Oral History of Arthur Porter

Interviewed by:
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Tim Robinson: So, here we are on the 8th of March, 2008, and I'm talking with Arthur Porter. So Arthur, let's begin at the beginning, when were you born?

Arthur Porter: I was born in Ulverston, in the northwest of England, on 8 December, 1910.

Robinson: Wow, that's amazing.

Porter: Quite a long time ago.

Robinson: So that makes this, by my calculation, your ninety-eighth year right now?

Porter: I'm in my ninety-eighth year.

Robinson: Amazing, absolutely amazing. So tell us a little bit about the family background that you were born into.

Porter: Well, my father at the time was a tool and die maker, and an extremely good one, at Vickers-Armstrong Shipyards in Barrow-in-Furness. And my mother had been a seamstress and was a very skilled seamstress. My grandparents, on the one side, was a painter and decorator, and on the other side was very much involved with the railway, the Furness railway, and indeed was one of the three directors in charge of the permanent way, as they called it, and that meant in charge of the actual track between Carnforth and Whitehaven. And this is interesting in so far as up to the time the railroad opened, which I think would be about 1870, something like that, the Lake District and the northern part of Lancashire were completely isolated and in many ways even the dialect and the language was quite different to the rest of Britain. And it gave rise to, we in that part of Britain, being quite private in our way of doing business, and regarding ourselves as rather superior, very interesting.

Robinson: And the railway spoiled all that <laughs>.

Porter: And the railway then connected us to the rest of the country.

Robinson: So were you the first child in the family?

Porter: I was, and the first in-- my father was the eldest son, his father was the eldest son, and subsequently I had two brothers, both of whom incidentally are deceased.

Robinson: So as a young boy the Great War, the First World War took place.

Porter: That's right.

Robinson: Did that actually have an impact on you at that age?

Porter: Well, I remember the beginning because my parents were on holiday in the Isle of Man and my father took off feeling that he should get back to his job at Barrow, because obviously it was very much associated with the war effort. He was on, I think, submarines at that time. So he left and my mother and I stayed on a few days and then went back on a very stormy voyage from the Isle of Man to Heysham on Morecambe Bay. I recall it because I was very, very seasick and so was my mother. I would be four years old.

Robinson: So is that one of your earliest memories?

Porter: That is, that is.

Robinson: Okay. Now, I'm interested in your early schooling and what started to shape the things you became interested in and what led you ultimately towards a career in physics and engineering.

Porter: Yes.

Robinson: Can you lead us through some of the things that influenced your early childhood there in that direction?

Porter: I had some very good teachers in the small primary school, standards one to, I guess, five, so that would be ages five to 10 or 11. But prior to that I was in a very small private school which cost my parents one penny a day, I remember it well, and at the end of each week we took six pennies to Miss Probert, who was an elderly spinster and had, I guess, twelve pupils, each of us having a slate, and we sat on the floor with our slate and she was a wonderful teacher. I think it was Miss Probert who induced the very idea of learning how to learn, which stayed with me until today, as a very important concept.

Robinson: So she was basically one teacher for the twelve pupils for all subjects?

Porter: That's right, and she taught us writing and a little elementary arithmetic.

Robinson: So what about then as you moved onto, I guess it was grammar school then?

Porter: Yes, but at the little council school there was one or two teachers there, there was a chap, Athersmith, who was a fine teacher. These people were very dedicated, and I recall in those days that teaching as a profession of course was non-unionized. I just can't conceive people like Athersmith and the headmaster of the day, it was Mr. Hubbard, a very austere gentleman, but looked up to by the whole population of the town, I just can't imagine them on a picket line, this is quite inconceivable. But these were dedicated teachers, and I enjoyed school.

Robinson: Would you say they had higher status in society at that time perhaps?

Porter: They certainly did, yes, the schoolmaster and the doctor and the rector, the parish church, were the three, I would say, even ahead of the solicitor, I would say he occupied a slightly lower status at that time.

Robinson: So there's obviously been quite a change since then?

Porter: Quite a change.

Robinson: So can you identify the first event that really got your interest engaged in the scientific side of your studies?

Porter: Perhaps it was when I went to the grammar school, I would say at the time the grammar school was private, in so far it was a fee was charged, and there were five scholarships to the grammar school. And I always recall this, because there was an examination in I think a composition and some arithmetic, and I think back, and I literally was number five, just scraped into the fifth slot, and I think of what the others, what number one boy did, I know he became a school teacher, but that was very lucky that I got that scholarship to the grammar school.

Robinson: So if you hadn't have got the scholarship your parents wouldn't have been able to afford to send you there?

Porter: No, no, no. No, then I would've gone and probably served my time in Barrow-in-Furness, at the shipyard, because you could go from the age of 14 I think.

Robinson: An apprenticeship?

Porter: I could've stayed in the council school until I was old enough for an apprenticeship, that's correct.

Robinson: So at the grammar school are there particular teachers that you recall, shaping you up?

Porter: Oh, I recall most of them actually, but Calderbank was my absolute hero, he was the physics teacher, he was not by any means the best liked teacher, he was Joey, as he was called, Joey Calderbank stood no nonsense at all, and on the other hand he was quite a fantastic teacher. He insisted that you knew what you were talking about, and he continued to stress the Kelvin Principle, which you may recall goes something like this, if that about which you are discoursing cannot be expressed in numbers your knowledge is but of a very meager kind. That's not quite right, but that gives you the general idea, that you've got to be able to express it in numbers otherwise you don't know much about it. And the other thing he stressed was definitions, definitions, know your definitions, so I had these definitions for about fifty items, you know, what is momentum, what is inertia, what is acceleration, and the actual dimensions of these things, the basic dimensions being length, time, mass. Mass, time, mass, time, mass, mass. . .

Robinson: Mass, time and length are the three.

Porter: Time and-- of course, mass, time and length, of course, of course. And--

Robinson: So it sounds as though there was a certain amount of rote learning though there, that you had these definitions and rules and you had to--

Porter: Yes, quite a lot, but also, quite a lot of examples, applications, and <clears throat> of definitions, and that sort of thing. It sort of inspired most of us, I was determined at that stage to be a high school teacher of physics, I mean that was my ambition.

Robinson: And would you say it was the subject matter itself, or just this outstanding teacher that was really the--

Porter: I'd say both, both. And when-- you see, in Britain at the time there were scholarships to universities, and these were not dependent at all on the economic, the level of one's family income, that sort of thing, that was completely independent. In other words, very wealthy students would sit for a scholarship just for the honor, of course, of winning them. And I repeated my last year at school, although I'd passed my higher school certificate, merely to obtain a scholarship, and I managed to get a county scholarship which was the very bottom. Now, my brother, on the other hand, he won a state scholarship, which was very good, I think gave 200 pounds a year in fees, and he won an open scholarship to Queens' College, Cambridge, and he won a county scholarship, whereas I only won a county scholarship, which, well, didn't even pay my fees. So I applied and got a grant for a teaching certificate grant, and that covered my-- it covered my fees for the whole program in physics, for the whole three years, and--

Robinson: That being on the assumption that you were going to go into a teaching profession.

Porter: Not only the assumption, but that if I didn't take on teaching I would have to repay the fees that had been paid for from my course in physics. And indeed, when I came back from the States years later, after I'd done a doctorate, and it was obvious I was not going to be a school teacher, then I started repaying <laughter> this money I got, and it was the war that saved me, because it was civil service got in such a-- in September, 1939, the outbreak of war, everything got in such confusion, vis-à-vis repayments and so on, that I just stopped repaying and nobody did anything about it, so. So I still owe it <laughs>.

Robinson: <laughs> Now somehow I don't think they're going to catch up with you at this point.

Porter: <laughs> So I don't think so.

Robinson: So did that scholarship allow you to choose where you were going to go, or was it tied to a particular institution?

Porter: Well, no, it didn't tie you, you just applied, and I didn't even try for Oxford or Cambridge, I knew there was not a hope. And I was fortunate in getting a place at Manchester actually, and I didn't realize at the time how very wonderful the Manchester School of Physics had such a great history I think of nine Nobel Prize winners among their students and faculty.

Robinson: So it was really a stroke of luck that you landed somewhere that was really great in the subject that you--

Porter: Absolutely, just luck. And then to get together with Hartree was just fantastic.

Robinson: Now, of course you didn't know Hartree when you first arrived there.

Porter: No, no.

Robinson: You were just in the undergraduate program presumably at that point.

Porter: Absolutely, yes, absolutely.

Robinson: So can you say a little about what it was like in those days in the physics program there? I imagine it was quite different from today.

Porter: Well, it was quite a small program, there was the-- <clears throat> excuse me, can I just--

Robinson: Of course.

Porter: Pause for a drink <clears throat>. There were about thirty in the Honors School of Physics, and it was quite a club in many ways, we sort of had independent classes. we didn't meet at all with the chemists, for instance, or any other groups, except in the first year, there were common classes, so that we got together with some wonderful teachers during our program. Bragg, who was the professor, and he'd become Professor at an early age, and he followed Lord Rutherford, both of whom won Nobel Prizes of course.

