Michael R. Williams
Dept. of Computer Science
University of Calgary

Mike: Here is the Autobiog. which I promised you.

Andrew D. Booth, an autobiographical sketch.

Early Days.

My father was descended from a long line of engineers and shipbuilders. He used to tell of his own young days when he and another youngster thought that they would try their hand at smoothing the deck of a liner which was under construction using an adze which they had seen in the hands of a skilled workman. The result was a disaster and resulted in my father being sent to sea for a period. Later, after graduating at Edinburgh University, he was again at sea during the first world war, this time as Commander of a `Q' boat; a sort of decoy which lured German submarines to surface and then unmasked its own armament and (hopefully) sunk the enemy. As a baby I was regaled with exciting stories of the loss of rudders and propellers and what to do about it.

My mother's family were quacks (i.e. medical doctors!) although mother herself was more interested in music and was an accomplished pianist and a soprano of some note. During the same war she was engaged in nursing.

I was born on 11 February 1918. My earliest recollection is of a visit to the theatre on armistice night (November 11th. 1918) where the brass band, the red plush curtains and the bright brass rail are still vivid memories. Many people have cast doubt on this early memory but I authenticated it with my parents who confirmed the accuracy of my description. Another early memory, about 2 years of age this time, was seeing my father mend a fuse which had blown and, next day when he was at work, performing the same service for my mother. Our `nannie', Rosie, was horrified and I well remember her screams,perhaps justified by the fact that British mains ran at 240 volts and were quite lethal.

In pre-kindergarten days I remember helping my father with the assembly of a mains charger for radio batteries which he had invented. Semi-conductor rectifiers had not yet reached England and his device used an assemblage of Nickel-Copper thermoelectric junctions which were heated over the gas stove grill. Surprisingly this device sold quite well! Shortly afterwards father invented what I believe to be the first automatic ignition advance for motor cars. I had no part in this but was fascinated to see how engineering drawings were made. Another enterprise, in which I played the part of lab assistant was in the production of `Anti-mist', an idea which father had for preventing the large plate glass mirrors in restaurants steaming up.

Like most children of professional families I was, in due course, sent to prep-school. Here I learned Latin, Greek and French but almost no mathematics. I remember that when I sat for Public school entrance, I got 6\% in maths. My father was livid and took my mathematical education in hand to such effect that when I resat the exam 3 months later I got 100\% in this subject. Father continued his instruction and, by the age of 10 I was quite at home with differential and integral calculus.

School.

My Public school days were not particularly happy ones, largely because, even then I suffered from `foot in mouth' disease. In particular I had no respect for the Gods of the school, the first fifteen Rugby football team, and this led to numerous excursions to the prefect's room for a flogging. It may interest the modern generation of `do-gooders' to know that I have never had any ill feeling towards my tormentors, have had no mental problems and have been happily married for nearly 40 years. So much for psychologists!

One happy thing at school was physics and another mathematics. In the event I was chosen to be the school entry for various Oxford and Cambridge scholarship examinations and had instruction from some wonderful Masters, H.C.Oliver, S.L.Baxter and W.L.Edge in particular. Another influence was W.L.Rawnsley, a Cambridge physicist.

University

In due course I went to Cambridge, as a Scholar, but found life there uncongenial. I was able to attend lectures by Eddington, Dirac and Rutherford to the detriment of my mathematical studies. After an unpleasant interview with my tutor who seemed to think that I should be more interested in `pure' mathematics, I decided to leave, much to the disgust of my parents. I entered for the next available University of London External Degree examination and was fortunate enough to get a `first' which placated my father to the extent that he let me do a graduate apprenticeship in his aero-engine factory in Coventry. As another early influence I worked for some months in the Actuarial department of the Sun Life of Canada, this gave me a taste for numerical mathematics and an appreciation of its difficu {original transmission lost something here} up a department for X-ray inspection of components, design of search-lights and designing motor car engines. These were only stop gaps, my mind was set on returning to a University and, in due course, I was lucky enough to obtain a graduate scholarship at the University of Birmingham to research in X-ray crystallography in which subject I eventually obtained a Ph.D. The team with which I was associated was heavily involved in computational work which, like Babbage at an earlier date,`I found no fit occupation for a gentleman'. I therefore determined that, if I could get an academic job, I would attempt to use my engineering knowledge to produce a computer to do this kind of work. In fact, during my graduate student days I designed three small analog computers[1,2] which proved to be of considerable service to the crystallographers of that era.

