Interview with Andrew Donald Booth (B:) done on Feb 6, 1993 by
Michael R. Williams (W:) at his home in Sooke B.C. Canada

After some preliminaries the interview began...

W: This is Februrary 6, 1993 and I am talking to Andrew Donald Booth in Sooke B.C.
As I said before your early years in Britain are fairly well documented and well known but I don't think I know why you came to Canada and what prompted it?

B: Two things. The first was a dissatisfaction with socialism in Britain because I don't happen to be a socialist. More importanly I had pursuaded International Computers Ltd, as it then was, to endow a chair at the University of London. Unlike Canada where any university with self respect would jump on a load of money, British Universities don't quite work that way - what they do is to ask all sorts of other universities whether or not they should be accepting a chair in numerical automation (now called computer science). They asked a number of other people around and the gang at Manchester (I call them a gang deliberately because that is what they were - specifically F. C. Williams and Thomas Kilburn) didn't like this idea at all. They were backed up by Max Newman who was a professor of mathematics and was quite well known. Max Newman, it is now a matter of public record, was against other groups starting computer operations - they tried to oppose the one at the National Physical Laboratory and they tried to oppose the Cambridge one - they didn't succeed in either of those cases, probably because they had more clout than the University of London had. The result of this was that this particular operation of mine got turned down. I was so disgusted that I said 'to hell with you'. I wrote three letters in the week after this happened, one to New Zealand, one to a friend in the United States, and one to Authur Porter (whom I will no doubt be talking about later). By two returns of post I got two offers of professorships, one in Wellington and one, with more prospects, in Saskatoon in Engineering. The third wasn't a letter at all but the Chancellor of Case Western Reserve University, Jack Millis(SP?) appeared at my door. He flew in and immediately invited me to breakfast the morning after he arrived and said 'I want to take you back with me'. We had actually met at a conference on mechanical documentation for libraries which had taken place a couple of years before.

W: What year was this?

B: This was 1962. I had a certain distrust of American Universities. I had had a lot to do with them. I like a lot of things about America but I dislike a lot of things equally about the place. While I liked Millis and I liked Case Western Reserve University, I had my doubts about chucking up a tenured job at London as Department Head to go to another job which although it had tenure but which I didn't know about. I had a real suspicision that in the United States if you weren't an American citizen, which I wasn't and had no intention of becomming, that you could get tenure at all in any real sense.

W: Refresh my menory. You were in London at Birkbeck College?

B: Yes as head of the Department of Numerical Automation, which would nowadays be called Computer Science. So anyway, the other alternatives, New Zealand wasn't really on, but I had

some New Zealand friends, (incidently at that time I didn't know about the great Babbage collection in New Zealand and if I had that might have been an enticement) I didn't take the New Zealand position seriously.

The Saskatchewan people that got in touch with me, John Spinks the then President, said that the Deanship had just been vacated by Arthur Porter and although they were not prepared to offer the Deanship on the spot, if I were prepared to go there, with no committment on either side for a year to head up the Department of Electrical Engineering and see what I could do with it (because it was in a mess after Arthur and Moody had walked out on it), would I like then to think about being Dean the following year if we all liked one another. I thought this wasn't a bad deal. I am a bit of an entrapanour, so I got in touch with Jack Millis and said 'supposing I go to Saskatchewan, would you then say that I was a terrible man and have nothing to do with me, or would you like to do a deal?' Millis, being a reasonable type of American said sure, let us do a deal. We will appoint you as a distingushed professor at large and you can come down during the summers. He flew me out to visit him and the University. It was quite interesting because he said 'we have to find a name for this chair' - Jack Millis is from the old style of academics, not the narrow minded physisists which is what he was originally but brought up in the classical tradition which is what I was. Over breakfact we thought about names and he eventually said 'from the Greek we will call it the chair in autonetics, from autos? meaning self and iosesis? meaning control - self control means doing what you like doesn't it?' I said 'fine' and that was it. I continued that position quite actively until 1970 when I became President of Lakehead University and I thought I shouldn't have some of these jobs hanging around.

W: So you went down there for summers?

B: As soon as term was finished and I had cleaned up the administrative chores in Saskatoon I just shoved off to Cleveland and spent most of the summer there.

W: Were you lecturing or doing research or what?