Robinson: Both while they were at Manchester?

Porter: No, I believe Rutherford got his when he was at McGill, now I wouldn't swear to that. However, they had their Nobel Prizes. And so we were very privileged in that respect.

Robinson: Did you actually get to interact with these people directly, or was it just they were the lecturers?

Porter: That's right, not really, not at undergraduate level, not really. It was quite different to the tutorial system at Oxford and Cambridge. We didn't have tutorials in that regard at all, at undergraduate level. At graduate level of course it was quite different.

Robinson: Now you had to conduct experimental work I would assume as part of the program. What sort of things were included in that program?

Porter: We had about 10 hours a week, I would say, in our finals year. I would stress as well, that the program was three years, not four, as in the States and Canada for that matter, so there was first, second and third year. At the end of second year you did part one, which-- part one of the degree essentially, the Bachelor of Science, and at the end of the third year, part two. So three years really covered the program because at high school at that time, the final year at high school covered most of what freshmen studies covered in other countries, so that's why three years was considered--

Robinson: You really started at a slightly higher level.

Porter: We started at a higher level. We'd done two years of calculus for instance, before entering university, which gave us a good start. So by the third year we were getting involved in fairly specialized stuff, I remember the course in Wave Mechanics, or Quantum Theory, which, you see, I began in 1930, so Quantum Theory was only four or five years old, so this was comparatively--

Robinson: Quite leading edge stuff at the time.

Porter: It was leading edge. And it's a fascinating thought, that when I graduated in 1933 the neutron hadn't been discovered, literally, no neutrons, so the atom, the nucleus of the atom was regarded in those days as consisting of protons and electrons, that was the nucleus. One year later the neutron was discovered by Chadwick at the University of Liverpool, and that of course changed everything, not least gave rise to the nuclear bomb <laughs>.

Robinson: Now in that third year you had to undertake some kind of project work.

Porter: Yes, yes, the third year, as far as the experimental part of the program was concerned, consisted of a fairly straightforward experiment. I think mine was some optical experiment relating to diffraction gratings, and having completed that and writing it up, then discussing with the supervisor a topic for which you would do a sort of thesis, a minor research program. And the supervisor was Brentano, who was the most feared man in the Department of Physics at the time. I mean, because he really held the future of we students in his palm of his hands, because he was not only determined what you were going to be doing in the laboratory, but he was a key member of the examining committee, and so he was really key and he didn't pull punches, he was a-- from year to year he really was the guy. So when I came to meet with Brentano after my first experiment had been finished, I was very fortunate, and here the sort of chaos theory came into effect, in so far as a tiny, tiny happening gave birth to everything else that happened, and the tiny, tiny happening which had rarely happened, if ever, in the past, I went into the library that morning, opened a book completely at random, it was a Cambridge Literary and Philosophical Society, completely at random, and saw a paper, something about a moving coil galvanometer, being utilized to solve a second order differential equation. And I just saw that and it was by a scientist, E.C. Bullard, I remember the name well because I got to know Teddy Bullard in later years fairly well. And I just thought that was that and I put the book back. So being interviewed by Brentano about four to five hours later, he said “What are you interested in?” I said “Quantum Theory.” “Well,” he said, “we're not going to do any problems here on cathode rays or scattering of cathode particles, electron beams and things, so what else are you interested in?” I said, “Mathematical instruments,” you know, things for solving, “Oh,” he said “interesting, I had lunch today with Professor Hartree, and he's just come back from visiting the United States, MIT, and he said to me “Would there be a student in final year physics who's interested in calculating machines or instruments? So Porter, you'd better go and see Hartree right now.” So I toddled off to see Professor Hartree and there in his office, and there he had these bits of Meccano stuck on his desk, and I said <laughs> “Good gracious, what's going on?” because as a boy I'd been given a Meccano set, which was a construction-- building bridges and cranes and, you know all about that--

Robinson: Like an Erector set for folks that are not familiar with the British version.

Porter: You're an expert, and that was that.

Robinson: So Hartree was Professor of Physics?

Porter: No, Hartree was Professor of Applied Mathematics, not even in the Physics Department.

Robinson: So, but it was possible to go and do your project?

Porter: Well, he of course talked Brentano into this. The project Hartree suggested was the torque amplifier, and to do experiments with various materials for the drums, you know, it's the capstan principle, so you’ve got sort of a rope or cords, and materials like brass and steel and so on, and to find out the kind of conditions and speed of rotation and so on, to get a torque amplifier working. And in fact, to figure out how close to the theoretical value of amplification of torque, or force, which is $e^{\mu\Theta}$, which is the angle in radians of the wrap of the rope around the drum, and how close, and of course it comes nowhere near that actually. But you do get considerable amplification, you’ve got a drum running around, and the rope-- say, if the rope’s here this is the slack end, so if you pull on the slack end you get quite a bit of torque here, and with the drum driven from a power source of course. And so that's what I did for my experimental work, set up a experiment on force, not torque amplification, and I wasn't looking so much at torque as the force in a single amplification stage. The capstan really, looking at the capstan principle.

Robinson: So now did Hartree relay to you the sort of big picture of why that was a useful thing to build?

Porter: Not-- well, in a general way, yes, saying that in this model machine that he had in mind a wheel would be rolling on a disc, and that the torque would be very small, and if you put any load on that, on the axis, but it would obviously stop, it would slip, so that you needed a device to amplify that torque. He did go into that.

Robinson: Okay, but I was thinking of the big picture of the machine that that would then be a part of?

Porter: Yes. But I was not too, until I'd finished, you know, I didn't sort of get too involved in the machine process, just in this very, very small--

Robinson: So you said that Hartree had just come back from the United States. So what was he actually doing while he was over there?

Porter: Well, <clears throat> one of the key people he came to see, he went to see at MIT was Slater, and Slater was Chairman of Physics at MIT, and like Hartree, he was very much involved in Schrödinger's equation, and which related to the energy levels in atoms, and indeed Hartree set up, developed equations which simplified the Schrödinger equations for these energy levels. And that was the prime reason for Hartree going, and at the same time a secondary reason was to see if the Differential Analyzer would be suitable-- could be adapted to the solution of the Hartree equations.

Robinson: So was he aware of the Differential Analyzer prior to his visit?

Porter: Oh I think so, do you think I could have a couple of minutes break?

Robinson: Sure, yes.

Porter: Yes.

Robinson: So Hartree was obviously a person who was going to have a major influence on your career?

Porter: Oh tremendous, tremendous influence.

Robinson: And he had been working on this Self Consistent Field Theory. I think he was one of the developers of that?

Porter: Yes, well, I mean the developer. I mean it's called the Hartree Method.

Robinson: And he was solving, as I understand it, those equations numerically, I mean what-- I guess this is 1933 or thereabouts, right, what really did it take to do those kind of numerical calculations?

Porter: Oh, many, many hours of-- he was solving the equations as difference equations, and using Brunsviga and various calculators involving turning handles.

Robinson: Literally cranking out the numbers.

Porter: Cranking out the numbers. He must've spent hundreds of hours.

Robinson: So he was doing that work personally himself?

Porter: Oh absolutely, he was just a wonderful, wonderful man in so many ways.

Robinson: So obviously that must've given him a powerful motivation to find a better solution to the problem.

Porter: Of course, yes, I think you're right.

Robinson: Now you say when he went to MIT that although he may have been aware of the Differential Analyzer, that wasn't the primary purpose of his visit, but I guess he did get to see it and use it while he was there?

Porter: It may have been the primary visit, the primary purpose, I'm not sure, but because, I say he certainly visited Slater at the time. So there was the dual, there was the dual purposes, and I'm sure the Differential Analyzer loomed largely as a prime purpose.

Robinson: And he came back obviously very enthusiastic about that machine.

Porter: Yes <clears throat>.

Robinson: So your Bachelor’s project then was really exploring the torque amplifier, the essential component to be able to make that machine work.

Porter: That's right, yes.

Robinson: And how did you decide what to do beyond your Bachelor’s degree at that point?

Porter: Well, at that point everything depends on the level of-- at what level you pass your final examinations in the degree, examinations, because scholarships are awarded, these are the graduate level scholarships, on the results of the examinations. And indeed, really in Manchester at the time, unless you graduated first or second in the class you were out of luck, really.

Robinson: Really had to be in the top tier to be able to move on.

Porter: Absolutely, yes. And I'm fairly sure that my placing depended quite a lot on Hartree and Brentano giving me pretty high marks for my work on that torque amplifier, although I did-- I think I did well on the other, the rest of the exam. So, I was lucky. So when I knew that, then Hartree had obviously decided that I was to work with him and--

Robinson: You were the man to carry that forward.

Porter: And that was the beginning of the partnership.

Robinson: OK, let’s just stop there and change tapes I think.

Robinson: So when Hartree asked you to continue working with him, was it clear to you what your master's thesis topic was really going to be or was it a leap of faith that you were going to be doing the right thing?

