(note that some of the dates etc are wrong. The ones that I have found so far are:
Keith Smillie's PhD was 1952
William Morton Kahan (born 1933) MA 1955, PhD 1958
Katsumi Okashimo, PhD 1955 (note the name is spelled wrong in this document)

My Early Days in Toronto

A Memoir by B. A. Griffith
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I was born on November 16, 1908 in the city of Hamilton, Ontario. Of my early life in that city I can remember very little. I was only 3 years old when, for the betterment of my father's health, my parents sold their home in Hamilton and bought a small farm in the Niagara Peninsula, near the tiny village of Fruitland, some ten miles east of Hamilton.

That was a very poor farm: the land had not been cultivated for many years and was covered with weeds and thorn bushes. For buildings there was a small, unpainted, frame house without a basement and a low shed for livestock - no barn. For my parents life on this farm was, for several years, very like that of early pioneer days. For me that simple life was a blessing in strange disguise: throughout my school days, from late 1915 until June 1926, there was little to take my attention away from school work.

In June 1920 I passed the examinations for entrance into High School, still only 11 years old. My parents felt that I was too young for High School and I was given a long holiday from school. In September 1921 I started to High School in Grimsby. After two years at Grimsby I transferred to the Collegiates in Hamilton - one year (1923-24) at the Central Collegiate (H.C.I.) and two years at the newly completed Delta Collegiate (D.C.I.). In my final year at D.C.I. I applied for several Ontario Scholarships and wrote 15 Upper School papers including the Special Problems paper for Scholarship candidates in Mathematics. In those papers I did not do as well

as I had hoped but did manage to rank for 3 or 4 of the lesser Ontario Scholarships. I could only accept one Ontario Scholarship but this carried with it free tuition for four years at the University of Toronto (U of T). This award, together with the First Sir John Gibson Scholarship, which I had won in the examination for Junior Matriculation and the First Carter Scholarship for Wentworth County gave me sufficient funds for my first year at U of T.

In September 1926 I enrolled in Physics and Chemistry (P&C), an Honours Course at U of T, in which the curriculum for the first year was identical with that for the first year in Mathematics and Physics (M&P). There were only two students who, in 1926, enrolled in P&C - Tuzo Wilson and myself. Neither of us stayed in P&C - Tuzo transferred to Physics and Geology and I transferred to M&P. I enjoyed my four years as an undergraduate and on graduation in May 1930 was awarded a fellowship in mathematics that would cover tuition and expenses during my first year as a graduate student. In that year my duties involved giving a lecture course in elementary Calculus to First Year Medical Students, acting as part-time assistant to Professor J. L. Synge (Pronounced 'Sing') and completing four graduate courses for the MA degree.

Professor Synge was a distinguished graduate of Trinity College, Dublin and had spent at least two years (1924-1926) on staff at Toronto. In the spring of 1926 he had returned to Ireland as a Fellow of Trinity College. During the next four years he published several important papers and cooperated with A. W. Conway, F.R.S., in editing Hamilton's papers in Geometrical Optics (See:*The Mathematical Papers of Sir William Rowan Hamilton, Vol 1*, Cambridge, 1931). Now, in September 1930, he returned to Toronto to establish a Department of Applied Mathematics. For me the return of Professor Synge to Toronto meant that a very considerable part of my work was much like work at Trinity College, Dublin.

The four courses that I selected for my MA included a course in Hamilton's Method in Geometrical Optics given by Professor Synge and courses in the Theory of Numbers, Mathematical Statistics, and advanced Theory of Functions of a Complex Variable. In that year my work for Professor Synge must have been reasonably satisfactory for, at the year's end, he asked me if I would accept a fellowship in Applied Mathematics for the following year (1931-32). This I was pleased to do and for the next few years I was to work under the direction of Professor Synge as a student, as a PhD candidate and as a colleague.

That first year as a graduate student was not only the start of my career in Applied Mathematics, it was also to mark the beginning of my life with the girl who meant more to me than life itself. Jean MacLean was the most beautiful girl I have ever seen. We had met at the Hart House Masquerade on my 21st birthday - a good omen. Jean had come to the Masquerade with a classmate of mine - we exchanged partners for one dance and I learned that Jean had, for one year, attended Grimsby High School. Throughout that dance we talked about Grimsby recalling old friends who we both knew. Jean was not only beautiful but also very interesting and that one dance was the most enjoyable that I have ever had.

But how could I see Jean again? I was still an undergraduate with very little money and an uncertain future. I did not know where Jean lived or how to find her telephone number. For over a year I lost touch with her. Now in early 1931 I met her at an informal party and dance - an annual affair put on by the M&P Society. I lost no time in getting a dance with her, told her I would like to see her again soon - could I have her telephone number? She gave me that number and I made good use of it. Early in the following week I called Jean; she was free for that evening and that was our first date, but far from the last. We would go to the movies, to plays, to dances at the Embassy Club or the Savarin. We had fun! In the spring we played golf, went to the races at Dufferin and got caught in a sudden downpour. We laughed about that - "In all kinds of weather, what if the sky should fall, as long as we were together it didn't matter at all." I soon knew that I could not face the future without Jean and told her so - we were married on July 31, 1931 and for more than 50 years Jean was my constant and lovely companion.

In the few weeks prior to our wedding I took stock of my finances. From my income as a fellow and extra money earned by tutoring a few pupils, invigilating at U of T examinations, marking Upper School examination papers for the Department of Education, and teaching a small Summer School class, I had saved a fair amount. I took out a Life Insurance policy, naming Jean as the beneficiary, and with the aid of an interest-free loan, purchased a small inexpensive Ford roadster. From August 1 to August 15 Jean would be on Holiday from the Reference Library and I could give my Summer School class a short break. Jean wanted us to visit her brother Charles, an engineer in Pittsburgh, and from there we thought we might travel to Washington and perhaps Virginia. That little Ford roadster would serve to reduce travel expenses and also leave us free to go where and when we pleased.