My scholarship at Birmingham was donated by the British Rubber Producers' Association. It's Director, Mr. John Wilson, C.B.E., became a life-long friend and supported my later computing activities to the extent of funding the production of two of my machines. I spent a short time as a research physicist at the B.R.P.R.A. labs at Welwyn Garden City, near to London. There I started on the design of the machine later to be called the ARC (Automatic Relay Computer). The first model was to use paper tape input and to be fairly special purpose, in fact a Fourier synthesiser. One innovative feature was the incorporation of a one-many function table which used selenium diodes. This was before I had even heard of von Neumann, or of the electronic work going on in the U.S.A. I was also involved in the design of a mechanical device for the same purpose[3] but, like Babbage, abandoned this in favour of the digital scheme.

In 1946 I was appointed to my first University post, at Birkbeck College London. There I taught 3rd. (4th. in North America) year theoretical Physics and spent my research time at the Davy-Faraday Laboratory of the Royal Institution. My `boss' was Prof. Desmond Bernal, a distinguished but controversial physicist who really should have received the Nobel prize for the helix work. He was a splendid person for a young man to work with, always ready for discussion and advice but never interfering. He was as interested in computing as I was and, having contacts in the U.S.A. soon heard of the intense electronic computer activity there. As a result he arranged for me to visit the U.S.A. in 1946 under the joint sponsorship of the Rockefeller and Nuffield Foundations.

America and after.

After speaking at a conference at Lake George, I proceeded to New York for conversations with Warren Weaver, then Natural Sciences Director of the Rockefeller Foundation. This resulted in introductions to most of the known computer research establishments and, with the help of the late Prof. I.Fankuchen `Fan' to his friends, to a lecture tour. The latter was an unique experience, it involved talking to Ladies Dining Clubs, something unknown in the U.K. They seemed more interested in seeing a live Britisher and hearing his accent than in the science which I attempted to instill!

On the serious side, I visited M.I.T. to see and talk to Bush and Caldwell of differential analyzer fame, project Whirlwind and Jay Forrester, the Harvard Computation Lab. where I met Howard Aitken, Bell Labs in New Jersey, Moore School in Philadelphia with Morris Rubinoff and von Neumann at Princeton. I also had a visit to California where I lectured to Linus Pauling and his colleague Corey, I still find it hard to understand that there is any major difference between the lock-and-key theory which they then had and the Watson, Crick, Kendrew (but really Rosalind Franklin) helix.

After the visits I returned to New York for discussions with Warren Weaver who suggested that I choose some group and come over on a visit on 1947. The choice which I had no difficulty in making was Princeton. The other contender was Aitken and Harvard but my feeling was that the thinking of his group was not very profound, in particular with respect to conditional transfer facilities.

England again.

The period between returning from the U.S.A. and taking up my Rockefeller Fellowship at the Institute for Advanced Studies in March 1947 was one of intense activity. First I had to complete a book[4] and second I had to finish as much as possible of the B.R.P.R.A. machine. In the latter activity I was greatly helped by two young lady assistants, Miss Kathleen Britten and Miss Zenia Sweeting. Between them they were responsible for most of the co {again something lost here} on work both at this stage and with the later machine. John Wilson decided to send Miss Britten to the Institute to assist in operations and, most generously paid both of our transatlantic fares, 1st. class on the Queen Elizabeth.

Princeton.

Despite a contrary report[5], the computer lab at the Institute was in operation by the time we arrived although little progress seemed to have been made with the actual construction of the machine. I soon realized that the electronic technology to be used would be unavailable in England and, after reading the Burks-Goldstine-von Neumann reports[6,7], determined that the central problem was that of storage. For this reason I designed a von Neumann type, parallel, machine (ARC again) using Siemen's high-speed relays which were available in Europe. These devices had a switching time of less than 1 milli-second and, by devising an anticipatory carry mechanism, I was able to produce a device which would add two n bit numbers in 1 milli-second. The design of the whole of the relay part of the machine was completed in about 2 months, the drawings were made by Miss Britten after which we wrote up the work in two reports[8,9].

I was astonished when I looked at our first report[8] in preparation for writing this note that I had suggested the principle of the hologram as a means of data storage ([8] p.6. section 1.21).I even pointed out how optical interrogation would lead to an associative memory. This had slipped my memory for 40 years!

Meanwhile I devoted my attention to the problem of producing a reliable storage device. The Princeton group were proposing to use the RCA `SELECTRON'. This did not appeal to me for two reasons, first I did not feel that it would work and second I knew that we could not afford it. In the event I was proved correct, from a design objective of 4096 bits/SELECTRON the final device stored only 16 bits (or so I was told). Many ideas were thought of and discarded, delay lines because of the NIH syndrome, capacitor storage because `discrete element stores were impracticable' (how wrong we were, see modern RAM) and secondary emission, CRT storage because it seemed to me hopelessly unreliable (how right I was this time). In the event I decided that magnetism could provide the only practical solution, I devised a matrix device known to the boys in the lab as `the bed of nails' and abandoned it because the only really effective way of using this type of device was to use toroidal structures which were unavailable. Then I thought of magnetic recording, a well-known audio technique at that time. It transpired that paper coated with appropriate oxide was available in the Brush Mail-a-Voice recorder. I aquired a number of the 10 inch discs and a player to conduct tests. The latter were satisfactory so this matter was settled to await construction in England.