Porter: Well, not-- no, the only thing that was clear was building the machine. There was no-- at the beginning of the year there was no indication that I would use it for determining the wave functions of the atoms of hydrogen and chromium for example; that didn't come in. The key problem first was to get the machine going, and that's what I concentrated on.

Robinson: Now, was the purpose of building that first machine that you're talking about here was the one built out of the Meccano parts--

Porter: Yeah, well, it was the one integrator. There's a photograph of that somewhere and that gave us the confidence that if one integrator worked then surely three integrators would work.

Robinson: Right. But was the purpose of building that machine at that time really in Hartree's mind, that "Well, once I've built this, we'll be able to use it to solve these self consistent field problems”?

Porter: Oh, I'm pretty sure.

Robinson: So he really believed that the Meccano machine would be good enough to do that?

Porter: Well, I would think so. After all, he spent about 20 pounds on it, so I think he probably felt it would work.

Robinson: So he was funding that--

Porter: He funded it.

Robinson: Out of his pocket?

Porter: Of course, oh yes, he paid for the whole lot.

Robinson: Could you just describe that machine for us, the principle of it and the various parts you had to design and build for it?

Porter: Golly. You mean, the-- well, you don't want me to describe the actual integrators.

Robinson: Well, just those things that you feel are unique to that kind of approach to calculation.

Porter: It would be much easier with the photograph, for instance.

Robinson: Well, okay, we can do that. <Camera pans to photograph> So this is the picture of the completed Meccano differential analyzer.

Porter: And the three major components are the three integrators, there, there, and there, and they consist essentially of-- I don't know if you can see it in detail, a disk which is here and the wheel rolling on the disk and the mechanism here which amplifies the torque, so that the wheel doesn't slip on the disk. And then shafts which connect that integrator with the others through these interconnecting shafts which run the full length here. And then there's an output table here, where you plot graphs which show the solution of the equations. There's also an input table here and I am providing input to the machine. See me turning a handle here, and this too, provides information which turns shafts which moves integrator tables, integrator positions and so on. So that's roughly what it looks like and what it does.

Robinson: And that's Hartree standing in the back corner?

Porter: That's Hartree. You notice an interesting thing. We're both wearing ties, you see that?

Robinson: Working in the laboratory wearing suits and ties. <laughter>

Porter: Working in the laboratory wearing ties, that's right.

Robinson: Things have changed a little bit. And there we can see one of those pieces of plywood you mentioned just sticking out of the front there-- didn't quite join up here.

Porter: Yeah, sticking out here, yes, right out of place, completely out of place. Incidentally these photographs were taken by a very wonderful guy called William Kaye who was the chief steward of the laboratory and was bequeathed to the laboratories by Lord Rutherford. Lord Rutherford regarded Kaye almost as his right hand man; he produced all the glasswork for the Rutherford experiments, and when Bragg took over, Kaye remained as chief steward. And I have the most warmest feelings for William Kaye, always "Mr. Kaye". I remember on the occasion when I gave a colloquium, because when I became a graduate student and just after I'd built this, the machine, I had to give a lecture on it, it was called, the junior colloquium. And I really was nervous, very nervous, and I remember Mr. Kaye coming to me, and he said, "Mr. Porter, remember that the great man himself was always nervous before he gave a lecture," and he said, "I can assure you that if you're not nervous then you won't give a good lecture. You've got to be nervous to give a good lecture." That was what Kaye said to me. Wonderful man. He was a good athlete, too.

Robinson: So how did the lecture go in the end?

Porter: I think it went not badly, not badly. I've done better since, but-- <laughter>

Robinson: Well, you were still an under-- well, no, you were not an undergraduate at this point, you were now a master's student.

Porter: That's right. Good.

Robinson: And I think the other picture you have there, right behind it, is an example of the output the machine actually produced, right?

Porter: Oh, yes, that was a thrill, that one. That really was-- I think this was one that you personally enhanced-- <Camera pans to photograph>

Robinson: There has been a little bit of modern PhotoShop to enhance it, yes.

Porter: You enhanced that one.

Robinson: But the underlying data--

Porter: The underlying data was a pencil curve, a wave function of chromium, and Douglas Hartree was so excited with that; so was I. And we thought well, these bits of Meccano, and a few other bits, produced this curve, and we thought, well, that's something that-- almost miraculous to us at the time. So very good.

Robinson: And in fact the Meccano company got very excited also?

Porter: They did.

Robinson: And published an article about this.

Porter: Yes, they did.

Robinson: Do you remember your interactions with them at that time?

Porter: Of course, 1934.

Robinson: What was it that you think got the Meccano Company excited?

Porter: Well, it was something quite different. Up to then, Meccano had been used essentially for building models of cranes.

Robinson: It was a child's toy.

Porter: It was a child's-- making toys essentially and this was definitely a piece of scientific equipment. As a matter of fact it had been used for many years in laboratories all over the place, but it had never been used in quite this way, in a very specific fashion, and I think that's what intrigued them.

Robinson: And so they actually went ahead and published an article--

Porter: And published the article.

Robinson: In the magazine of the company.

Porter: That's right.

Robinson: So your master's thesis then was partly the construction of this model machine and partly the work you did in particular on the chromium atom?

Porter: On the hydrogen and the chromium atom, yes.

Robinson: When did you actually graduate with your master's degree?

Porter: That was in the summer of 1934.

Robinson: And you stayed at Manchester at that point to continue the doctoral program?

Porter: Oh yes, yes. And then carried on with the Ph.D. The large machine, the McDougall differential analyzer was finished in, I think, late 1934. Well, four integrators were. And I used the four integrators and then later the eight integrators. So--

Robinson: So literally they just brought another piece up and bolted it on to extend the size of the machine?

Porter: Right. But my Ph.D. <clears throat> thesis did in fact include some work with the Meccano. The Meccano model was used for the finite time lag control problem involving the special input table-- input/output table, where you put in the finite time lag actually-- could be modified and--

Robinson: Perhaps it's worth taking a little diversion on the subject of the control theory because that was something that was very much a research topic at this time.

Porter: That's right.

Robinson: And you and Hartree were involved in the early stages of that.

Porter: Very much, yes.

Robinson: --how that developed.

Porter: That's right. In fact, it's been cited as one of the seminal papers in control theory at that time.

Robinson: Can you say a little about what the subject matter of control theory actually is and what the kind of problems were that came up.

Porter: Can I have another drink?

Robinson: Certainly.

Porter: Control theory is applicable of course in a very broad range of technological systems. Driving a motor car, for instance, is a form of control. When you use the steering wheel to steer the car, you're controlling the position of the car and invoking the elements of control theory. But as far as the build up of the theory, it was the chemical industry that brought about what we generally call control theory. In the early days controls were essentially on/off; that is, a tap was turned on or off. A switch was on or off. Later, things were more sophisticated and you got proportionality, where the degree to which you're controlling something was proportional to the error. Later still, you brought in the rate of change of error, and the integral of error, so that you got no build up over time. So you got what was called the three-term controller, the proportional-to-error term, the derivative-of-error term, and the integral-of-error term. And the differential analyzer was very suitable for studying these kinds of problems because you could in fact simulate the total system, and change the parameters of the system and the controller; that is, the coefficients associated with the proportional derivative and integral controls accordingly in order to optimize the control of the process. And that's what we did. And that work of course has been expanded greatly since, with the computer being essentially the controller.

Robinson: The modern digital computer that is.

Porter: That's right.

Robinson: So effectively you were modeling this system as say a second order differential equation and then solving that equation with all of the nonlinearities, whatever may be in the system, using the differential analyzer as the tool.

Porter: That's correct, yes, yes.

Robinson: So what did it actually take to take a problem like that and translate it into the language of the differential analyzer, effectively program the machine to put the problem on?

Porter: Well, it was fairly simple actually. It wasn't as difficult certainly at the time as programming a digital computer. Not by any means. Because-- now, I'm getting a bit--

Robinson: Well, of course, when you were working on this first Meccano machine, there were no digital computers.

Porter: There were no-- that's what I mean. I was jumping ahead a bit and thinking of the time I was trying to utilize a digital machine and working in machine language, which was a very time consuming process, and very difficult. So I would say it was a simple process. I mean, programming a differential analyzer, I mean, very simple, and that's why I think of it as being a great educational tool because you can visualize when you set up a diagram of the machine, and how the parts are interconnected, then you get this visualization which lends itself to understanding the process of solving the problem. So to me it's a very simple and straightforward process.

Robinson: But I think the point that I wanted to touch on was that it does involve mechanical modifications to the machine; you have to insert gears and so on, it's not like you think of programming on a modern computer where it's purely a software undertaking.

Porter: Oh, yes, yes.

Robinson: So you really get your hands dirty with that machine?

Porter: Oh yes, sure. Oh, sure, yes, yes. Yes, you're playing around with gears and shafts and that sort of thing, so you get your hands greasy, there's no doubt about that. <laughter>

Robinson: So how did the big machine come about? I mean, how did Hartree go about making that happen?