On our return to Toronto in mid-August Jean went back to work at the Reference Library and I returned teaching my Summer School class. In September I would have my income as a fellow in Applied Mathematics and could earn extra money by tutoring two or perhaps three pupils. The depression was now deepening and rules forbidding work for married women were being enforced - Jean was able to stay on at the Library only until about mid-January, 1932. During those few months we put all the money we could spare into our joint savings account - perhaps those savings would see us through until September 1932 when, if all went well, I might expect a promotion to Lecturer in Applied Mathematics.

As a fellow in Applied Mathematics my work was fairly heavy: I had to mark weekly test papers for three rather large classes, give a course in Calculus to third year Honours Science students, complete two rather difficult graduate courses as part of my work for a PhD, and find time for tutoring. At the end of that academic year (1931-1932) I was appointed Lecturer and we could now see the end of our financial worries.

From September 1932 until May 1941 I gave many lecture courses to undergraduates in M&P, completed extra graduate courses and my thesis for a PhD. In the years 1938-1941 Professor Synge and I worked together writing the text book *Principles of Mechanics*, published by McGraw-Hill in 1942. Two later editions were to appear, one in 1949 and the other in 1959.

In the spring of 1941 I was given leave of absence from the U of T and we travelled west to Southern Alberta where I was to work for the Department of National Defence at the Field Experimental Station near Suffield. This work is classified and I am not at liberty to say much about it. Between September 1941 and July 1945 I wrote some 18 Suffield Reports, served as chairman for a special committee in the United States, gained some experience in the application of statistical methods, and learned some meteorology. In my years at Suffield I came to like the open prairie very much. Our area was located near the southern border of the Badlands and parts of it were populated by antelope, coyotes, rattlesnakes, black Widow spiders, and a few scorpions. Life at Suffield was rather interesting and sometimes dangerous.

When the war ended in August 1945, Jean and I with our two children (Doug, born on June 30, 1932 and Lynne born on January 15, 1943) set out for Toronto - a long tedious drive. Our furniture was in storage in Toronto so I left Jean and the children at a summer cottage in Haliburton and drove down to Toronto to find an apartment. There were no apartments to be

found and I was forced to buy a very small bungalow which would do until we could find a larger home.

We got settled and I checked in at the University only to find that, in my absence, some important changes had taken place. In 1943 Professor Synge had left Toronto to take a senior position at Ohio State - there was no longer a separate department of Applied Mathematics. The staff members of that former department (Stevenson, Infeld, Weinstein, and myself) were now to form a division of the Department of Mathematics and come under the administrative direction of Dean Beatty. Thus my official title was Assistant Professor of Mathematics not Applied Mathematics.

Shortly after my return to Toronto, Dean Beatty spoke to me saying that he had a small grant to finance a study of computer activity in the United States - would I take part in such a study? This I was pleased to do and so I joined the Toronto computer committee. The original members of this committee were Dean Tupper from Engineering as Chairman, Professor W. J. Webber (Mathematics), Professor A.F.C. Stevenson (Applied Mathematics), Professor Colin Barnes (Physics), Professor V. G. Smith (Electrical Engineering), and myself. Throughout the remainder of 1945 and the winter months of 1946 we met frequently to exchange information about computer activities in the United States and to agree on centres of interest that we might visit. I acted a Secretary-Treasurer of the Committee and, in that capacity, wrote to many centres in the United States asking if we might visit them in the spring of 1946. Each of my letters was answered with a warm invitation to come and discuss matters of mutual interest and to see, at first hand, their computing facilities where these existed.

Early in June of 1946 our committee (ex Dean Tupper) set out on a tour of the eastern United States and visited the Naval Research laboratories in Annapolis, the Pentagon in Washington, the Moore School of Electrical Engineering in Philadelphia, Aberdeen Proving Grounds, and Princeton University. From Princeton we drove to New York to visit the Bell labs, I.B.M. Headquarters, and a small Univac Research group. From New York we drove to Boston stopping on the way at Brown University; in Boston we spent two or three days at Harvard and MIT and then started on our way home, but did stop in Burlington VT for talks with Dr. George Stibitz.

From these visits, over a period of two to three weeks, we formed an impression of what

the future might hold for computers. Some of the highlights of our tour were:

1/ the visit to the Moore School where ENIAC was then processing a program written by Professor Hartree. We were much impressed by the speed of ENIAC but it was a specialized machine destined for Aberdeen Proving Grounds and designed for ballistic calculations. For this purpose its limited storage capacity and lengthy set-up time were of minor importance.

2/ The visit to Princeton where we met von Neumann. His views on the future for electronic computers were most interesting and inspiring.

3/ The visit to the Bell Labs where we saw two large relay computers each built to a design due, in large part, to Dr. G. Stibitz. These two computers were intended to work simultaneously on the same problem and check each other at each stage of the computation. Disagreement at any point would bring computation to a full stop. Computation could resume only when the reason for disagreement was found and the fault (or faults) corrected.

4/ The visit to the Univac Research Group where we met a few engineers experimenting with magnetic tapes. To us these tapes promised a practical means for back-up storage but we had no idea of the importance these tapes would have in the not too distant future.

5/ The visit to Harvard where we met Mrs. Grace Hopper who was then busily engaged in calculating Bessel Functions to 23 places of decimals using the Harvard Mark I. This computer was a relay machine and very slow compared to the ENIAC. The Mark I was, I believe, largely the work of Commander Aiken who was absent at the time of our visit.

6/ The visit to MIT where the Differential Analyzer with its several integrating tables occupied a very large room - perhaps larger even than the room where ENIAC was housed. We were told that the Differential Analyzer was normally capable of four decimal digit accuracy but that perhaps five digit accuracy could sometimes be achieved. As I recall it was used, for the most part, in solving differential equations numerically.

Back in Toronto I gave Dean Beatty a summary of our tour and a brief financial report;

then I left Toronto for a brief holiday with my family. Whilst on holiday I had much to think about. How could a small computation group be established? What equipment would it need and where could we find working space and funds for such a group? Could we look forward to building an electronic computer of our own?