B: Doing research, talking to people - what he was interested in, and I happen to think that is the the real name of the game nowadays, is interdiciplinary work. One of the difficulties that Millis had perceved is that many of the sciences had gotten into little boxes and the boxes don't communicate with one another. Where you can do a lot of good is to mix them all up and tell group A what group B is doing and vice-versa.

One of my great interests in those days was bio-medical engineering because the engineers had made enormous strides in things like control and data communications devices of one sort or another - of which the medics knew nothing. Likewise the engineers knew nothing about medicine. As it happens, historically, I started life after wanting to be a mathematician, being persuaded to do some work on medicine. My mother's family were all medics and I did the best part of a medical degree - I didn't do the clinical ward work but I took my batchellors.

W: I didn't know that! You really have a varied background.

B: Most people don't. I am a fast worker and I go through phases. At the age of about 12 I was a chemist. I just about learned everything there was at that time about chemistry (there wan't much) and then my next love was music and I very nearly screwed up my school career by

taking too much interest in music. I used to practice for about 8 hours a day to the exclusin of studies in Latin and other useful subjects.

W: Just for the benefit of the tape, you told me earlier that Youhoudi Menheun was a a classmate of yours with the same music teacher.

B: Yes that was with Karl Flesch who was the premier violin instructor in Europe at that time. So was Edith Handell, but she was younger than I was. I was really not good enough to make my living at the violin so my parents talked me out of it. My other great love at that time was mathematics. I got a mathematics scholarschip to Cambridge. My tutor was G. H. Hardy. I took an instant dislike to Hardy and he took an instant dislike to me. Hardy took a dislike to me because his first question was "what did I know about some arcane bit of cricket history - some test match?" I rather dispised sports, I always thought they were a waste of time. I had no interest in cricket and told Hardy so. At this point I could feel the intelectual temperature going down like a rocket and he didn't like me much after that. It was a shame because he was one of the world's great mathematicians. He was the man who had the honours list abolished in Cambridge, around about 1909. Up to that time the first class was published in order of rank and G.H. Hardy didn't like this. Largely his argument was that a lot of the people who were not senior wrangler became much more famous mathematicians than the man who did. The reason I didn't like him was that my mother had warned me about people like him. According to C. P. Snow Hardy was an active homosexual - I don't have anything against homosexuals doing things in private but it they want to protest about it in public then I think it is rather odd. I can only suggest that this is why I disliked him. The result of this was that I didn't want to do what he wanted me to do and he wouldn't let me do what I wanted to do. My interests have allways been in appplied mathematics, particularly in mathematics applied to engineering and science. Hardy's view, as he published in his book *A Mathematician's Appology* was 'if I die I would like to see engraved only tomb stone that none of my mathematics was usefulf' - that's a paraphase, but it is close.

W: Do you happen to know if it was ever engraved that way on his tomb stone?

B: No it wasn't. Unlike Shanks - he was the man who in the 19th century computed PI to 609 decimal places - he made a mistake around about 508. He did have PI engraved on his tomb stone in Stoke Podges churchyard. In later life when I found about this I wanted to see it but the Germans had a V1 land on the church yard and blew the stone to pieces. I would have loved to have a picture of it.

Hardy and I parted and I went back to my father's aricraft engine factory. I did a graduate course in mechanical engineering (I shouldn't have because I was not then a graduate) which was a two year course - you started with a broom in your hand and finished up in the manager's office. That was how I became a mechanical engineer. In the meanwhile I did what you can do in London, I wrote to the University and siad that I wished to sit the BSc examinaiton in mathematical physics. They agreed and six months later I took the degree without doing any additional studies. I spend a couple of years working at the maechanical engineering course in my father's factory. I then spent some time at a variety of engineering jobs - including the job as director of research making searchlights - it was a good job actually, lot of good physics in that. Then I had a job, which which I was fired, as assit. cheif engineer at Nufield Moters in their