My recollections of Princeton are entirely pleasant. First a ripening friendship with Miss Britten, later to become Dr. Kathleen Booth, second contact with people like Johnny von Neumann and Hermann Weyl and last, but not least the happy gang of young engineers involved with the project. Among these I must mention Dicky Schneider, Willis Ware, Jim Pomerene and Ralph Slutz. Ralph was a wit and an adept at limerick construction, a favourite one was:

> There was a computer named Booth,
> Who said by-gad and forsooth,

To shorten the delay
of the highest speed relay,
Apply a spot of Vermouth.

I never realized that Johnny was a lightening calculator. He was certainly quick on the uptake and could make approximations in his head but not in a way which seemed particularly remarkable. Also he sometimes gave insufficient thought before answering a question, for example I once asked him if his non-restoring method of binary division had a parallel for multiplication. He replied that no such algorithm was possible which deterred me from seeking one for some time. He was also incorrect over a question in Fourier series which later resulted in a paper which I had published in the Proceedings of the Royal Society[10]. These are minor points as our visit was an unqualified success

After leaving Princeton in August 1947 I travelled to New York to meet with Warren Weaver with the object of getting Rockefeller support for my project. Weaver reminded me that mere calculation would not attract money as the Americans were ahead in this field. During the discussion I raised the questions of Machine Translation and Medical Diagnosis. It seems that Weaver already had thoughts on the translation business and had discussed it with Norbert Weiner. His thought was that translation was simply a form of code and that, since codes could be broken, we should approach the problem that way. In the event this proved unworkable but the MT application provided us with major support.

Another interesting experience was with IBM. Weaver gave me an introduction to T.J.Watson with the idea that IBM might provide funding. Far from it! T.J. informed me that there was no future in this new-fangled electronic business and that, if I came back in 5 years, the relay would still be paramount.

Return to England

After returning I immediately turned my attention to the construction of a memory whist Miss Britten and Miss Sweeting set about building the relay portion of the ARC. My first thought was the floppy disc! I would spin the 10 inch Mail-a-Voice disc at about 3000 R.P.M. at which speed it would stay flat, and then move a rigidly mounted read-write head close to the surface. The theory was that the Bernoulli effect would draw the disc to a fixed distance from the head and maintain a very small air gap. Unhappily this did not occur, the attraction was perfect but the distortion of the disc surface resulted in unstable `flapping' which led to eventual disintegration. Thus, although I suppose I really invented the floppy disc, it was a real flop!

My second attempt was more successful. I had a 2 inch diameter brass cylinder plated with .0005 inch of Nickel. This metal is robust and is magnetically remanent. It stored data permanently and well. The original device is now on display in the Science Museum in London as is the larger drum actually used on our first machines. Our original, Nickel plated drum was a parallel device with 21 channels plus a clock channel. It stored 256 words of 21 bits and fed the ARC which was demonstrated to members of the Board of Directors of B.R.P.R.A. on May 12th. 1948. Warren Weaver and his colleague Gerard Pomerat also saw the machine in operation on May 25th. of the same year.

T.Kilburn, from the F.C.Williams group at Manchester, visited the lab on 2nd. November 1948 and took away a sample of our read-write heads which we later found to have been copied by that group and by the Ferranti organization.
With the ARC in operation I set about the next phase which was to design an all-electronic control and arithmetic unit. The test bed was SEC, or Simple Electronic Computer, it formed the Master's thesis for Norbert Kitz. This led to the final machine of the series APE(X)C or All Purpose Electronic (Sponsors name) Computer of which we built several to make money to support students. (For a detailed list see[7])

It may be of interest to mention some of the achievements of my group in the period 1948-1962. First there was the Binary Multiplication procedure (usually called Booth's algorithm)[12]. Then the binary partitioning technique applied to the solution of equations and to dictionary search[13,14]. Finally the idea of binary trees[15]. Major activity was devoted to Mechanical Translation with the support of the Nuffield Foundation and thei led to numerous publications and a book[16].

By this time I was Director of the Computer Project and a senior faculty member. My father, who had retired by then, set up a small factory to produce our magnetic drums. This factory (Wharf Engineering Co) probably made more drums than any other in the world, largely for export to the U.S.A. It continued in operation until I left England in 1962.

To support my laboratory I made various arrangements with Industry. In particular we supplied details to the British Tabulating Machine Company in return for cash but with a non-publication agreement. This was an undoubted mistake in the light of the archaic technology deployed at that time, but the Company offered the University funds to endow a Chair in Computer Engineering for me in 1962 and I acknowledge with gratitude the efforts of Cyril Holland Martin their then Technical Director.