Porter: Well, I remember the day that Mr. McDougall, who was head of a very large flour making company, being shown the model differential analyzer, the Meccano.

Robinson: I had never made that connection. So he was the McDougall of McDougall's Flour?

Porter: That's correct. He was the McDougall of McDougall Flour. And he was a member of the board of governors of the university and I suspect it was at the end of one of their meetings that perhaps another board member that had heard about this machine-- because by then odd bits had appeared in the News Chronicle or some Manchester paper, and a bit of interest had been generated, and maybe he said that he would like to see it. And so one of the board members got hold of Professor Hartree and Professor Hartree took Mr. McDougall to see it and he sort of said-- told him about the Bush machine, the real, properly machined differential analyzer and what a great asset it would be for the physics department and for the university as a whole to have such a machine, and Mr. McDougall provided the wherewithal, and I think it was about 6000 pounds.

Robinson: Wow.

Porter: And Dr. Vannevar Bush provided all the drawings of his first machine and Metropolitan Vickers were approached to build it, and Starling, Mr. Starling was the designer, and his assistant was called Edgar, Ted Edgar. And they built that in an incredibly short time, within three years, they built the whole eight integrators and had four working in about two years. Because in my Ph.D. thesis, I was finished in 1936, I got the degree in 1936, and most of it, I did on the big machine, so you see, they couldn't possibly have started—I didn't complete the model until 1934. And my Ph.D. was in-- So it was pretty fast.

Robinson: Did Metropolitan Vickers have any relationship with the university before? Were there people there who had the connections to make it happen quickly?

Porter: Yes, I think-- I have an idea that the director of research was on the board. I'm fairly sure that the director of research of Met Vick was on the board of the university. But it was a real achievement to build it as quickly as they did.

Robinson: So they started from Bush's basic design; you say they had all the plans.

Porter: Yes.

Robinson: What were the principal differences between your machine and Bush's machine?

Porter: Well, they had two tiers of the bus bars, that is the interconnecting shafts. They had a lower and an upper deck, which gave much greater flexibility for the interconnections, of course. So that was one. The other was they had the input/output table for problems involving finite time lags; they had a much more elegant cabinets for integrators; they were built in pairs and contained in oak.

Robinson: I've actually seen those at the Science Museum in London.

Porter: Very nice pieces of furniture, really. And they also had a system for obtaining numerical output as opposed to graphical-- but I don't think it was ever used to my knowledge.

Robinson: So that was the digital counters-- the camera that could take pictures of them.

Porter: Yes, the camera; we always used the graphical output.

Robinson: So really in practice and use, it was a purely analog machine--

Porter: Purely analog.

Robinson: And the digital attachment never got used.

Porter: No.

Robinson: So this machine was then installed right next to your little model?

Porter: It was next door, yes. Interestingly, just one aside, is that Rudolph Peierls, who became Sir Rudolph, and who was the physicist who determined the critical mass of uranium, three-- oh, my goodness, what is it, the critical-- it's uranium-- 235. 235. And he determined the critical mass, Rudolph Peierls, and on the day of the Physical Society Exhibition both the large machine and the small machine were on display. It was open to the public and I asked Rudolph if he would demonstrate the model and I would demonstrate the big machine, and Rudolph agreed. So he demonstrated the small machine and I demonstrated the big one on that day. It was a Saturday.

Robinson: You must have had lots of dignitaries and people visiting.

Porter: Oh, yes.

Robinson: Can you remember who some of those people were?

Porter: Well, I think of Rutherford; let me think of the Nobel Prize winners first. Rutherford, Chadwick, Mott, G.P. Thompson, he's the son of J.J. Thompson, Blackett, PMS [Patrick Maynard Stuart] Blackett, Cockroft, who was my examiner, Ph.D. examiner; well that's just--

Robinson: That's a pretty impressive list of names.

Porter: That's right. Oh, and Darwin, of course. Darwin. The grandson of Charles. He was also Charles.

Robinson: He was also Charles.

Porter: That's right. He was the grandson. And he was the one who leaned on the output table and we were having this-- looking at this problem of the possible occurrence of subharmonics in radio loudspeakers, and using Lissajous figures on the output table to determine whether or not a subharmonic was being generated and of course the criteria was that if a subharmonic was being generated, then instead of a single loop, a subharmonic would be shown up as a double loop; it wouldn't join up on the first go round, it would join up on the second. So you'd have this double loop. Just like this. And by great good fortune-- it was complete luck, because I hadn't succeeded in getting the coefficients right in the non linear damping in the loudspeaker, getting that coefficient right. I just set something quite at random before Darwin arrived that morning because Hartree called to say he was bringing Darwin, so "Set it up, Porter, get is set up for something" so I had set it up and to my utter astonishment, it produced this Lissajous figure with the double loop. Literally that's the first time that it had happened, and furthermore there was this little bump and Hartree, or rather Darwin, looked at this, and he said, "But what's this?" and Douglas Hartree said, "Well, all I can say, Darwin, it seems to be the Darwin bum effect," because he'd seen Darwin's bum leaning on the output table. <laughter> So that was it. A very wonderful experience to have set that thing up, completely random and it produced this subharmonic.

Robinson: I believe you also had a group of football supporters there.

Porter: Oh, my goodness. That was really something. This was the day of the British football cup semi-final, a great occasion, and Manchester United were involved in this, and the exhibition, the Physics Society Exhibition was on, and we were open in the evenings, the laboratories. And I recall these gang of supporters, Manchester United fans, coming in, quite, I suppose, slightly intoxicated I suspect, some of them. Anyway, they piled into this small room and I showed them the machine running, this was the model machine, and they really were quite fascinated, these mostly men, and at the end one of them said-- they had real Lancashire accents; I've got a bit of a Lancashire accent, but these people were really broad Lancashire, and I remember him saying, "I think we should give this young fellow a good hand; so, let's do that, fellows." So they clapped and there I stood, very embarrassed that these football fans gave me a clap. <laughter>

Robinson: So, I know when the big machine was inaugurated it was also a major event for the press being interested and so forth. What kind of experience did you have of the excitement caused when the machine was inaugurated and announced?

Porter: Do you know, I cannot remember it much. When it was opened to the press, you know, I don't think I was there. I think it was delayed until I was probably at MIT by the time they--

Robinson: They actually came out--

Porter: They actually did the formal thanks to McDougall and so on. But I certainly don't recall anything, no.

Robinson: Okay. I'd be interested to understand what it was like, you know, the day to day operations of this machine. You've got the big machine installed, and the kind of issues that you ran into in actually operating the machine and maintaining it and keeping in a functional condition. What was really the routine involved in doing that?

Porter: You mean the big machine?

Robinson: Yes.

Porter: Not, the Meccano. Well, the only problem was I suppose the torque amplifiers. They were, they had to be adjusted, the tension in the bands, continually, but apart from that, it was just routine maintenance, about once every two or three weeks, we'd set it up for the circle test and you know, that $d^2y/dt^2 = -y$ and get-- plot dy/dt against y and that is plotting a sine against a cosine-- and a circle.

Robinson: Should produce a perfect circle.

Porter: And how well they join in plotting the circle is effectively how accurately the machine is working, and we would do that. The inaccuracies, of course, are caused by backlash in the gears, chiefly. So that was the sort of thing that we did. But maintenance was not much of a problem. Metropolitan Vickers had done us a very good job. And I seem to remember we had a log book--

Robinson: Yes, in fact, that log book is in the Science Museum in London. I have actually seen it, and it made some very interesting reading--

Porter: Oh, I had forgotten.

Robinson: --at least during the commissioning. The number of times, you know, you've got the date and "replace the belts on torque after fire number 3," "adjusted torque amplifier number--" It seems, as you say, that that was the major problem with that machine. It was very, very illuminating to read that document.

Robinson: So it's in the Science Museum

Robinson: It's in the Science Museum, yes.

Porter: I see. Good gracious. Right. In fact, that's how, of course, Bonita Lawrence came in contact with differential analyzers, through going to the Science Museum and seeing that exhibit, isn't it.

Robinson: So half of that machine is in the Science Museum and the other half is in a museum-- is it at the University?

Porter: No, I think it's at the Manchester Museum of Science and Technology. I think so.

Robinson: I've seen the portion in London.

Porter: And I think it's in working condition. I think so.

Robinson: I don't know if they actually operate it though? Maybe it's, in principle, working.

Porter: Yes.

Robinson: So on the big machine, just like the little machine, how long would it typically-- if somebody came with a new problem that they wanted to explore on the differential analyzer, would you be the person that would have figured out how to set it up on the machine? Were you sort of involved in the--

Porter: For some, but a problem like the heat conduction problem in a semiconductor under high voltage, alternating voltage, at what level of voltage does the dielectric break down. So it's a heat conduction problem in a dielectric. A problem like that I couldn't-- I wouldn't cope with that. Douglas Hartree, he would and did brilliantly, because, well-- first of all, they were partial differential equations, so he set it up as a step by step process, so that one independent variable would be as smooth and the other would be stepped. So a problem like that-- and he did similarly for a problem in aerodynamics, air flow over an airfoil for an aerodynamics problem, he did that, he did. But most of them, certainly for my Ph.D., I did the set ups and the, you know, it was fairly straightforward.