engine factory at Coventry. I am not ashamed of being fired from that place - it is a nice story actually. I was supposed to be working out the thermodynamics of the motor car engine that was going into Morris Motor cars after the Second World War. I had a litle group that had worked very carerfully using all the latest light alloys - I had learned about these in the aircraft business. I thought it was terriffic - we had a light weight engine, lots of power, much superior to everything else. I put the report in to the man (White his name was, can't remember his christian name - he was the Chief Engineer) and White summoned me the next morning, threw the report on the table and said "your fired". I said "what for". He said "this report - garbage". I said "please let me resign, but just tell me why it is garbage". In the report, among other things there was a complete set of drawings for this engine. These included the connecting rods between the camshaft and the pistons - there were nicely stressed - all the stress ananysis was in the thing and we had reduced all the sizes from the conventional ones - the old Morris engine had things about as big as your nuckles - these were all very small. He said "any fool knows that you can't have things as small as that, they will fall to pieces". I pointed to the stress analysis and he said "I don't hold with all that new-fangled stress analysis, all I knw is that if you look in the machinests handbook and it says there that the outer diameter of a ???? bearing must be 1.25 times the internal diameter". So I said "look if you see who did this work it was James Watt about 1790!". "It doesn't make any difference, that is the way we build engines". So that was the end of that job.

Then I got ??? by C.P. Snow. There was a thing called the technical and Scientific Registry in England during the War. Anybody who had technical qualifications had to go and get interviewed. I was interviewed by C.P. Snow and Wardlaw - a distinguished chemist. These are very strange these coincidences - he was also a professor of chemistry at Birkbeck and, of course, I got to know him very well post War. I got sent up to Birmingham to work on the crystal structure of explosives becasue I had some chemical as well as mathematical background. That is how I cam to do crystalography. Of course it was doing crystalography which got me interested in computing. I was running a little group of three people, besides myself, who were working out the structure of a substance called penta???tetronitrate - an explosive - nowadays it is also a very useful heart medicine for angina. Anyway we worked out the structure of this and it took this little group (3 computers and myself) three years! During the course of this project, because of my engineering background, I invented a lot of little computers to help us out. Most of it got published in dure coures. This got me interested in computing. When the War was finished I took a job at Birkbeck College with Desmond Banall(?). Banall(?) encouraged my engineering ideas to improve computational efficiency in crystalography. I spent about 5 happy years working for him - doing exactly what I like which was theoretical crystalography and inventing machine.

Anyway that was how it started. I guess you know all about England so you don't want to talk about it.

W: Well I know all about the machines made a Birkbeck.

B: One thing that I would like to say is there is one Englishman who has never been credited as far as I know in any historical stuff. This is a man called E. Williams-Phillips.

W: What did the E stand for?

B: I don't know. He was an acturary. After we got some publicity about our machines at Birkbeck he came to see me. I guess he was some type of lawyer as well as an acturary because he had room in one of the Inns at Court. He asked if I knew about his binary computer? I didn't. He told me that he published this stuff in (I am speaking from memory so I may be

wrong) in the Journal of the Institute of Acturaries about 1932. He brought in the model he made at that time of a small binary computer. It was made out of bits and pieces but it demonstrated the principles of binary computations. As far as I know this is probably the first thing (apart for Leibniz) - this was the first recent idea that you should use a binary computer.
W: This was essentially an adding machine that would operate in binary?
B: His idea was that it was to be a general purpose machine - or that was his description of it - I am only sorry that he offered it to my lab and I didn't take it in. He is a person that I like to remember - he was a nice old ???. It might be worth somebody digging out the history of that. If you look in old telephone directories from about the 1930s you would probably find his Inns of Court address. He showed me the paper. It should be possible to tract it down.
Anyway, that is why we came to Canada. I had little battle with Williams and ??? - probably why they wre a bit beastly about the whole business. Kilburn, and this is all documented in my diary (I sent Eric Weiss some transcripts just the other day). Kilburn visited my lab where I was developing this magnetic drum technology. We actually gave him some of our heads and things. The next thing we discovered was that he and the Ferranti gang were actually manufacturing these things. I was a little bit narked about this and made some rude noises and eventually he published an acknowledgement that these ideas had come from our group. It didn't really bother me very much on the production side because by the time he had developed anyting there we had got much better recording heads - my father was manufacturing them. So it didn't bother me, but I suspect he didn't much like having his nose rubbed in it. Also I made some scathing remarks about Williams' technology, which I never did think much of (the Williams' tube). I had seen these tubes in the Forrester operation (MIT) in 1946 - they were advanced, they were working and they seemed to work beautifly. Williams' things looked just like a load of junk to me. I had seen the pictures of the Manchester machine and it really was a hay box. I was never the most tactfull person.
(This story seems to confuse the Manchester Baby machine with the MarkI as to size and capabilities)
I am sure you know about the early Williams machine. When the machine was finished, Williams said "we must a demonstration" - you know Turing participated in the design of the first version of the machine. Williams wondered what he could do with this machine. Turing has persuaded him that the only operation that he wanted to do arithmetic was subtraction. Turing, who was a very bright guy, said "obviously you can test to see if Merssine Primes are, or are not, prime. That is, in fact, what they did. Williams was very cross about this because the machine was, at that stage, practically useless. As far as I remember it only had a thousands words of storage. The other interesting thing was that you could only use the machine at 3:00 in the morning because the electric tram, which ran right outside the labs, created such a lot of interfeerence that these tubes picked up every tram flash on the overhead wires.
W: I have certanly taked to people who have used that technology and they said that if you walk into the room and comb your hair ....
B: Oh yes, terrible, terrible technology. I remarked on this, I am afraid, publically - probably not the best thing to do. I never liked Williams anyway - he is dead now so I can't be accoused of slander. E.A. Newman, is he a live do you know?
W: I don't beleave so, but I am not sure.
B: He was at the National Physical Laboratory. I havn't sen him for years. He was, temporally, one of my graduate students. I was on the management committee for the NPL and Newman felt that he needed to get a doctor's degree - fairly obviously he was in a position where it would