In collaboration with the Actuarial Science Department, I had already planned a numerical laboratory course in calculations of a statistical or actuarial nature. For calculating equipment we could share a small number (three or four) very old manually operated calculators that we had resurrected, an old Monroe calculator, and two electrically operated calculators that we hoped to purchase. Perhaps this course would spark an interest in Numerical Computation for a few bright students[1].

I returned to the University long before the fall term opened and was able to discuss some of the above with Professor V. G. Smith. He was much interested in building an electronic computer and had even then drawn up preliminary plans for the arithmetic unit and was studying various methods for storage of program instructions and numerical data.

From September 1946 until the end of 1947 my time was fully occupied . In addition to a rather heavy load of lectures and the preparation of numerical exercises for the laboratory course mentioned above, I undertook or became involved in two important projects. The first project (Project A) was the establishment of a small computer group using IBM punched card equipment. The second (Project B) was my involvement in preliminary plans for a computation centre with an electronic computer.

Project A

Late in 1946 I met with sales representatives of IBM (Canada) and was able to learn from them the equipment I might need for a small computation group such as I hoped to form. They recommended a calculating punch (the 604 or 604A) and ancillary equipment: a keypunch, a sorter, a tabulator, and perhaps a collator. To the rental cost of this equipment for one year, I

[1] Much later (in the late 1950s or early 1960s) one of these first students (Curly Graham) went to Waterloo University and there he played an important part in starting a Computing Centre. I believe that he served as director of that centre for a considerable time.

added the estimated cost of two junior assistants and arrived at a total of about $7,000 for the first year.

With the approval of the Computer Committee I applied to the Canadian National Research Council (NRC) for a grant of $7,000. In this application I pointed out that

(a) If approved, this grant would provide financial support for a small computation group using IBM equipment

(b) U of T could provide working space and staff time to help with the work of this group, and

(c) This computation group could be regarded as a first step toward the formation of a computation centre equipped with an electronic computer.

This application met with a favourable response and I placed an order for the IBM equipment which was delivered in late March or early April 1947. Space for the equipment had been found in the Physics Building, almost certainly due to the influence of Professor Barnes.

To operate the IBM equipment I hired Perham Stanley and Miss Sachs as an assistant to Stanley. Miss Sachs had been recommended by Professor Norrie Sheppard of the Actuarial Science Department and Stanley was a recent graduate of M&P with an excellent record in mathematics. I suggested to Stanley that he might undertake a numerical study of hypergeometric functions. I do not remember that I gave Miss Sachs any definite task other than learning to use the IBM equipment and to assist Stanley.

In June or early July of 1947, Harvey Gellman came to me saying that he was unhappy in his work at Port Hope with the Canadian Atomic Energy Commission - could he join our computation group? To employ Harvey meant that we would have three assistants and that our funds might prove insufficient. After some consideration I decided to take a chance and hired Harvey: that was to prove one of the best moves I ever made.

To become familiar with the IBM equipment, Harvey worked for a time with interpolation methods. Then, at the suggestion of staff members at the Canadian Atomic Energy centre at Chalk River, Gellman and Stanley worked together on the numerical evaluation of internal conversion coefficients for gamma radiation from the K and $L_1$ shells corresponding to various atomic numbers. In this work hypergeometric functions and interpolation methods were both involved and so the early work of Stanley and Gellman was quite useful. Their results were

published in two papers, one by Stanley[1] and the other by Gellman et al[2]. My name is mentioned in the first and included as co-author of the second: my part in the work was very minor. I had merely carried out an independent check of the rather tedious simplification of Hulme's [3] formulae for a number of cases.

In late 1948 or early in 1949 Stanley went to England to pursue post-graduate study and do some computer work. Gellman continued to work at Toronto: for the most part his work was with Professor Bullard who had come to Toronto in the autumn of 1948. Until early in 1951 Gellman's work in computation was with IBM equipment; thereafter he worked with the electronic computer which was installed in April 1951.

During the time that Gellman worked with Professor Bullard at least two papers were published [4,5] in which Harvey is named as one of the co-authors. In the same period Gellman completed several graduate courses in Physics and submitted a thesis for a PhD. In the mid-1950s, perhaps as early at 1953 or 1954 Gellman left the Computation Centre to establish a small but very successful computing firm known as Harvey Gellman and Associates. To the best of my knowledge that firm is still active and Harvey has not yet retired. For many years Harvey Gellman has been known as one of the leading Canadian authorities on the use of electronic computers.

As for Miss Sachs, her early work in Project A was rather routine. Later she used the IBM equipment in the preparation of at least two geophysical reports printed by the Department of Physics. These reports are dated August 1955 and February 1, 1956; the research involved was made possible through the support and sponsorship extended by the Geophysical Research Division of the Air Force Cambridge Research Center. I am named as co-author of one of these reports; Professor Jack Jacobs[2] and Miss Sachs are named as co-authors of both. I believe this work was carried out long before the dates shown on the reports.

Apart from part-time work with Stanley, Gellman, and Miss Sachs, I did work under

[2] Jack Jacobs had come from England in the Autumn of 1948 as a replacement for Professor Stevenson. He and I worked together for several years and became good friends. In the mid-1950s Jack became very interested in Geophysics and did some theoretical work with Tuzo Wilson. Later Jack transferred from Mathematics to Geophysics, went west to British Colombia, and later to the University of Alberta in Edmonton. After the death of his wife, he returned to England and an important position in Geophysics.

Project A on a problem of my own. In the spring of 1948 I undertook the solution of $n$ simultaneous linear equations for $n$ unknowns. In these equations the matrix of coefficients for the unknowns was a non-singular $n \times n$ symmetric matrix $A$. Using our IBM equipment I succeeded in solving the equations and inverting the matrix $A$ for $n = 14$. This problem arose in connection with some actual data for which I was using linear regression analysis.

