tion, other than the ASCC/Mark I, that could be called computers. These would include the Bell machines (associated initially with George Stibitz), the various machines used in code breaking such as the American "Bombe" and the British machines designed for the operations at Bletchley Park, and Konrad Zuse's Z3 in Nazi Germany.* I know of no evidence that the term "bug" was ever used in the context of a computer before the entry in the Log Book of the ASCC/Mark I. Accordingly, at least as of now, the first documented use of "bug" in relation to a computer was in Bob Campbell's entry in the Log Book in April 1944.**

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* As mentioned in the earlier footnote, there is no universal agreement concerning whether machines of the wartime years should be considered to have been computers. Some writers would restrict the designation of "computer" to digital machines, ruling out such devices as the differential analyzer; others would require that a "computer" must have a stored-program capability. In 1944 there were adaptations made of business machines for purposes of wartime computation, notably IBM machines. In England, A.J. Comrie had pioneered in adapting business machines to the needs of scientific calculation. It is even possible that the word "bug" may have been used by Charles Babbage or by any of the skilled mechanics working with him on the Difference and Analytic Engines.

** Should any reader know of any earlier use of this term, it is hoped that he or she will send a note about it to the editor of the *Annals*. [The editor heartily endorses this request.]

Biographies

ERIC A. WEISS, EDITOR

My Early Days in Toronto

Editor's note: In keeping with our Canadian theme, we include this memoir by B.A. Griffith.

I was born on November 16, 1908, in Hamilton, Ontario. I was only three 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 10 miles east of Hamilton.

That was a poor farm: The land had not been cultivated for many years and was covered with weeds and thorn bushes. There was only 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 the first several years, very much like that of early pioneer days. For me the simple life was a blessing in strange disguise for throughout my school days, from late 1915 until June 1926, there was little to take my attention away from school work.

In June 1920, at the age of 11, I passed the examinations for entrance into high school. My parents felt that I was too young for high school so I was given a long holiday from school. In September 1921 I started high school in Grimsby a few miles away. After two years I transferred to the collegiates in Hamilton — one year (1923-1924) at the Central Collegiate, followed by two years at the newly completed Delta Collegiate. In my final year at Delta 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 I did manage to rank for three or four of the lesser Ontario scholarships. I accepted an Ontario scholarship that carried with it free tuition for four years at the University of Toronto. This award, together with two scholarships, the first Sir John Gibson Scholarship and the first Carter Scholarship for Wentworth County, gave me sufficient funds for my first year at the University of Toronto.

In September 1926 I enrolled in physics and chemistry (P&C), an honors course in which the first year's curriculum was identical with that for the first year in mathematics and physics (M&P). In 1926 only two students 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, I had to give a lecture course in elementary calculus to first-year medical students, act as part-time assistant to Professor J.L. Synge (pronounced "Sing"), and complete four graduate courses for the MA degree.

Synge was a graduate of Trinity College, Dublin, and had been on staff at Toronto for at least two years (1924-1926). 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, FRS, in editing Hamilton's papers in geometrical optics (*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. The return of Synge to Toronto meant that a very considerable part of my work was much like work at Trinity College, Dublin.

The four graduate courses that I selected were Hamilton's Method in Geometrical Optics given by Synge, the Theory of Numbers, Mathematical Statistics, and Advanced Theory of Functions of a Complex Variable. At the year's end, Synge asked me if I would accept a fellowship in applied mathematics for the following year (1931-1932). I did and for the next few years I worked under the direction of Synge as a student, as a PhD candidate, and finally as a colleague.

My 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 first met at the 1929 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 whom 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. In early 1931 I met her for the second time 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 and 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.

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 University of Toronto 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 her job at 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.

On our return to Toronto in mid-August Jean went back to work and I returned to 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 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 — hoping 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 honors science students, complete two rather difficult graduate courses as part of my own work for a PhD, and find time for tutoring. Fortunately, 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 finished my thesis for a PhD. In the years from 1938 to 1941, Synge and I worked together writing the textbook *Principles of Mechanics*, published by McGraw-Hill in 1942. Two later editions appeared, one in 1949 and the other in 1959. [Editor's note: This was known to some of Griffith's students as "That celebrated text — Synge and Griffith," with no sarcasm intended at all.]

In the spring of 1941, I was given a wartime leave of absence from the University of Toronto, and we went 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 technical 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. I came to like the open prairie very much. We were located near the southern border of the Badlands, parts of which 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 went 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 have to 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 Synge had left Toronto to take a senior position at Ohio State University, and 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 Samuel Beatty. Thus my official title became assistant professor of mathematics, not applied mathematics.

