people, agencies, and information sources at home and abroad are already highly developed and Canada is in a position to make the very best of what under other conditions might well be a disastrously inadequate response to the current needs for northern terrain engineering. It is highly significant that the Associate Committee on Geotechnical Research is a national interdisciplinary committee providing a balanced representation in its membership of industry, universities and government. It has been particularly effective in promoting the core of engineering expertise which has now become of vital importance. Although sponsored by the National Research Council, which is a government agency, it is essentially non-governmental, having no responsibilities in day-to-day operations of government."

APPENDIX "C"

A note on early workers in Canada:

Although I have never made a special search for records of early geotechnical work in Canada, my diggings into the history of civil engineering in this country have turned up some notable pioneers and some remarkable early work. This is "out of my province" but as a help to those who will be looking into this part of the History of Geotechnique, the following references may be useful:

Sir Sandford Fleming; two papers by F. L. Peckover and me will be found in the Journal, vol. 10 for 1973 recording quite the most remarkable early work known to me.

Samuel Fortier; an almost unknown pioneer who used soil cmmpaction before R. J. Proctor was born; see CCE Dec. 1970.

Noseph Hobson: another of the unknowns, builder of the Sernia CNR tunnel (test borings every 20 ft); see CCE Sept. 1979 and March'8

The building of the Murray Canal near the end of last century involved some remarkable test borings work: CCE Jan. 1980.

So also did the studies, by the Dept. of Public Works, for the Georgian Bay Ship Canal in the first decade of this century; see Sessional Paper No; 10 of the House of Commons, 1909.

Professor Barnes, of McGill University, was a somewhat unconventiona pioneer in Ice Research; I think he wrote a book on his work (1920?).

Prod. E. Brown of McGill and C. G. Clark had a paper in the Eng. Journelin the late 1920s or early 1930s on their experiments with ice in a cold storage warehouse.....

and there must be many others!