Unfortunately, because of the preoccupation of the then Master of Birkbeck College, Sir John Lockwood, with Colonial education, and the petty mindedness of F.C.Williams, T.Kilburn and Herman Bondi (Bigmouth Bondi to most of his aquaintences) the proposal fell through. As a result I decided to depart from the hive of Socialist mediocrity which England had become and go elsewhere. Within a few weeks I had offers of Chairs in the U.S.A., New Zealand and Canada. As the latter had the prospect of advancement to the rank of Dean in one year I accepted it but, at the same time accepted a `professorship at large' of Autonetics at the then Western Reserve University. Autonetics, incidentally, is a neologism invented by Jack Millis then Chancellor of Western Reserve, from Greek roots meaning Self Control, otherwise `doing what you like'. A happy thought which I have perpetuated in our present Company name.

Both Canada and Case Western Reserve proved good choices. At Saskatchewan I was able to raise a moribund College of Engineering into the 20th. century. In fact, by the time I left in 1972 it had the 3rd. largest Graduate School of Engineering in Canada. At Saskatchewan, with the help of Ken Cameron, a bright Grad. Student we constructed the M3 computer[17] in less than one year. It was sponsored by the National Research Council of Canada and by the Defence Research Board. The machine worked well for a decade but is now honorably retired.

Into the sere and yellow

My tale is nearly told. In 1972 I was invited to the Presidency of Lakehead University in Ontario. Again the objective was to start graduate programs. This did not prove easy as there was an economy drive on the part of the Provincial Government during my term of office. However some things were achieved. The particular program of which I am most proud was the `bright kids' program. In this any youngster who had the ability could take courses, free, at Lakehead and if a pass was obtained credit would be banked against the time when the student should enter the University. We had a number of these young people, amongst which were my own children both of whom started full time studies at 12 and graduated at 16 with 1st. class honours. I am please to say that they graduated after I had retired so that influence cannot be implied against their record. I feel very strongly that the present psychological rubbish about peer groups which prevails in the schools prevents our best young people from achieving their full potential. It is simply the Cancer of Socialism which seeks to make all men equal - of course to the lowest!

In conclusion it may be of interest and even amusement to remark that I was probably the only University President on record to have his political affiliation recorded in`Who's Who' as Anarchist (Philosophical of course).

REFERENCES

1. Booth, A.D. {\it A method of calculating reciprocal spacings for X-ray reflections from a monoclinic crystal.} J.Sci.Instr. {\bf 22} (1945) 74.

2. Booth, A.D. {\it Two calculating machines for X-ray crystal structure analysis.} J.Appl.Phys. {\bf 18} (1947) 837.

3. Booth, A.D. {\it Fourier technique in X-ray organic structure analysis.} Cambridge University Press (1948) 76-81.

4. See item 3.

5. Regis, E. {\it Who got Einstein's office?} Penguin Books, London (1987) 111.

6. Burks, A.W., Goldstine, H.H. and von Neumann, J. {\it Preliminary discussion of the logical design of an electronic computing instrument.} Institute for Advanced Study Princeton (1947).

7. Goldstine, H.H. and von Neumann, J. {\it Planning and coding of problems for an electronic computing instrument.} Parts I-III. Institute for Advanced Study Princeton (1947-48).

8. Booth, A.D. and Britten, K.H.V. {\it General considerations in the design of an all-purpose electronic digital computer.} Institute for Advanced Study Princeton (1947).

9. Booth, A.D. and Britten, K.H.V. {\it Coding for A.R.C.} Institute for Advanced Study Princeton (1947).

10. Booth, A.D. and Britten, K.H.V. {\it The accuracy of atomic coordinates derived from Fourier series in X-ray crystallography, Part V.} Proc.Roy.Soc. {\bf A 193} (1948) 305-310.

11. Booth, A.D. and Booth, K.H.V. {\it Automatic Digital Calculators, 3rd Ed.} Butterworths (Academic Press) London, (1965) 22.

12. Booth, A.D. {\it A signed binary multiplication technique.} Q.J.Mech. and Appl.Math. {\bf iv} (2) (1951) 236-240.

13. Booth, A.D. {\it A computer programme for finding roots.} Comp. and Auto. {\bf 5} (1956) 20.

14. Booth, A.D. {\it Use of a computing machine as a mechanical dictionary.} Nature {\bf 176} (1955) 565.

15. Booth, A.D. and Colin, A.J.T. {\it On the efficiency of a new method of dictionary construction.} Information and Control {\bf 3} (4) (1960) 327-334.

16. Booth, A.D., Brandwood, L. and Cleave, J.P. {\it Mechanical resolution of linguistic problems.} Butterworths (Academic Press) London, 1956.

17. Booth, A.D. and Cameron, K. {\it A small transistorized digital computer.} Electronic Eng. {\bf 37} (1965) 368-374.