Project B

Following our tour of the eastern United States in June 1946, Professor V.G. Smith had kept in touch with computer activity in the United States. By early 1947 he was following with great interest the work at the University of Illinois. At the time, I was not quite certain whether this work was, as yet, only in the planning stages or whether some construction had already begun. At the meetings of the Computer Committee in the winter of 1947 V.G. gave us much interesting information about the work at Illinois and their plans to build an electronic computer to be called the Illiac. V.G. felt that several of their plans could be of use to us if we were to start building an electronic computer of our own.

I t was clear to the other members of our committee that V.G. was now anxious to start building an electronic computer. How long would this project take and how much would it cost? Could V.G. give approximate answers to these questions? If not, could he learn from Illinois their answers to these same questions applied to the ILLIAC project? As secretary of the committee I undertook to draft an exploratory letter to the N.R.C. telling them of our hopes to build an electronic computer and asking advice as to how we might obtain financial support for such a project. In my first draft of this letter I left cost and time estimates blank pending the receipt of information that V.G. might give me. As I recall, I mentioned this draft letter to the other members of the committee and may even have discussed it with them.

In the spring of 1947 (late May or early June) I received an invitation from the President of the University, Sidney Smith, to visit him in his office and tell him of our plans to build an electronic computer. When I met with President Smith he was most friendly and asked me if I would give him a short resumé of the work of our Computer Committee. This I did and concluded with an account of V.G. Smith's continuing interest in the computer activity in the

United States - especially his interest in the work at Illinois and their plans to build ILLIAC and his plans to build one of our own. To embark on this building project we would need to be assured of adequate financial support. President Smith asked several questions which I was able to answer but when he asked me to estimate the financial support we would need I could only give him a very rough estimate - perhaps a quarter of a million dollars or possibly somewhat more. I went on to say that V.G. was endeavouring to learn the expected costs for ILLIAC and that might serve as a basis for a more reliable estimate of our own expenses. President Smith then asked about the future importance of electronic computers. Here I could tell him about their importance for scientific research, the potential for storage and rapid retrieval of information such as statistical data, medical or legal records, etc, and their probable importance for business and industry. He then thanked me for the information I had given him, said that he would take the matter under consideration and perhaps we would need to meet again. He might have further questions and we could discuss ways of finding financial support. When we met again he asked me for details concerning my proposed exploratory letter to the NRC. These I gave him and he agreed that I should send that letter - it might be quite helpful, at least it could do no harm. He himself said that he might be able to help and would do whatever he could. I knew that he had important political connections and the he could possibly be of considerable aid.

Later in the same summer a letter arrived from the Defence Research Board (DRB) addressed, I believe, to the Chairman of the U of T Computer Committee. This letter must have been referred, probably by Dean Tupper, to Dean Beatty. In it the Chairman of the committee and myself were invited to come to Ottawa to meet the director of the DRB, Dr. Oman Solandt. Dean Beatty agreed to act for Dean Tupper and he and I went to Ottawa. In this meeting Dr. Solandt directed most of his questions to me, asking about computer activity in general, the future of computers, and why a university should be involved. I gave Dr. Solandt a fairly complete account of computer activity in the United States, pointing out that practically all the work on electronic computers was at a university. Regarding computer activity in Britain or elsewhere we had no knowledge. However the fact that Professor Hartree had, in early 1946, come from Cambridge to work with ENIAC in Philadelphia would indicate that there was definite interest in electronic computing in England. To deal with the future importance of computers I repeated most of what I had told President Smith in Toronto. As to university

involvement I pointed out that at a university there would be many bright students and some of them could be expected to take an interest in electronic computing. From among such students there could be some who would pursue a career in computing; others might become teachers in our schools or join the work force in business or industry. Thus from a Canadian university information about computing might be spread widely throughout Canada. Also at a university the staff would represent a great variety of interests - arts, science, engineering, medicine, etc. In this atmosphere one could expect that new or unusual uses for a computer would be suggested.

Dr. Solandt then asked me, if financial support was forthcoming, when could we start work? I said immediately! Professor V. G. Smith had already done considerable planning, could employ the services of recent graduates, and direct their work with the assistance of Professor Barnes and perhaps others. I went on to say that the University would provide working space and that the part-time services of members of the University staff would be available - all at no cost to the project. At Toronto the only costs to the project would be the salary of a full-time director when needed, the salaries of all full-time assistants and equipment costs. Dr. Solandt had, as I recall, no further questions and our meeting came to an end.

In the autumn of 1947 we were advised by Ottawa that the sum of $300,000 ($150,000 each from the DRB and the NRC) would be available to the U. of T. as support for the building of an electronic computer and the establishment of the computing centre. There may have been several factors that contributed to our success in obtaining this financial support.

1. President Smith's political connection could have had some effect, either directly or indirectly.
2. Dr. Solandt was a graduate in medicine from U. of T. and would take a favourable view of work at his Alma Mater.
3. The DRB and the staff at Atomic Energy of Canada at Chalk River had need of a high-speed computing facility. The NRC was also interested in computing.
4. Among Dr. Solandt's staff there were at least two members with whom I had worked at Suffield. They could have been helpful.
5. The work of our Computer Committee, my exploratory letter to the NRC, and the subsequent talk with Dr. Solandt may have been of some importance.

Whatever the reasons for our success, we were most grateful for the support we had been given. Now all my questions of the summer of 1946 were answered or about to be answered in a very

positive way.

Work on Project B was begun in late 1947 and continued until December 1950, when the decision to purchase a Ferranti Computer was made. In those three years Professor V.G. Smith took a very active part in Project B as did Professor Colin Barnes. In September 1948 Professor Bullard came to Toronto as Head of the Physics Department, acted as Chairman of our Computer Committee, found space in the Physics building where all work on Project B could be consolidated, and, in mid-1949 appointed Dr. C. C. (Kelly) Gotlieb as Chief Computer. In that summer of 1949 I was at Chalk River learning something about the Atomic Pile, talking about computer activity in the United States and doing some statistical work. When Professor Bullard told me that he was appointing Kelly as Chief Computer I said that I could think of no better choice. I had known Kelly as a student of Applied Mathematics in the third year of the M&P programme and had been impressed by his ability. Following his appointment Kelly acted as full-time director of the work on Project B, and from early 1951 on as director of the Computation Centre at U of T until his retirement some 40 years later.