Robinson: We need to change tapes again at this point.

Robinson: There were a couple of last questions I think I had relating to the big DA at Manchester, Arthur, if we can finish up with that. So you talked about the special input table that you had on the machine for dealing with the time lag problems. Could you just elaborate a little bit on that because I think that was a unique feature on your machine that wasn't on any of the other differential analyzers that I'm aware of.

Porter: Well, <coughs> oh, excuse me, bad start. Well, the special input output table, it provided a facility whereby the output plot was being made, and at the same time, but following at a certain interval, which could be varied, would be a crosshair manually controlled following the curve which was being plotted as the output from the machine. So in fact you were getting an input into the machine derived from the output function but delayed by a certain amount, thereby providing the finite time lag.

Robinson: And the sort of problems you mentioned were things like the chemical process control that might involve that?

Porter: Yes, the best way of thinking of it is consider a chemical process with a chemical flowing along a long pipe, but the control valve is at one end, and for some reason or other, it's got to be located at this end, and the system it's controlling is at the other end. So in fact, although the control part is staying in place here, the actual process of control is taking place here. So this is known as a distance velocity lag;. the velocity of flow of the fluid and the distance over which it's flowing. So that distance divided by velocity gives time and that is the time, finite time delay in the process. And obviously, it's a destabilizing influence because no control's taking place during that period.

Robinson: There's sort of dead time--

Porter: It's a dead time.

Robinson: --between taking action and it actually having any effect.

Porter: That's right.

Robinson: I'm also curious on the sort of problems that you actually ran on the machine. I believe your big machine had four input tables, if I remember correctly?

Porter: That's right, yes.

Robinson: Did you ever use four input tables?

Porter: Not that I can remember. In fact, I'm fairly sure-- in fact I just can't conceive problems involving four input tables.

Robinson: Well, I think that aspect of the machine may have been a hangover from the original before people had figured out that you could actually use integrators to do--

Porter: Absolutely, that you could produce trigonometric functions and algebraic functions through integrators.

Robinson: So you completed your doctorate using the big machine and I think your thesis title was "The Differential Analyzer and Some Applications."

Porter: Yes.

Robinson: Can you remember what those applications actually were that you studied in your thesis?

Porter: Well, the first one and the biggest problem was this time lag problem, which was the work carried out in collaboration with ICI and in some place in Cheshire. But Imperial Chemical Industries, of course, is the biggest chemical company in Great Britain. So that was the really big problem. Then another problem was the study of the characteristics of transmission line behavior when struck by lightning, and in particular, the behavior of Thyrite lightning arresters. And again, finite time lags were involved in that due to a reflected wave. I won't go into it, because quite frankly I don't remember the details now about 70 years ago since I studied it, <laughter> so my memory's a little bit hazy.

Robinson: That presumably also had non linear elements in it, which of course the differential analyzer--

Porter: Yes, the Thyrite arrestor is a non linear element, yes. Another problem was, I think I mentioned before, the radio loudspeaker, the element possibly generating a subharmonic. Another was a problem related to the behavior of a triode. Now, this was the days of valves and diodes and triodes, now of course replaced by transistors. But in those days these were vacuum tubes and we were studying so-called secondary emission in these triodes. That was a problem submitted by Metropolitan Vickers, as a matter of fact. Those were the problems I can recall. They were the major problems.

Robinson: Now did you have to have an oral exam as part of your thesis?

Porter: I did. Very interesting one. In Manchester at the time, the system required two examiners, one the internal examiner, normally your thesis advisor, which in my case of course was Professor Hartree, and an external examiner who would come from another university, and in my case was Professor John Cockroft, from Cambridge. And just the two of them sat in Professor Hartree's office and I appeared and was told to appear at the blackboard and answer questions. They were very, very kind to me, I must say, and I just can't resist mentioning one particular question which related to the finite time lag problem, and in particular to this introduction of the derivative of the error function in the control. And I remember Cockroft asking the question and I went to the board, I was at the board actually, and obviously I didn't know the answer and I fooled around with it. And after about five minutes, I remember Professor Hartree saying, "Cockroft, do you know the answer?" And Cockroft says, "No." Then Hartree said, "That makes three of us. Let's carry on." Well, of course the point was that Hartree, obviously, he knew the answer completely, and so did I about 10 years later when I really understood something about those things <laughter>, and it was really quite a trivial question to anybody that really understood control theory, but Hartree really got me off the hook there. He just was so wonderful.

Robinson: So on with your newly minted Ph.D., what did you set about to do as your next step in your career?

Porter: Well, about this time Bragg, Professor Bragg, he was a member of the Award's Committee of the Commonwealth Fund Fellowships and this committee at the time was chaired by the vice-chancellor of the University of Manchester, of course, where Bragg was and I was. And I, unfortunately I can't remember his name-- anyhow, Bragg called me to his office one day and he said, "Have you thought about applying for the Commonwealth Fund Fellowship?" and I said I hadn't even heard of it. So he told me a bit about it and he said, "I think it would be a very good idea if you applied," and he said, "Do talk to Professor Hartree." And of course I did, and of course Hartree was very enthusiastic. He said, "If you got one, of course, you could go to MIT with Vannevar Bush, because unlike the Rhodes Scholarships, which are awarded for tenure at Oxford exclusively, the Commonwealth Fund fellows can attend a university virtually of their own choice. Of course it is approved by the Commonwealth Board, but generally if it's somewhere like Harvard, Yale, MIT, and so on, Berkeley, it's always approved.

Robinson: Was it always to the U.S.?

Porter: No, it's always to the U.S. and they were funded by the Harkness Foundation. And in fact subsequently the title "Commonwealth Fund Fellow" was changed to Harkness Fellow, the Harkness Fellow. And there were 24 appointed each year, about 12 were at doctorate level and 12 baccalaureate level and the requirement was-- there were only really two stipulations: one that one would return to UK or the British Empire after spending two years in the United States, and the other requirement was three months travel in the United States, I mean, which wasn't hard to take, which of course they funded. One thing about the Harkness Foundation, they were extremely generous with the funding. They even provided funds for clothing and they sent a dress-- clothing requirements which started off with tails, dinner jacket, which is tuxedo, then two business suits, blazer, four pairs of slacks and so on and so on. I mean, unbelievable, and this was the required clothing you needed, and which came to quite a tidy sum, which they funded. So it was a really quite fantastic organization at that time. The interview was in London, and they interviewed I think about 50 people and half of them were awarded the fellowship. But it was a rather intimidating affair.

I remember sitting in the waiting room and suddenly one end of the room opened up and there was the examiner, or rather the committee of award, and for instance, there was Lord Halifax, was it? There were eight people and Southwell, Benians master of Balliol, the Rector of Imperial College. They were a very prestigious committee. Sir Lawrence Bragg, my professor, he was on it. And as I say, it was pretty intimidating suddenly to be faced-- and of course I recognized people like Lord Halifax, who was then Foreign Minister. But fortunately Bragg got me off to a great start. He said, "Why are you particularly interested in going to the United States and where would you go if awarded the fellowship?" Well, of course, he knew what I was going to say. So I got going on the differential analyzer, which obviously went over quite well.

Robinson: Were any of the members of that committee familiar with the differential analyzer?

Porter: I wouldn't think so. No, I don't think so.

Robinson: So they were going by your enthusiasm level?

Porter: Except Bragg of course.

Robinson: And obviously you succeeded in being awarded that.

Porter: That's right.

Robinson: So when did you take off for--

Porter: Went in September, 1937.

Robinson: And you arrived at MIT and got straight to work with Vannevar Bush's team?

Porter: With the team.

Robinson: Can you describe that project a little for us?

Porter: Well, it was the Rockefeller differential analyzer funded by the Rockefeller Foundation. And very obviously the most ambitious analog/digital computer ever conceived and built. It was a 16 integrator with several input tables, and I think a couple of output tables, but the interesting feature was that the basic units like the integrators and the adding units were all analog in operation, but the interconnections were essentially digital. In other words, instead of the bus bars, the long shafts and gears interconnecting the units, these were replaced essentially by switching systems, which used at the time telephone switching gear, which were provided by the Bell System for free. And it was the network of switches that Claude Shannon, who later did this fantastic job on information theory eventually leading to the Internet and everything else relating to this world of information, that was charged with developing a design for that system. So you saw information being transmitted from the integrators, for instance, using servos through a network of switches, to other integrators and the like. So the machine was used very extensively, I think in the Manhattan Project and in ballistics work, in radar development, during World War II. But then, and this I find it very difficult to comprehend, it seems to have disappeared off the map. I don't think any parts of it remain.

Robinson: Today.