Shortly after my return, Beatty told me 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? I was pleased to do so and joined the Toronto computer committee. The original members of this committee were Dean Tupper from engineering as chairman, Professor W.J. Webber (mathematics), Professor 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 centers of interest that we might visit. I acted as secretary-treasurer of the committee and, in that capacity, wrote to many centers 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 1946 our committee (except for Tupper) set out on a tour of the eastern United States and visited the Naval Research Laboratories in Annapolis, the Pentagon near 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, IBM 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 University and MIT and then started home, but did stop in Burlington, Vermont, for talks with 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 Douglas Hartree. We were impressed by the speed of ENIAC, but it was a specialized machine, designed for ballistic calculations and destined for Aberdeen Proving Grounds. For this purpose its limited storage capacity and lengthy setup time were of minor importance.
2. The visit to Princeton, where we met John von Neumann. His views on the future for electronic computers were most interesting and inspiring.
3. The visit to the Bell Labs. We saw two large relay computers, each built to a design due, in large part, to 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 backup storage, but we had no idea of the importance they would have in the not-too-distant future.
5. The visit to Harvard University. We met 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 with the ENIAC. The Mark I was largely the work of Howard 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 five-digit accuracy could sometimes be achieved. It was used, for the most part, in solving differential equations numerically.

Throughout the remainder of 1945 and the winter of 1946, we met frequently to exchange information about computer activities in the United States and to agree on centers of interest that we might visit.

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. While 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 Department of Actuarial Science, I had already planned a numerical laboratory course in calculations of a statistical or actuarial nature. For calculating equipment we could share three or four very old manually operated calculators that we had resurrected, an old Monroe calculator, and two electrically operated mechanical calculators that we hoped to purchase. Perhaps this course would spark an interest in numerical computation for a few bright students. (Much later, in the late 1950s or early 1960s, one of these first students, Wes "Curly" Graham, went to Waterloo University, where he played an important part in starting a Computing Centre for which he served as director for a considerable time.)

I returned to the university long before the fall term opened and discussed some of this with V.G. Smith. He was 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 numerical laboratory course, I undertook or became involved in two important projects. The first was the establishment of a small computer group using IBM punched-card equipment. The second was my involvement in preliminary plans for a Computation Centre with an electronic computer.

Punched-card project. Late in 1946 I met with sales representatives of IBM (Canada), who told me what equipment I would need for a small computation group. They recommended a calculating punch (the 602 or 602A) with its ancillary equipment: a keypunch, a sorter, a tabulator, and perhaps a collator. To the rental cost of this equipment for one year, I 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 National Research Council (NRC) of Canada for a grant of $7,000. In this application I pointed out the following:

- If approved, this grant would provide financial support for a small computation group using IBM equipment.
- The University of Toronto could provide working space and staff time to help with the work of this group.
- This computation group could be regarded as a first step toward the formation of a Computation Centre equipped with an electronic computer.

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

To operate the IBM equipment, I hired Perham Stanley and Bluma Sachs as an assistant to Stanley. Sachs had been recommended by Professor Norrie Sheppard of the Department of Actuarial Science, 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 Sachs any definite task other than learning to use the IBM equipment and to assist Stanley.

In June or early July 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 took a chance and hired him: It was to prove one of the best decisions 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 project at Chalk River, Gellman and Stanley worked together on the numerical evaluation of internal conversion coefficients for gamma radiation from the K and L1 shells corresponding to various atomic numbers. In this work hypergeometric functions and interpolation methods were both involved, 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] I am mentioned in the first and included as coauthor of the second, although 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] formulas for a number of cases.

In late 1948 or early in 1949 Stanley went to England to pursue postgraduate study and do some computer work. Gellman continued to work at Toronto: For the most part his work was with Professor E.C. Bullard, who had come to Toronto in the autumn of 1948. Until early 1952, Gellman's work in computation was with IBM equipment; thereafter he worked with the electronic computer, which was installed in April 1952.

During the time that Gellman worked with Bullard, at least two papers were published[4,5] in which he is named as one of the coauthors. 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 as 1953 or 1954, Gellman left the Computation Centre to establish a small but very successful computing firm, 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 the punched-card project 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 was made possible through the support of the Geophysical Research Division of the Air Force Cambridge Research Center. I am the coauthor of one of these reports; Professor Jack Jacobs and Sachs are named as coauthors of both. I believe this work was carried out long before the dates shown on the reports. (Jack Jacobs had come from England in the autumn of 1948 to replace 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. He transferred from mathematics to geophysics, went west to British Colombia, and later went to the University of Alberta in Edmonton. After the death of his wife, he returned to England and an important position in geophysics.)