The first employees in Project B were Alf Ratz, a promising recent graduate in Electrical Engineering, and Josef Katz who was then in his final undergraduate year in M&P. In November 1947 Katz had applied to me for work in Project B and I referred his application to V.G. Smith, adding a strong recommendation of my own. Throughout his undergraduate years Katz had worked as a full-time employee of Rogers Majestic where he had made many significant contributions in electronics. Somehow he managed to achieve high honours in his studies although seldom able to attend lectures. He would do the same in working for Project B and would be of great use to us.

In the years between December 1947 and December 1950 I had very little to do with computing other than my work on Project A which I have already described. As for work on Project B, I could be of no help - I was a user of computers, not a builder. In any case I had little time to spare from my academic duties. I was deeply involved in the development of a small statistical division within the Department of Mathematics and also with the reorganization of the Applied Mathematics division following the loss, in 1948, of Professor Infeld and the resignation of Professor Stevenson. However, I did find time to keep in touch with progress in Project B. When Joe Katz developed the binary adder tube (BAT) I soon heard of it. It appears that Alf

Ratz had been of some help to Joe in this development and there was considerable badinage: remarks such as "Katz and Rats are working on Bats", and the like, were common. Joe did not appreciate this good natured humour; in fact I think he was rather annoyed. Soon he went to the trouble of having his surname changed officially from "Katz" to "Kates" - after that the witty remarks ceased.

In the summer of 1950 I was again at Chalk River giving a few lectures on statistics and working on a research problem. During that summer a senior staff member at Manchester University visited Chalk River and gave a lecture on the computer they had constructed. I think that visitor was Professor F. C. Williams but perhaps it was one of his colleagues - in what follows I shall refer to him as Professor Williams. That lecture was one of the most interesting that I can remember. In a very simple yet clear manner Professor Williams described the Manchester computer as a robot, able to obey a sequence of instructions, one after the other, and equipped with a large sheet of paper that could store any amount of data. For that robot one could provide a sequence of instructions: to identify these instructions Professor Williams did not give their technical codes, but used plain English words such as Read, Store, Bring, Add etc. For each of these instructions he explained, in a simple way, the operation that the robot would perform. Now he wrote out a short sequence of instructions (a complete program) and explained how, by following this sequence of instructions in order, the robot could find the sum of a convergent series to any desired degree of accuracy. This program contained a 'loop' - a very useful device in computer programming. The material in this lecture stayed with me for all the years in which I was to work with electronic computers, at U. of T. and later as a computer consultant.

In December 1950 representatives of the DRB, the NRC, and as I remember, of the staff at Chalk River came to Toronto for an important meeting with senior people in our computing group. Could we set a firm date for the completion of Project B? If not, there was now a commercial firm, Ferranti Electric in England, which was building electronic computers patterned after the Manchester Mark I and one such machine could be delivered in Toronto within a few months. Should we purchase an electronic computer from Ferranti, discontinue work on Project B, and put on the shelf the work we had completed? The staff at Chalk River could use computer aid as of yesterday. Our expenses to date had not been very great - there was

enough left of the original fund of $300,000 to cover the cost of a Ferranti computer. Since we could not set a firm date for the completion of Project B, the final decision in that meeting was clear - we would purchase a Ferranti computer. This decision must have been a great disappointment for V. G. Smith and others in our group: for me this decision was less important as I would soon be able to gain first-hand experience with an electronic computer.

In April 1951 the Ferranti computer, to be named FERUT, was delivered in Toronto and set up in the Physics building. With this computer Ferranti sent a few people to help us - one or two engineers to train our own engineering people in the maintenance of FERUT, and programmers to provide training for our computing staff in the use of that computer.

While FERUT was being installed and tested ready for operation, the Ferranti programmers were giving training lessons to member of our computing group and other interested staff members or graduate students. I would have attended those training lessons but was not free at any time that they were being given. When I was free to visit the Physics building I found the computer area a veritable hive of activity. Would-be programmers busy punching paper tapes with instructions and numerical data; others were busy splicing corrections in tapes they had previously prepared. I found a copy of the coding manual and retired to a quiet corner to learn about programming.

For my first program I selected the problem that I had solved with IBM equipment, namely the solution of $n$ linear and linearly independent equations for $n$ unknowns, together with the inversion of the $n \times n$ matrix of coefficients. Now the number $n$ was no longer limited to $n \leq 14$; I attempted to write a program that could deal with any value of $n$ up to $n = 40$. For test data I constructed a set of some 20 linearly independent equations and then prepared a tape, complete with instructions and numerical data. When I took this tape to the console of FERUT it did not run - due to the fact I had not been able to attend the training lessons, I was not aware of a few special codes that were very necessary. Now, when I found out about these, I repaired the tape and it ran quite successfully. That program, which I shall refer to as Program I, became one of the library programs and was used by Joe Kates to good advantage in regression analysis.

Although Project B was now discontinued, the principals in the project took time, in early 1951, to write up a fairly complete account of their work. In particular Joe Kates wrote up a detailed account of his developments and presented this as a PhD thesis. During the years from

1948 to 1950 Joe had completed several graduate courses in Physics. Now when he presented his thesis he had completed all the requirements for the PhD in Physics and received that degree, as I recall, in the spring of 1951 or 1952. To most he was now known as Dr. Kates - to us he was always Joe. For a few months Joe worked with FERUT and became quite a good programmer. Then, with two colleagues, Len Casciato and Joe Shapiro, he formed a small computer service firm, incorporated as KCS Data Control. This firm was founded on a very frayed shoestring. They rented a small office in Bay Street, engaged the part-time services of a secretary - an employee of another tenant in the same building - and set out to interest potential clients in the services they could offer. Before long they acquired three major clients - an integrated oil company, the Ontario Department of Highways, and the Metropolitan Toronto Planning Board. The work load grew and staff had to be hired. They then moved to a much larger office further north on Bay Street, hired a full-time secretary, rented an IBM 650, and did not look back until in 1967 KCS merged with a large management consulting firm.