Porter: That's right.

Robinson: As far as I know the only thing the MIT Museum has is one of the integrators from the original Bush machine. It's very sad. So you mentioned Shannon there. Now, I understand you actually shared an office with him at MIT.

Porter: Yes, yes.

Robinson: Can you tell us anything about him?

Porter: About Claude Shannon.

Robinson: His personality or anything?

Porter: Well, he was a very shy and retiring personality. A very good poker player and very, very bright individual. In his work on information theory for instance he drew this fascinating parallel between thermodynamics and particularly entropy, the concept of entropy, and the concept of information that such an idea as entropy would only occur to somebody like Claude Shannon. And that was-- he would come up with concepts that seemed so very far off the mainstream. He continually amazed me. He was actually the operator of the original Bush machine while he was doing this research on information theory. His paid job was to run the machine.

Robinson: Was to literally get his hands dirty running the machine. <laughter>

Porter: Running the machine, that's right.

Robinson: I would never have guessed that given what he went on to do later.

Porter: Yes, that's right. He went to Bell Labs, of course.

Robinson: Of course. So when you arrived at MIT what was the status of the project at the time when you arrived?

Porter: I think they'd got about, I suppose, about 10 integrators. But it was very decentralized, I mean, they didn't have one room where the thing was being assembled. So that the team, which-- and I've got the team listed on that testimonial-- we were just working in offices, and the machine was being assembled essentially in the MIT workshops. And so I didn't see it really and none of the others, as a machine.

Robinson: It was just a collection of parts still at that time?

Porter: That's right.

Robinson: And so everything was actually manufactured at MIT?

Porter: Oh, yes, yes. Except the interconnecting switch gear which came from the Bell company.

Robinson: And what was your assignment on the project?

Porter: Oh, I was on the design of the function unit. The idea there was to replace the graphical inputs as on the input tables by tabulated functions and to be able to use punch tape inputs-- as input to the integrators were to be punched tapes, too. The set up diagram, the set ups of the machine were on punched tape. These were-- the setting of the connections in the network, the telephone sort of switchboard network. This was all punched tape. So for a start I looked at the difference-- you get your tables and then differences first-- approximations to derivatives, and to try and set up-- the basic idea was to fit polynomials so you got in the coordinates of a point, say-- this is all very difficult to explain without pencil and paper, really. And so if it was a simple straight line you wanted to put in, you just needed two points, and then you've got a straight line defined by two points. If you had three points, they could be defined by a quadratic, that is, a second order equation. And you get it done. If it was a fourth order, then it would be defined by a third, a cubic, if you had four points, you'd get a cubic. And so you would try to fit the curves, and if the sort of next point coming up didn't fit, you'd measure the error and try to change the coefficients. But this gets much too difficult to--

Robinson: To try and explain--

Porter: This is the so-called Porter-Stoneman extrapolator system, but it's difficult.

Robinson: But obviously the objective was to eliminate the human element of feeding data in graphically so the machine could be more automatic.

Porter: Of course, sure.

Robinson: Now, because you only had two years on that fellowship, was the machine actually complete by the time you left?

Porter: No, no. No, it didn't, and indeed I don't think it ever was completed, and for the functioning, for the Manhattan Project work on the atomic bomb and so on, they used manual inputs and they used graphical inputs with manual tracking systems.

Robinson: Now, the machine, of course, used a fairly sophisticated system of servos to connect all the parts together, so that ties in with the other servos that we've been talking about, as servo systems being simulated by the differential analyzer. So, here's an example of, if you like--

Porter: Excuse me.

Robinson: Do you want to take a minute?

Porter: It's my eyes are really itchy, sorry.

<Crew talk>

Porter: This is worse than writing an exam. Ooh, that's a shocker. I'd better have a candy. So I met Patricia in May 1940.

Robinson: Okay, back in England.

Porter: Sure. Well, let's see. I guess I'm beginning to flag a bit.

<Crew talk>

Porter: Okay.

Robinson: So I was just asking you, Arthur, about the servo systems and the fact that differential analyzers were a great tool for designing servo systems, and here's this great new differential analyzer which uses some of the most sophisticated servo systems. So is that an early example of computers being used to design computers, would you say?

Porter: I don't know much about that actually. That was part of the-- you mean the servos of the RDA? No, I know very little about that.

Robinson: Okay. So in 1939, your fellowship was complete, you came back to England, and was that just before or just after the declaration of hostilities.

Porter: It was before.

Robinson: So when you got back, what were your thoughts about what to do next?

Porter: Well, it was fairly obvious that I'd have to be involved in some kind of war activity, although on one hand Blackett was very anxious that I should go back to Manchester and take over the differential analyzer because Hartree was going up to London to be involved in, again, certain military activities, and so there'd be nobody really around to run the machine, which could of course be used on all kinds of defense problems. On the other hand, Bragg talked to Sir Charles Wright at the Admiralty, and he was anxious that I should join the Admiralty Research Laboratory, and indeed I went for an interview with them, and I was offered a job there, which I turned down, whereupon Bragg wrote me a real crushing letter in which he said, "Now is not the time for a young man to decide on what he wants to do, but to do what his country requires him to do," something along those lines, which really was a bit of a crushing comment coming from a Nobel Prize winner to a young man just back from his two years at MIT, which Bragg himself had done a great deal to encourage and got me there. However, so I relented and accepted the Admiralty job and joined the Admiralty Research Laboratory the day after war was declared, that is, on Monday, the 4th of September, 1939.

Robinson: What was your role there?

Porter: Well, they put me on what I regard as a very trivial problem, and in fact the more I think about it, the more I think it was quite ludicrous. It had no value whatsoever. It was completely-- it was a simulation of a Bofors predictor system set up in the laboratory. Well, first of all, there was nothing approaching conditions which would exist on the battlefield, so to speak. The gun would be firing about 120 rounds a minute and therefore the vibration would be quite high and trying to follow a target on a telescope mounted on this thing would not be inconsequential. So that was the first thing. This was set up in a pristine situation, everything set up so that you could get accuracies incredibly high, but of course the conditions just didn't correspond to anything likely to be met in practice. So there was I trying to determine rates of enemy targets, simulated in the laboratory, to fractions of a degree per second, under these highly idealized conditions, and being told that I wasn't getting them accurate enough.

So I was not very happy there. And indeed, I told my supervisor I was going to join up in the Air Force or something like that and I was promptly told that if I tried, being in a reserved occupation, I'd probably land in jail, that was literally the response. Until one day, to my great good fortune, Blackett arrived on the scene as a member of the inspection committee from the-- the government had these committees going around the government laboratories, and he spotted me in the laboratory, which was quite unusual in a way because I used to spend at least 90 percent of the time in my office cooking up other systems to do this tracking problem. And he came and he said, "How are you doing, Porter?" and whereupon I said something like, "Terrible. For God's sake, get me out of here." <laughter> And he said, "I will." And he did, within about 10 days. And so that's how I joined this group in calibrating early radars and got into the Blackett's circus, so-called, great people, like Andrew Huxley and Bayliss and Hill. There were just six of us placed on gunsights and these were the very early radars which the army people in anti-aircraft command, they didn't have a clue of course, it was all new technology to them, so we civilians were brought in to take care of the operation of the radars.

Robinson: I think we need to change another tape here.

Robinson: At some point in that time, you became involved with Freddie Williams?

Porter: No, that was much later.

Robinson: Ah okay, I must have got that wrong.

Porter: Much later.

Robinson: So the work was really to do with servo systems and control of the guns from the radar, target information coming in from the radar. Is that my understanding?

Porter: Well no. We really, as Blackett's Circus, we were concerned with the effective use of what equipment they had, ensuring it's adequate calibration. We would bring in Lysander aircraft flying with oscillators tuned to the frequency of the radar so that we could calibrate our system, and trying out new methods of coordinating the radar with the guns and predictor equipment.

<phone ringing>

Robinson: So you had a particularly interesting incident to report. We're rolling.

Porter: Oh I see, I didn't realize. Then we're on?

Robinson: Yes, go ahead.

Porter: This was after I'd been at the Admiralty Research Laboratory, I would imagine, about four months. So this would be in January, 1940, and I got a message that I had to go up to see the Director of Scientific Research at the Admiralty. About this time, I'd been particularly obstreperous about these experiments I was doing, and I was trying to get out of the Admiralty. I said, "Oh my goodness, I'm for it. This is it. <laughs> I've got to go up to see--" However, I was received in a very friendly fashion by the Deputy Director and he said, "We probably have a problem for the differential analyzer." And he went on to say that a magnetic mine had been washed up in the Thames Estuary, and that two young naval lieutenants had defused it and obtained the actual magnetic device that actuated the mine itself, which was a magnetic type fuse, and that the mathematics department at the naval headquarters at the Admiralty had been unable to solve these equations. And it was thought that perhaps the differential analyzer could handle them. What did I think? So they produced this equation and I said, "Of course, they can." So they said, "Would you phone Professor Hartree and have him clear the machine for you tomorrow morning? We'll have this material packaged and we'll have somebody go with you so that you're protected." And so I was a security risk at <laugh> this time. So I got Douglas Hartree on the phone and I said, "Could the machine be available in the morning? I will be coming on the night train," which I did. And I traveled with a chap called Leese, who was not only a good mathematician, but he was a fabulous musician, as well.