have been useful. I used to have a lot of people from industry working at the University of London - you do your research at your place of work and come in periodically for supervision - E. A. Newman did this for a time. Then he fell out of it - I don't know the reason. He would ave done very well - he was a top class person. The story he told me, which is highly discreadable and made be dislike Williams intencely, is this - he apparently worked at the same place as Williams and a man called Alan Blumlein. Blumlein was one of the most inventive people in the British electronics industry. He was one of the really great figures - he has just been sort of dieafied by the Institution of Electrical Engineers in Britain. Alan Blumlein invented a lot of things, including the sort of circuitry which eventually found its way into the Williams' mahcine. According to E. A. Newman (and I say "According to" because I have no personal evidence, but I think Newman was a very honest person) - where you can tie up with established fact so that things match is that Blumlein was very inventive and F.C. Williams was his assistant. Blumlein, very unfortunatley, was killed in an aircraft crash - they were testting some piece of equipment that he had designed and he went up with the other peope and about eleven of the scientists were killed in the plane crash. According to Newman, when it became known that the plane had crashed, they found that some of Blumlein's notebooks had disappeared. Again, according to Newman, some of the papers which then Williams published (and he hadn't been a publisher belfore) contained ideas which he knew Blumlein had, in fact, originated. I can't vouch for the truth of this story, but historians might want to get on to it some time.

W: You mentioned something about you being on the Board of Management fror the Teddignton machine?

B: Oh yes. Sir Charles Darwin was director fo the NPL and he appointed ... I am not sure if I wan't the only person because I was conviennnent in London ... they had the Managmeent Committee which met periodically and had a big fiest, drank wine and did all sorts of other nice things, and we sort of gave Sir Charles light advice on how well the establishment was going. Of course it was going very well - very impressive organization. Sadly, after they got rid of my old friend John Womersely .. Turing, of course was there originally and they had to get rid of him - he thought he was an engineer and he wasn't - he kept trying to impose his ideas on the people who could build them. Eventually Darwin gave him leave, so he just got the Hell out of the place and went to Manchester.. Anyway, then there was Womersly - he was very difficult in many ways. Fortunately for the NPL he was enticed away by International Computers - ICL or BTM as it them was. His first act there was to bring Holland (?) Martin, who was the technical director of ICL to see me and say that they wanted to take our machine over.

W: So this is how you got into the connection with them.