During August 1951 the Canadian Mathematical Congress was scheduled to meet in Halifax. Practically all members of the staff in Mathematics at Toronto planned to attend and I was no exception. Jimmie Chung, a recent PhD in Group Theory was now working full time in the computing centre and he would also attend the Congress. I had been asked to give one or two lectures to the Congress dealing with computers and arranged with Jimmie Chung that I would give a general lecture about computers and that he would give a second lecture dealing with FERUT and its operation. There was considerable interest in these lectures but they had little lasting effect for the Pure Mathematicians at Toronto. Back in Toronto I found myself involved with various activities in addition to my normal academic duties. Over the next few years these extra activities included:

1/ Instruction classes and/or discussions dealing with numerical methods

2/ Helping a few PhD candidates with the preparation of their theses.

3/ Writing one or two computer programs (far less than I had expected).

4/ Helping with programming instruction together with Professor J.N.P. (Pat) Hume and Kelly Gotlieb.

5/ Acting as Examiner-in-Chief for Upper School Trigonometry - an activity covering a period of three successive years (1952 to 1954).

6/ Working for the DRB at Valcartier during the summer of 1952.

Activities 5 and 6 had little to do with computing but took time away from work that I might otherwise have done.

The classes dealing with numerical methods were initiated by Kelly Gotlieb soon after FERUT was installed and were, I believe, very helpful to our computing group. Kelly himself gave some lectures dealing with interpolation, iterative procedures and filing problems. Prof. Ben Etkin of Engineering Physics gave several interesting lectures on Relaxation Methods. There were many discussions dealing with various topics such as numerical integration, the numerical solution of differential equations, etc. In these discussions I may have been of some help. In the early summer of 1953 I made my main contribution by giving a fairly full account of Regression Analysis dealing with one dependent variable $y$ and several independent variables $x_i$ ($i$ = 1,2,3,...,$n$). Starting from $N$ observed values for $y$, namely $Y_\alpha$ ($\alpha$ = 1,2,3,...,$N$), each of which corresponded to a known or fixed set of values $X_{i\alpha}$ ($i$ = 1,2,3,...,$n$) for the variables $x_i$ one forms by the Principle of Least Squares, the normal equations

sum from {j = 1} to n A SUB {i,j}b SUB j = l SUB i horz 200(i = 1,2,3,...,n)

(1)

y = sum from {j = 1} to n b sub j x sub j

A sub{i,j} = sum from { alpha = 1} to N X sub {i alpha} X sub {j alpha}

Here, the quantities $b_i$ represent the coefficients in a linear regression formula

And

l sub i = sum from {alpha = 1} to N Y sub alpha X sub {i alpha}

Since some, or perhaps all, of the $x$'s could each be a function of several subsidiary variables $u$, $v$, $w$,... the linear regression formula could actually represent a rather complex function $y$ =

f($u$,$v$,$w$,...)

In 1951 I had written Program I to solve the equations (1) and invert the symmetric matrix ($A_{ij}$). The method used was quite systematic and determined not only the final regression formula

y = sum from {j = 1} to n b sub j x sub j

y = sum from {j = 1} to k stack{(k)#b sub j}x sub j

but also each intermediate regression formula

for any values of $k$ in the range $1 \leq k < n$. An observed value for $y$, say $Y_\alpha$, would differ from the value predicted by the $k$th regression formula, viz

sum from {j = 1} to k stack{(k)#b sub j} X sub{j alpha}

by

Y sub alpha - sum from {j = 1} to k stack{(k)#b sub j} X sub{j alpha}

and the total variation of the observed $Y$'s about the $k$'th regression formula would then be represented by

V sub k = sum from {alpha = 1} to N (Y sub alpha - sum from {j = 1} to k stack{(k)#b sub j} X sub {j alpha})sup 2

Similarly,

V sub{k-1} = sum from {alpha = 1} to N (Y sub alpha - sum from {j = 1} to {k-1} stack{(k-1)#b sub j}X sub{j alpha}) sup 2

would represent the total variation of the observed $Y$'s about the ($k$-1)st regression formula. Clearly, the difference $V_{k-1} - V_k$ is the extra variation explained by $x_k$ over and above the variation explained by $x_1$ to $x_{k-1}$. Thus the extra variation E($x$) explained by each $x$, in addition to that explained by its predecessors can easily be determined. Each variable $x$ for which E($x$) is less than some agreed value such as $4e^2$ where $e^2 = V_n/(N-n)$ could be regarded as unimportant and might well be omitted from consideration. We could then arrive at a final simplified regression formula

y = {sum from{j = 1} to n} ' b sub j sup * x sub j

where the primed summation sign indicates summation over all $x$ values except those considered

unimportant.

If for some value of k all $x$'s from $x_1$ to $x_k$ were important and all $x$'s from $x_{k+1}$ to $x_n$ were unimportant, the final simplified regression formula was obvious - it was the $k^{th}$ regression

y = sum from {j = 1} to k stack{(k)#b sub j} x sub j

given by the systematic solution of the equations (1). However if $x_{k+1}$ were unimportant and any $x_s$ (s > k+1) was important then the final simplified regression formula was not readily available. In this case I was able to describe a procedure for finding the final regression formula even though it was not readily available from the systematic solution of equations (1).

Joe Kates was very interested in the possibility of simplifying a regression formula by the omission of the unimportant independent variables. He asked me if I would write a computer program to identify all the unimportant independent variables in a linear regression problem and to produce the simplified regression formula resulting from their omission. Over the next few days I managed to write that program which I shall refer to as Program II since it was essentially a companion program for Program I. Joe Kates had been given a regression problem by one of his clients and thought that problem could be a suitable one with which to test Program II. He give that regression problem together with my coding for Program II to his partner Joe Shapiro who then undertook the testing of my program. I think he had a few difficulties for he came to me with some questions which I was able to answer. He may also have found a 'bug' or so and perhaps a 'beetle' in my program. In any event he was soon able to get Program II to work. During the time that KCS worked with FERUT, Programs I and II were used to advantage. Much later, when KCS acquired a computer of their own, I was able to help their programmers to rewrite Programs I and II in Fortran and those programs proved to be an important part of the software available to KCS.