However, we got to the university and to Douglas Hartree's office, and I well remember that this was in one of these bags they produced that's sealed and so on, to be opened only by Hartree <laughs> and all this stuff. He looked at it, and he looked at us, and he said, “You go and have breakfast. You can get breakfast across the road, and come back in about an hour.” So, we did <laugh> and we came back and he said, “We won’t need the machine. I can solve it.” And he said, “If you’ll wait till the afternoon train, I’ll get the solution,” and that’s what happened. <laughs> So the Admiralty were duly delighted and surprised that Douglas Hartree produced the solution to that--.

Robinson: Without ever having to use the machine.

Porter: That’s right. And then interestingly, I mentioned Bullard as the guy who wrote the paper on the galvanometer being used to solve this secondary equation. It was Bullard who then took over the technical end of degaussing ships in the navy to avoid degaussing them as protection against magnetic mines that the Germans were then using. And the degaussing was based on Douglas Hartree’s solution of those equations. So the differential analyzer was, in fact-- it could have been used, but it wasn’t. But it was used later by me, because the real problem in fire control with the radar in those days was the noise problem. The signals were extremely noisy and smoothing them was very difficult. So I was trying to devise smoothing networks and I had various configurations to try out, and I did go up to Manchester and use the differential analyzer to solve the equations. So I went up quite a few times.

Robinson: Both the machine in Manchester and the one in Cambridge were used extensively during the war, not just by Hartree and yourself, but by a number of other groups, as I understand it.

Porter: It’s interesting on that question that a short time later, I came up with an idea for the Sperry Predictor. Now the Sperry Predictor operated in Cartesian coordinates, whereas the Vickers Predictor operated in polar coordinates. But the Sperry operating in Cartesian was more readily adaptable to cope with this noisy signal problem, because you see the difficulty with coping with noise, you can put in these filters, but then that delays. You can’t smooth anything without delaying the information, so it’s never up-to-date. If you smooth it, it’s always out of date. However, I came up with a scheme that did. It wasn’t too bad and worked fairly well with the Sperry Predictor. And my brother was at that time attached to General Pile who was Commander in Chief of Anti-aircraft Command, to his command and he had facilities available. So I got together with my brother, as a matter of fact, and these things were then called the Porter Smoothers and tried out in a few Sperry Predictors. But about that time, a little bit later, the American system SCR-584, I think it was, came in and replaced the whole lot. By the time the flying bombs came out, they were perfect targets for the American fire control system using 90 millimeter guns, and so the American systems were deployed so that all of the stuff that we did on smoothers was obsolete by then.

Robinson: Just tossed away. And they were perfect targets because they flew at a constant speed?

Porter: They flew in a straight line. <laughter> That’s right.

Robinson: First start out and the simple case and then deal with the more complex one later!

Porter: Sure. The only other thing I did which was of some interest, and that was when in 1940, London, the Blitz, of course, had taken quite a beating. And of course, Patricia and I were in London. But then the provincial towns like Manchester, Birmingham, Liverpool began to get bombed, and they had no radar. They had none. So the decision was London had to sacrifice some radar. And I remember the day when Blackett, they'd got together and decided that they'd got to change from four-gun batteries, as the London gun sites were-- about 40 of them were four-gun batteries-- to eight-gun batteries, thereby saving the radars, so you'd only need half as many radars and the other half could go to these other cities. And Blackett said, "Porter, where do we put them? Where should the eight-gun batteries be located?" And taking into account the characteristics of the radar, particularly of the radar, and that was it. And he gave me a guy, a young man, just graduated, a South African called Nabarro who later became a professor of physics at Johannesburg University. And Nabarro and I, after three sleepless nights, we got the plan which I promptly left in a restaurant, actually. I'd forgotten it and retrieved it about four hours later. <laughter> That plan was accepted. And that was the only bit during the war, I really felt I'd done something when I saw all the movement, military movements in London of these radar sets being taken out. And I thought well gee, there I'd been stuck at the Admiralty doing this useless work, and then I thought well at last, I've done something worthwhile.

Robinson: Well of course, the other very significant thing that happened to you at that time was that you did meet Patricia, and were subsequently married.

Porter: Oh yes, yes, certainly, that's right in May, 1940.

Robinson: So during the height of the fighting, of course.

Porter: Yes.

Robinson: At what point did you move to the Air Defense Research Establishment? I'm a little confused.

Porter: Well that would be in 1941, in the spring of 1941.

Robinson: Okay, so that was after you were married.

Porter: In the spring of 1941, I moved to ADRDE in Malvern. We didn't go-- We moved to Christ Church first, but then Winston Churchill gave the order that all radar research establishments must be moved from the coast, because he feared the Germans would retaliate after Britain had attacked their radar installation in northern France. And so we were moved, including TRE, the RAF radar research to Malvern. That's where Freddie Williams and I worked together.

Robinson: Ah that's where you met him, right, okay. What did you actually work on with him?

Porter: Well not really. It was more a social thing with the Williams at that time. It led to him being brought into the servo design field for radars. And he invented the so-called Velodyne, I seem to think, which was used extensively by the Air Force in automatic control of radars. So that was the link, but we became very good friends from there on.

Robinson: And of course, after the war, he went back to Manchester--

Porter: Yes.

Robinson: --and worked on the digital machines.

Porter: That's right, yes.

Robinson: Then after the war, you had a very brief stint at National Physical Laboratory, right?

Porter: That's right, yes.

Robinson: And you said at that time that you didn't believe that digital machines were going to be successful.

Porter: I certainly didn't on the ground of reliability, and had to go to Canada to be really-- it took a lot of convincing.

Robinson: Well presumably, it wasn't just your self. What were other peoples' views on that issue at the time?

Porter: Oh I think many people felt reliability was a very critical problem, and at that time, vacuum tubes, the reliability was not particularly good. So in other words, the transistor was a fantastic leap forward, <laughs> I mean undoubtedly.

Robinson: Right, right.

Porter: It changed civilization.

Robinson: You had very brief spell at NPL to set up a new group in the metrology division, do you say?

Porter: That's right, yes.

Robinson: In parallel with that, you'd also been seeking a position at the Royal College of Military Science.

Porter: Yeah. Actually, it was called The Military College of Science. It later became The Royal Military College of Science. Yes, that job, there were four faculties set up; the faculty of mathematics and ballistic technology, the faculty of technology, the faculty of mechanical engineering, and the faculty of chemical engineering, chemical warfare stuff. So there were the four faculties and I applied for the job of the technology, which covered electrical and mechanical instruments, and range finders and all that kind of stuff in those days. I didn't think I'd have any chance at all.

Robinson: It was a very senior position.

Porter: It was, yes. It was three levels above where I was at that time. So I didn't think there was-- But fortunately, my interviewer, the great author, C.P. Snow was very sympathetic. And I talked about the differential analyzer.

Robinson: I find it interesting that a great author, C.P. Snow was interviewing you for a post that was to run a technology college. <laughs>

Porter: Well he had graduated in the natural sciences at Cambridge and he wrote a great deal on bridging science and the arts. He was a great disciple of bridging that gap.

Robinson: So you got to know that you'd been accepted only shortly after you had already taken up a post at NPL?

Porter: That's right, sure. It was interesting. The day I got word of this, there was a little buff envelope. I had picked up the mail on my way to work that morning, <laughs> and just opened it as I was going. It just had Civil Service Commission, examination number 678472, candidate winning-- was it winning candidate it put?-- yes.

Robinson: Successful candidate?

Porter: Candidate-- or something like winning candidate, Porter A. <laughs> I looked and I couldn't believe my eyes, winning candidate, Porter A. <laughs> That's all it said, nothing else.

Robinson: And so of course, you didn't hesitate to accept that.

Porter: Or no, successful candidate, it wan't winning. It was "Successful candidate, Porter A"; <laughs> is what it said.

Robinson: So you didn't hesitate to accept that position.

Porter: No.

Robinson: How did you go about extricating yourself to NPL?

Porter: Oh that was tough, talking to Sir Charles Darwin, but he was very understanding, sure, I explained. And it was a fantastic job at Military College. I mean a wonderful house provided and everything. I had the rank of full colonel. I mean it was quite unbelievable. <laughs> There were officers aged about 50 of lieutenant colonel rank saluting me <laughs> And it was a great job. We got all kinds of opportunities. And of course, I built a differential analyzer, or had one built in the machine shop, and introduced the ideas there. And it was good.

Robinson: And specifically, using the machine as an educational tool.

Porter: Absolutely, just for education.

Robinson: Can you just speak on the issue of its value as an educational tool? I know that's obviously something that you've even recently been promoting from an educational perspective.