B: Oh yes, John was a very good personal friend. So the deal was that I was not to get any personal money out of it, what I did was to have them support my lab. As I said, eventually they were prepared to endow this chair in the subject. Womersley was the instrument in doing that. Unfortunately poor old Womersley, when he got into a discipline, also felt that he needed a PhD. He got in with a person called McDonald at St. Bartholamew's Hospital. He decided that, for his PhD topic, he was going to investigate the flow of blood in veins. The Vein, as yu know, is an elastic tube and when the heart pulsates the pulse is transmitted through the whole system. Actually I think he was concerned with artries but we used to call it "the bloddy flow in veins" because he made eveybody's life a misery. The net result of this very sadly was that ICL decided that he had to go and they moved him to the United States where he died shortly after of bladder cancer. His daughter stayed with us for a long time after. She was a medical student.

Anyway he left NPL and they got F. M. Colebrook (quite a well known radio engineer) to head up the group. When that happened everything went just as smooth as anyting. Of course the people that did the real work were Wilkinson on the mathematical side and Newman - both top class people. So that was the connection with the NPL.
W: That was one of your earlier aspects that I had no idea about.
B: We used to get all sorts of nice invitations to bean feasts or some nice thing like that. I got to know Charles Darwin very well - an extremely fine person.
W: Did you ever know Bullard who took over from him?
B: Oh (long pause) I am jut trying to think of his name -a noted geophysist. Sir Edward Bullard, yes. Yes I knew him but not as well as I knew others. Have you ever read any of his wife's books? If you want to know about history and everything you should read "Perch in Paradice" by Margaret Bullard. They say that everytime Sir Edward took a job anywhere, Margaret would write a book about it after one year and they would both have to leave. "Perch in Paradice", which is the NPL book, will show you why - it digs up the dirt.
W: I must read about their time in Toronto then too.
B: I don't know the book on that is called. I do know that people whould say "poor Bullard, he is gone to the NPL, I wonder how long he will last until Margaret writes her next book".
W: He had something to do witht he development of the Computer Centre in Toronto.
B: Yes with Gotlieb of course. Not that I knew of that at the time he went to NPL. Have you read "Victorian Childhood" by Gwen Rabberat(?)? She was one of the Darwin's - Charles Darwin's sister. She married an artest called Rabberat(?) - she was also a very capable artist. She wrote this book which is an account of Cambridge and the Cambridge people around the turn of the century - a fascinating book.
W: Let's get back to your coming to Canada. You ended up in Saskatoon, first as head of Electrical Engineering and a year later you took over as Dean of Engineering.
B: And I continued as head of Electrical until 1972 when I went to Lakehead.
W: So you held both jobs.
B: yes.
W: Why don't you tell me a litle bit about the kinds of work, particularly the computer work that you were doing at that time.
B: Well, when we wnt to Saskatoon I took with me - OH, backtrack just a moment - I think I told you my father had a factory where we made magnetic drums among other things. We also made a small number of computers to my APEX design, and I had one of these in the house when we were leaving and we exported it to Canada. I took it woth me largely because i thought I wanted to get the outfit started on computer design and building. There is nothing better than taking one that is working. So I took this over - we still have pictures of this machine.
W: That wasn't a small machine - you didn't pack it in a suitcase and take it over.
B: No but all my machines were very small. Really, I suspose if I wanted to boast about it, I would say that I was the apostle of the microcomputer. I nver believed in big machines. If you remember some of my history - the first of my machines, the relay machine, the 32 bit parallel machine with magnetic storage and electronic circuits - that machine was quite compact.
W: I remember seeing photographs - one rack.
B: Yes just one rack, About 6 feet high and 19 inches wide - mostly relays and a little tray on it with electronic business. When we made machine which were all electronic, and when we got minuature tubes, which you couldn't get in 1947, the whole of my machines were literally 19" wide (the size of one panel) - two panels wide and four packages deep. The big part of the