Apart from part-time work with Stanley, Gellman, and Sachs, I did work under the punched-card project 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 nonsingular $n \times n$ symmetric matrix $\mathbf{A}$. Using our IBM equipment, I succeeded in solving the equations and inverting the matrix $\mathbf{A}$ for $n = 14$. This problem arose in connection with some actual data for which I was using linear regression analysis.

Electronic computer project. Following our tour of the eastern United States in June 1946, 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 only in the planning stages or whether some construction had already begun. At the meetings of the Computer Committee in the winter of 1947, Smith gave us much interesting information about the work at Illinois and their plans to build an electronic computer to be called the ILLIAC. Smith felt that several of their plans could be of use to us if we were to start building our own electronic computer. [Editor's note: A.L. Samuel's autobiography makes it clear that the ILLIAC was, at this time, only in the planning stage. See E.A. Weiss, "Eloge: Arthur Lee Samuel (1901-1990)," *Annals*, Vol. 14, No. 3, 1992, pp. 55-69.]

It was clear to the other members of our committee that Smith was now anxious to start building an electronic computer. How long would this project take and how much would it cost? Could Smith give approximate answers? If not, could he learn from Illinois their answers to these same questions applied to the ILLIAC project? As secretary of the committee, I drafted an exploratory letter to the NRC telling them of our hopes to build an electronic computer and asking advice as to how we might obtain financial support. In my first draft I left cost and time estimates blank, pending the receipt of information that Smith 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) the president of the university, Sidney Smith, invited me to visit him in his office and tell him of our plans to build an electronic computer. When we met, Sidney Smith was most friendly and asked if I would give him a short résumé of the work of our Computer Committee. This I did, concluding 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 project we would need to be assured of financial support. Sidney 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 give him only a very rough estimate — perhaps a quarter of a million dollars or possibly somewhat more. I went on to say that V.G. Smith was endeavoring to learn the expected costs for ILLIAC and that might serve as a basis for a more reliable estimate of our own expenses. Sidney 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, and so on; and their probable importance for business and industry.

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.

The university president thanked me, saying that he would take the matter under consideration and perhaps we would need to meet again. 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 he might be of considerable aid.

Later that summer a letter arrived from the Defence Research Board (DRB) addressed, I believe, to the chairman of the University of Toronto Computer Committee. In it the chairman of the committee and I 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 we went to Ottawa. In this meeting 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 him a fairly complete account of computer activity in the United States, pointing out that practically all the work on electronic computers was at the universities. Regarding computer activity in Britain or elsewhere, we had no knowledge. However, the fact that 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 Sidney 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, and so on. In this atmosphere one could expect that new or unusual uses for a computer would be suggested.

Solandt then asked me, if financial support was forthcoming, when could we start work? I said immediately! V.G. Smith had already done considerable planning, could employ the services of recent graduates, and could direct their work with the assistance of 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.

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 University of Toronto as support for the building of an electronic computer and the establishment of the computing center. Several factors may have contributed to our success in obtaining this financial support:

1. Sidney Smith's political connections could have had some effect, either directly or indirectly.
2. Solandt was a graduate in medicine from the University of Toronto and would take a favorable 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 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 my subsequent talk with Solandt may have been of some importance.

Work on the computer was begun in late 1947 and continued until December 1950, when the decision to purchase a Ferranti computer was made. In those three years V.G. Smith took a very active part, as did Professor Colin Barnes. In September 1948 Professor Bullard, who had come to Toronto as head of the Department of Physics, acted as chairman of our Computer Committee, found space in the Physics Building where all work 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 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 in the third year of the M&P program and had been impressed by his ability. Following his appointment, Kelly acted as full-time director of the work on the electronic computer project, and from early 1951 on as director of the Computation Centre at the University of Toronto, until his retirement some 40 years later.

The first employees in the project 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 on the project, 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 honors in his studies even though he was seldom able to attend lectures.

In the years between December 1947 and December 1950, I had very little to do with computing other than my work on the punched-card project, which I have already described. As for work on the electronic computer project, 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 the electronic computer project. 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 Ratz are working on Bats," and the like, were common. Joe did not appreciate this good-natured humor; 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 Williams). That lecture was one of the most interesting that I can remember. In a very simple and clear manner 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 Williams did not give their technical codes, but used plain English words such as read, store, bring, add, and so on. For each of these instructions he explained, in a simple way, the operation that the robot would perform.

Williams 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 the University of Toronto and later as a computer consultant.

In December 1950 representatives of DRB, of 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 the electronic computer project? 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 our project, and shelve 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, and 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 the project, 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 1952, the Ferranti computer, to be named Ferut, was delivered in Toronto and set up in the Physics Building. With it 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 its use.