The first PhD candidate with whom I worked was Miss Beatrice Worsley - always known to members of our computing group as Trixie. She had graduated with a BA degree in Honours Mathematics at Toronto during World War II. In late 1945 or early 1946 she had gone to England for post-graduate study at Cambridge. There she became interested in the work on electronic computers and for her PhD thesis she undertook to write an account of the early pioneer work on the construction of electronic computers at Cambridge, Manchester, and the NPL. For reasons unknown to me she left Cambridge and returned to Toronto before finishing

her thesis. I believe that must have been in early 1950. Before leaving Cambridge, Trixie was able to make arrangements to complete her thesis in Toronto. The authorities at Cambridge merely required that she find a senior staff member at Toronto who would act as an extramural representative of Cambridge and supervise the completion of her thesis. In the autumn of 1950, Trixie came to my office and asked me if I would agree to act as her supervisor until she completed her thesis. I agreed to help and during the next few months Trixie gave me quite regular progress reports to read. Her work was well organized and clearly written - I had no need to do more than make some encouraging comments and perhaps a few minor suggestions. In a few months, 5 or 6, Trixie completed her thesis, had the required number of copies typed, bound, and sent to Cambridge. Soon she was granted the degree of PhD and now continued to do valuable work for our computing centre until at least 1959 and probably for many years thereafter.

In 1952 Trixie worked with Pat Hume in writing a program for FERUT that was known as TRANSCODE. In part that was an interpretive routine that enabled FERUT to accept simple mnemonic instructions and convert them into the usual FERUT instructions. That program was more than a simple interpretive routine: it contained a number of subroutines for the calculation of some transcendental functions such as trigonometrical functions and probably $e^x$, log x etc.

The mnemonic instructions provided by TRANSCODE were very simple and proved useful in the training of would-be programmers. Pat Hume and Kelly Gotlieb initiated evening courses of about 20 sessions for the training of those wishing to learn computer programming. In each session, Pat or Kelly would present, as an example, a simple program, point out the need for each instruction and describe the operation that would be performed by the computer in response to that instruction. Then one or two exercises, similar to the given example, would be assigned to the members of the class. In addition to Pat and Kelly there were always a few volunteers, some from the computing centre and others with experience in programming. These were available to assist, in a tutorial manner, members of the class who had any difficulty with the assigned exercises. Joe Kates and I often acted as two of these volunteers.

I seem to recall that the first of these training courses was given during the academic year 1952-53 but it might have been somewhat later in the year 1953-54. That course was repeated for perhaps two or three succeeding years with other groups of trainees.

In 1954 my work as a lecturer was greatly changed. In late February of that year my voice had become quite husky and I was advised to seek medical advice although I felt it was simply the result of straining my voice in lecturing to one or two rather large classes. the trouble was found to be a malignant growth in my throat and in early March I underwent an operation to have that growth removed. The operation was successful in removing the cancerous growth but left me without a larynx (voice box) and only able to breath through an opening in my throat. Now I had great difficulty in speaking but with help from Mr. Carasso, who had had the same operation a few years earlier, I learned a method of speaking called oesophageal speech. With that method I could, with considerable effort, speak slowly in a low growl or croak that could be heard at a short distance if there were no background noise.

For the next four years I carried on lecturing to a few very small classes, often with the aid of a microphone, and was able to continue with the numerical work for the laboratory course in statistics. Having a lighter load of lecturing I now had more time for computational work and was able to act as supervisor for four PhD candidates. I also was able to spend some time with individual programmers in the Computing Centre suggesting a numerical method that might be useful in a particular problem.

The four PhD candidates with whom I worked were 1/ Kac Okashima, a young Japanese-Canadian student; 2/ Keith Smillie, 3/ W. Kahan (known to us as Vel), and 4/ Stuart Baxter.

Okashima and Smillie each worked on a problem in statistics - Okashima with data supplied by the Meteorology Service and Smillie with data supplied by members of the staff in the Department of Biology at U of T.

In dealing with the data he had been given Okashima wrote one or two computer programs and the statistical analysis of the data brought out a few interesting things. When complete his thesis was quite a good piece of work and he defended it satisfactorily in late 1955 or early 1956. After Okashima received his degree I lost touch with him and have no knowledge of his later work.

Smillie's work was rather similar to Okashima's in that he too wrote computer programs to deal with the data given him by biologists. However his analysis of that data was quite different. Smillie's thesis, when complete, gave a clear account of his work and he defended it successfully in 1956[1]. After receiving his degree, Smillie continued his work in statistics, often

using electronic computers. In 1963, or perhaps late in 1962, Smillie joined the staff in the Department of Computing Science at the University of Alberta. He remained there for almost 30 years and has only recently retired. In 1966 he published a text dealing with correlation and regression analysis for users of electronic computers. Very kindly, Smillie sent me a complimentary copy of that text - I found it quite interesting. Afterwards, Smillie continued to work with computers and to teach many classes - understandably he has had a very useful career.

Kahan must have started work with the Computing Centre about 1955 and, after completing his MA thesis in 1956, undertook a study of numerical methods for the solution of partial differential equations of the elliptic type. Kelly Gotlieb referred Kahan to me since the Computing Centre was not, as yet, a teaching department of the University and could neither sponsor nor supervise graduate students in work toward a degree. When Kahan began his work with he had already identified many problems which involved the solution of a large number of simultaneous linear equations. He made a thorough search of the recent literature dealing with methods used by electronic computers in the solving systems of $n$ simultaneous linear equations in which $n$ could be several hundreds or even a few thousands. Kahan then set to work establishing new methods and improving older methods that could produce solutions for such large systems with reasonable accuracy.