machine was the transformer which we needed to power the tubes. It was very good actually because it stabalized the whole assembley - it sat on the floor and kept the frame from falling over. Appart from that the machines were very small. When we got to Saskatoon I installed this machine and got it up and running. In fact we used it as a computing organ in the Electrical Engineering Department. We were very luckey because we were the only department in the University at that time that had a computer to itself. As the same time I got te graduate students started on the construction of an all Canadian machine. I wanted to do this for two reasons: the technology in Britain at that time was totally abismal - that was one of the reasons why I left. The sort of things that we could get hold of - if you wanted minuature tubes you had to go over to the States and buy them, for dollars which you couldn't get - which meant you had to have made money in the States, If you wanted tranasistors they were away - they were still using point contact transistors - in fact for our magnetic drums we used electronic head switching using transistors and I got these from Phillips in Holland because you couldn't get them in Britain. they were some special bisyemetrical transistors which were produced for this purpose. As to getting enough to make a whole machine - NO WAY! I tried - it was the obvious thing to go with transistors. It was my belief then, and now, that if we had been stuck with hot tube technology computers would not have deleloped that way they have done. Tubes were so unrealiable. Even our litle machinew which had about 600 diodes (point contact seniconductor diodes) for the coding operations in the machine and something like 200 hot tubes - tiny machne. I had a contract , which is not very well known, with the Ministry of Supply in Britain, to produce a desk electronic calculator - called DEC - there is a report around somewhere in the archives which is called "Design for DEC". The object was to get rid of this transformer business and what we opted for, and this is the reason it never came to anyting, was to use decimal counting tubes - a ting called the decatron. What is not very well known is that the decatron is not very reliable. While we produced the design and a prototype, I didn't regard it as a very satisfactory machine. In any rate, that was all you could get in England. Now, when we came to Canada, the National Research Council and the Defence Research Board were extremely generous. One of my friends was Sir Edward Appleton, he must have said some nice things about me because the National Research Council immediately gave me a whopping great grant, by the then standards, and pursuaded the Defance Research Board to give me another big grant. The object being to produce a computer.

W: Now this would be 1962?

B: Yes, 1962 just as soon as I went there. They were very nice and I have reason to be extremely grateful to Canada.

W: Now at the same time the DRB was trying to produce their own computer.

B: Yea, I wish they had told me about it.

W: They wre keeping that under wraps wee they?

B: Presumabely.

W: I will speak to you more about that later.

B: Anyway, I got this little thing together. Arthur Porter, my predescissor, had left the place in a real shambles. He got a lit of extrememly bright ideas which he had thrown into the air. That simply distracted the whole College because, by and large, a lot of aged who graduated in traditional branches of engineering like concrete bridge work etc. The Electrical Engineers were all power engineers. Anyway I attempted to get a group together. I got, as the principal person in charge of it, a young PhD candidate who was also a jr faculty member, Herb Ratz. Do you know of him?

W: I know of his brother, or cousin, Alf Ratz.
B: Well Herb went to Waterloo. But what has happend to him sence I do not know.
W: His Brother Alf worked on the first UTEC machine that Toronto tried to build.
B: Really? Herb Ratz was one of my graduate students at the time and also a jr member of faculty. He headed up this group for me. We took the design of he APEX (by that time the name had changed to the MAC) machines and converted these into an all transistor design. The result was, of course, that the whole thing fitend into the back of a door of a small cabinet. It really ranked as a micro-computer despite the fact that we didn't have ICs at the time. What we did was to take the magnetic drums that I brought over and use them as the principal store, and also use core storage - one of the students that I had at that time did a thesis on core storage. Another student, Ken Cameron, did the over all design of the machine for a master's thesis. Several other students participated in the peripherals - we had a high speed paper tape reader, and a high speed punch. I had been working on high sped punches over in England and we had a proper analysis made of it and produced a perfectly respectable design at Saskatoon. Well of of this was done in the year 1963.
M: You said the name had been changed from the APEX to the MAC?
B: Well the series of machines is a follows: The fist one was called the ARC (Automatic Relay Computer): Relay arithmetic computer - full arithmetic, multiplication, division etc. on the Princeton model - duplicated the logic of the princeton machine, with a magnetic drum store. The magnetic drum store fed electronic registers in series (because that is the way magnetic drumstores work) which then parallized the data and fed it into the parallel registers. That I did quite deliberately because there was one man and my wife - we were the team - and Kathleen wired just about the whole of these machines up with the aid of another young lady. (name is on the tape but I couldn't mad it out). I perseved that the real difficulty was not in building the counting circuits, they did that in Princeton, but the real difficulty was in storing anything. I certainly didn't believe in the Williams' tube. The mercury delay line didn't partiularly appeal to me for a variety of reasons, not the least of which was that Wilkes was using it and doing very well - so why bother to copy somebody elses's thing. Perhaps more important than anything was that I didn'tbelieve that any store which was luminescent was valid in the long run - I wanted something which was a store and something which you have to regenerte all the time is not a very good store.