While Ferut was being installed, tested, and made ready for operation, the Ferranti programmers were training members of our computing group and other interested staff and 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 were punching paper tapes with instructions and numerical data; others were 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: 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 of unknowns was no longer limited to 14; I attempted to write a program that could deal with any value up to 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. Because I had not been able to attend the training lessons, I was not aware of a few special codes that were necessary. 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 the original electronic computer project 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 on 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 farther north on Bay Street, hired a full-time secretary, rented an IBM 650, and did not look back. In 1967 KCS merged with a large management consulting firm.

There was enough left of the original fund to cover the cost of a Ferranti computer. Since we could not set a firm date for the completion of the project, the decision was clear: We would purchase a Ferranti computer.

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 Computation 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 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 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 fewer 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.

The last two activities 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. Professor 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, and so on. 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 and several independent variables.

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 written earlier to invert a symmetric matrix.

Joe Kates had been given a regression problem by one of his clients and thought that problem was suitable to test Program II. He gave 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 two 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 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 Beatrice Worsley, always known to members of our computing group as "Trixie." She had graduated with a BA degree in honors mathematics at Toronto during World War II. In late 1945 or early 1946, she had gone to England for postgraduate 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, probably early in 1950, and returned to Toronto before finishing her thesis.

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 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 Trixie completed her thesis and 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 Computation Centre until at least 1959 and probably for many years thereafter. [Editor's note: I believe this to be the first PhD in which the thesis actually involved modern computers, but not the first awarded by a Department of Computer Science.]

In 1952 Trixie worked with Pat Hume in writing a program for Ferut that was known as Transcode. The program enabled Ferut to accept simple mnemonic instructions and convert them into the usual Ferut instructions. It contained a number of subroutines for the calculation of some transcendental functions such as trigonometrical functions and probably e^x, log x, and so on. [Editor's note: See the article in this issue by J.N.P. Hume on software for the Ferut.]

The mnemonic instructions provided by Transcode were very simple and proved useful in the training of 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 a sample program, pointing out the need for each instruction and describing 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 Computation Centre and others with experience in programming. They 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 1952-1953, but it might have been the following year. It was repeated for perhaps two or three succeeding years.

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 it 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 esophageal 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 was 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 to act as supervisor for four PhD candidates. I also was able to spend some time with individual programmers in the Computation Centre, suggesting a numerical method that might be useful in a particular problem.

The four PhD candidates with whom I worked were Kat Okashimo, a young Japanese-Canadian student; Keith Smillie; W. Kahan (known to us as Vel); and Stuart Baxter. Okashimo 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 Zoology at the University of Toronto.

In dealing with the data he had been given, Okashimo 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. After Okashimo 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 1952. [Editor's note: Griffith originally had the date as 1956, but Keith Smillie informs us that it was 1952.] After receiving his degree, Smillie continued his work in statistics and computing, and in 1963 he joined the Department of Computing Science at the University of Alberta. He remained there for 30 years and retired in 1992. 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. Afterward, 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 Computation Centre about 1954, and after completing his MA thesis in 1955, he 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 Computation Centre was not, as yet, a teaching department of the university and could neither sponsor nor supervise graduate students. When Kahan began his work with me, he had already identified many problems that 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 solving very large systems of simultaneous linear equations. Kahan then set to work establishing new methods and improving older methods that could produce solutions for such large systems with reasonable accuracy.

Transcode's mnemonic instructions were very simple and useful in training. Pat Hume and Kelly Gotlieb initiated evening courses of about 20 sessions to train those wishing to learn programming.

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 Kahan, 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 made 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 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 on 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 has 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 hyperbolic partial differential equations. His work on this problem consisted, for the most part, of skillfully writing quite difficult computer programs. He received his PhD in 1958. For postdoctoral work he continued, for a time, working at the Computation Centre, but he later joined the staff at NRC. In the mid-1960s, NRC acquired its own electronic computer and established a computing center for which Baxter was appointed as director. Except for a brief period in which he helped to establish a computing center at Queen's University, I believe Baxter remained with NRC until his retirement in 1992. Stuart Baxter has done a great deal of useful work in the computing field.

Leaving the University of Toronto. My work at the University of Toronto was now nearing its end. The Computation Centre, under Kelly's able direction, was quite strong and needed little help from me — in fact, in that center 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 the University of Toronto. In fact, I spent only enough time at the university 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 the University of Toronto, 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 the university. In early June I decided to accept Joe's offer and, with regret, sent in my resignation.

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 color space.
2. My work with a colleague, John Lion, in the development of computer programs for the preparation of school timetables.
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 four from Germany, three from Hungary, two from Belgium, two from Holland, and one 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.

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: Some of that 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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