In his work Kahan showed both originality and skill, but his proofs were often quite concise and not easy to follow. He had little need of any help from me though my efforts to verify his results, often with the aid of explanations by Kahn, may have been of some small benefit. My main contribution to Kahan's work came later after he had achieved many results. Kahan was not greatly interested in writing an account of work he had completed - to him that was 'old hat' and he grudged the time taken from further research to write an account of past achievements. However, by 'twisting his arm' I finally persuaded him to start writing his thesis.

Once started Kahan wrote an excellent account of his work, complete with reference to previous work of others and examples illustrating his own work. I read the whole of this account carefully, had a few comments, and only one or two suggestions for minor changes. At his departmental oral, Kahan defended his thesis admirably and his examiners were unanimous in their approval - in fact the examiner who submitted the required appraisal to the Graduate School had this to say: "In this appraiser's opinion the present thesis is one of the most outstanding

which he has been privileged to read."

After receiving his degree Kahan resumed his research work on large systems of linear equations, adding many new results. For a short time he continued to work in Toronto but then went, I believe, to California. In 1969 or 1970 he paid a short visit to Toronto and gave me an interesting report on his work since 1958. Among other things he had used numerical methods in dealing with problems in hydrodynamics. This was of particular interest to me since my own PhD thesis had dealt with a problem in this area and, in my years in Applied Mathematics, I had given undergraduate and graduate lectures in the subject as well as presenting a few papers at seminars.

I did not hear from Kahan for many years after 1970, but recently Kelly Gotlieb informed me that Kahan had been given the Turing Award for the most outstanding contribution to computing. This was indeed good news to me. Kahan's work in computation has been outstanding and is not yet finished.

The last of my PhD candidates, Stuart Baxter, selected for his thesis the problem of solving partial differential equations of hyperbolic type. His work on this problem consisted, for the most part, in skilfully writing quite difficult computer programs. His work could not be considered outstanding, as was Kahan's, but it was good work and in 1958 his earned his degree. For post-doctoral work he continued, for a time, working at the U of T Computing Centre but he later joined the staff at NRC. In the mid-1960's NRC acquired their own electronic computer and established a computing centre for which Baxter was appointed as Director. Except for a brief period in which he helped to establish a computing centre at Queen's, Baxter remained with NRC until his retirement in 1992. Stuart Baxter has done a great deal of useful work in the computing field.

My work at U of T was now nearing its end. The Computing Centre, under Kelly's able direction, was quite strong and needed little help from me - in fact, in that centre I would be something of a fifth wheel and a squeaky one at that. As an instructor, my lectures were limited to a few very small classes and even there my work was less effective than it once might have been. What voice I had tired badly at times and I came to realize that I was often of little help to my students. In the early spring of 1958 I had gone over to the offices of KCS to help Joe Kates with one or two projects and became quite interested. It was not long before I was spending

more time at KCS than at U of T. In fact I spent only enough time at the U of T to attend the few classes for which I was responsible.

When Joe Kates offered me a permanent position at KCS I considered it carefully. I would regret leaving U of T where I had worked for so many years; on the other hand I realized that I was no longer really effective as a lecturer. Joe's offer was a good one - a partnership in KCS plus a salary somewhat greater than my salary as an Associate Professor at U of T. In early June I decided to accept Joe's offer and, with regret, sent in my resignation to the Head of the Mathematics Department.

At KCS I had a quiet private office and no need to talk very much and then only for short periods. My work was partly administrative but I spent most of my time as an analyst devising methods for dealing with client projects and designing computer programs which KCS programmers coded in Fortran.

In my years with KCS I was involved in many projects, some more interesting than others. Three of the more interesting projects were:

1/ My work with Dr. Wyzecki of the NRC in dealing with problems related to the Geometry of Colour Space

2/ My work with a colleague, John Lion, in the development of computer programs for the preparation of school time-tables.

3/ The cross tabulation of survey data in which I found some of the elements of set theory to be useful.

The staff at KCS consisted of members from many different nations and this contributed much to the interest of working at KCS. Joe Kates himself had come from Austria, having escaped and made his way to England at the time of the Nazi Anschluss in 1938. Other foreign members of the staff included: 4 from Germany, 3 from Hungary, 2 from Belgium, 2 from Holland, and 1 each from China, France, Norway, Syria, and Indonesia or Thailand. There were also several from the British Commonwealth and several from the United States. In total the foreign-born members of our staff outnumbered those born in Canada.

Two of the German members of our staff had had rather unusual experiences in world War II. Hans von Cube had served with the German army in Russia and was present at the Battle of Stalingrad. There he became separated from his unit and wandered for several days,

lost in the snow and cold. He would have perished had he not found shelter with a Russian peasant family. When able he made his way back to his unit but was no longer fit for active service and was invalided back to Germany. After the war Hans, with his small family, came to Canada where, as a landed immigrant he worked as a farm labourer. For shelter they were given a small cabin in the Beverly swamp near Galt Ontario. Somehow they managed and later Hans was able to attend the Agricultural College at Guelph. In the mid-1950s Hans came to Toronto and was one of the first employees hired by the original partners of KCS.

The other German with an unusual experience was Werner Hirschman - he had been an officer aboard a German submarine. During the war that submarine spent the winter of 1942-43 on the floor of Halifax harbor. They were probably out of ammunition or afraid to use it: they did no damage to Allied shipping. In the spring of 1943 they managed to slip away and return to Germany.

In the summer of 1967 Joe Kates arranged a merger with a management consultant firm. Peat, Marwick et al. After the merger the firm was, for a time, known as Kates, Peat, Marwick. I was offered a partnership in that firm which I declined. I had no competence in, or liking for, management consulting: to me, it seemed that some of their work reminded me of "The Emperor's New Clothes". I finished the project on which I was working and then, in 1968, retired. Joe himself did not remain for long with that consultant firm and departed for greener pastures. He had retained control of the computer wing of KCS as a separate company known as Setak (Kates in reverse). With that and many other activities Joe kept busy.

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1. Keith Smillie reports that he actually obtained his PhD in 1952.