Porter: That's right, sure.

Robinson: Why do you see it as being so valuable for educational reasons?

Porter: Well it stems back to Vannevar Bush, that's what he always felt about the differential analyzer. It would be more important educationally than from a research point of view, and I felt, because it helped me personally, in perceiving problems, rather than a conceptual approach, say, to calculus and being taught by pure mathematicians, for instance, who obviously, approach it, conceptually, as you would expect, because they have not been involved in the applications of calculus in the real world. So when you look at it from the applications point of view, that it's a hands-on, and so you've got to look at it perceptually. And that's what the great thing about the differential analyzer; you can actually perceive a problem being solved. So you see there the wheels grinding out and the whole problem takes on a new dimension. So that's why I think it's so important.

Robinson: Right. So that machine really was used exclusively for educational purposes.

Porter: Oh yes, sure, teaching.

Robinson: I know you have a nice picture of that. I think that's the only documentary evidence that remains of that machine, if I'm not mistaken.

Porter: That's right, absolutely.

Robinson: We will capture that on the camera in a moment to get that. Were there any other significant things that you undertook at the College in this sort of area of computation and servo mechanisms that you'd like to mention?

Porter: Not really. I felt it wasn't quite the place for A. Porter, really, because of the military requirements that you've got the quartermaster's store sort of thing, and you think "where is it listed here?" A wonderful example, actually, I was doing some comparative tests on range finders. I thought this would be a wonderful experiment, not necessarily because of the value of optical range finders at the time, but as an actual experiment. Of course, at that time, radar range finding was the way to do it, so you wouldn't use optical, but there were several types of range finders which could give rise to a good experiment being carried out. And so I got two or three of these, and there was one I needed particularly. It appealed to me very much, and so I put in a request for this thing to the people that provided this kind of thing. I got a note back to say that this was not possible or something, for some reason or another. So I thought well I've seen these things, no doubt they exist and I should be able to get it. However, to cut a very long story short, I got hold of a vice-admiral and these particular range finders were obsolete. They were being dumped in the North Sea. I knew I'd seen some there. This particular friend of mine, who was a vice-admiral-- he'd being a member of this servo panel which Douglas Hartree was chairman of and I was secretary, that's how I knew him—and he arranged to send me, I think, 50 of these things, cleaned them up. After they arrived, I got them set up in the laboratory, and when this guy, this quartermaster saw it, I said, "Incidentally, what do you think of these?" <laughs> That sort of thing decided me that maybe my whole career was not going to be at a Military College. That was it.

Robinson: So you had achieved your objective of being in the teaching environment, but the other constraints made it unpleasant.

Porter: Yes. Well you know, it just felt that maybe I didn't want to spend my time arguing with military people <laughs> about various things. Another situation arose where we failed a very distinguished military officer, a technical staff officer. You see these people; most of them had service experience in World War II. I don't think we had any VC's, but we certainly had MC's and DSO's among the students. This guy was very distinguished, but he failed. And the administrative side of the college put up a terrific fight that they should pass, even though they had failed. We, on the academic side, don't go for that sort of thing.

Robinson: It's uncomfortable.

Porter: Yes.

Robinson: How did you get out of that situation?

Porter: Oh we did.

Robinson: No, I mean out of the RMCS to make your next move.

Porter: Oh well <laughs> I was a sort of unpaid consultant to Ferranti, and on a drive up to one of their plants in northern England, one of the directors said one of the questions they were going to bring to me was; who should they recruit to go to Canada to set up a research division, because funds were going to be available for research in Canada, so they were going to set up a research division.

Robinson: How had you establish a connection with Ferranti?

Porter: It was essentially through this servo panel I set up which was quite a unique thing. The idea was a joint idea between a fellow, Sullivan, who was a professor at Harvard, a professor of psychology-- no physiology, actually, and he joined me at ADRDE [Air Defence Research and Development Establishment]. The two of us put up this idea and it was accepted, so we got a cross services and industrial group to discuss problems in servo mechanisms. Which, of course, the Americans, at the time when I went on this mission, they thought this was incredible, the idea that we could bring in industry, and the Navy, and the Air Force and the Army all together, discussing these problems, <laughs> whereas over here, they were <laughs> keeping themselves secret from one another, and no way they would discuss them in the open, the idea that in Britain everything was being shared, and it was. So it was through that that I have very close ties with Ferranti and how I went to Canada.

Robinson: But basically, they were approaching you for suggestions as to who might take that position.

Porter: Who should go and at the end I said I'll go. <laughter>

Robinson: So that was a complete clean sheet of paper you got to set things up from first principles.

Porter: Absolutely, absolutely.

Robinson: Were there any clear ideas as to exactly what research would be done in this establishment?

Porter: Oh yes, completely. They got a pretty big contract from the navy for that DATAR [Digital Automated tracking and Remoting] project. This was a biggie, and this is where it just obviously had to be digital.

Robinson: So did the acronym DATAR stand for?

Porter: Oh gosh, I think it's in my book, actually. I can't remember. Anyhow, but it was effective. It was information handling for a group of ships so that all ships had the same information available at any time. They were all in the network, so it was fantastic. In fact, the American navy adopted it, but it was Ferranti, Canada.

Robinson: What year was it when that was made?

Porter: 1954.

Robinson: When it was deployed?

Porter: Well a prototype, yes.

Robinson: When did you actually first go to Ferranti in Canada?

Porter: Oh 1949. Yes, I was at Military College in 1947-- in 1949.

Robinson: So it was a fairly extensive development period involved in that system.

Porter: Oh yes.

Robinson: What were some of the unique features that you worked on there? I know that there were a couple of interesting technologies that could have applications outside of that system.

Porter: What do you mean?

Robinson: Well in the group at Ferranti, I know you described in your memoirs some very creative people there.

Porter: Oh well, of course. We invented the mouse basically, but it was regarded as top secret by the Canadian navy and, therefore, classified. People visited, you know from the States, from the service industries, so a lot of people saw it and there was no reason why they shouldn't adopt it.

Robinson: What were you actually using it for in the context of this system?

Porter: Oh well it was used for identifying targets, and it was a Canadian bowling alley ball in plaster of Paris mounting, air bearings, air blown, and so it was movable in all directions with little wheels, photocells, signals and exactly as the mouse works.

Robinson: A track ball, right?

Porter: So that you could put your arrow on a target, which might be a radar target, a sonar target, and then press a button and it would come up, all the characteristics which you'd have on your storage drum, your magnetic drum and that's it. The submarine depth so-and-so, course so-and-so, speed so-and-so and that's it. It would come right up. That's what it did.

Robinson: So there was a whole--

Porter: That was 1954.

Robinson: But there was a whole processing section behind this, presumably, taking the signals from this track ball and controlling that display.

Porter: Oh yes. The track ball identified the target then from the monitor to the drum, where all the information was stored.

Robinson: Was this based on an established digital processor, or was it entirely designed from the ground up, a custom processor?

Porter: I'll have to have a rest. I'm sorry. <laughs>

Robinson: Okay, I think we're at a point when we need to change a tape, as well, so this is a good point to stop.

<crew talk>

<camera zooms to small circuit module>

Robinson: So Arthur what is that little device you're holding there?

Porter: Well after the conclusion of the big naval information processing system, the so called DATAR System, we at the Ferranti Research Group we looked around for the next phase in our operations and it was obvious that we would be moving away from vacuum tubes. The DATAR project, each individual unit which fitted onto a minesweeper had 30,000 vacuum tubes, diodes.

Robinson: Wow!

Porter: They were massive computers, and so very clearly we had to move into the field of transistors. At that time individual transistors were costing about $80 each, these were the germanium transistors and we were dealing with the Texas Instruments people, and as I said seeking applications and there were two in particular we were interested in. One of them was mail sorting and the Post Office had a very, very far seeing engineer who really conceived this mail sorting system in 1955 or late 1954 and he came and discussed it with us and had a bit of money for a pilot project. So a small group, small team of my pretty big group which numbered about 100 at that time, took it on and together with this post office engineer and came up with a system which is pretty well what is operational today and this was these little units, circuit boards were similar to the ones used. This actually was presented to me in 1955, July when I left the company to take up my professorship at the University of London. But this was a typical circuit board, these are fakes, <points to individual transistors> but if they'd been for real they would be worth $90 each, these germanium transistors.

Robinson: And that was 1954 dollars right?

Porter: That was 1954 dollars, that's right and just imagine if you think of today where you take a silicon chip, I mean how many bytes can you put on say a thumbnail, a billion, at a cost of what, $10?

Robinson: Yes, yes, my daughter just bought a 4 gigabyte memory stick for $25.

Porter: Yes that's right, so just think of this fantastic explosion in technology, it's just been an absolute miracle. So this was the stage it was in those days. Unfortunately the Canadian government didn't go ahead with it and but fortunately the American government did and so at that time as I say I left the company, left Ferranti and the other circuit board here actually is an example of printed circuitry and they are on both sides the initials of my team. <camera zooms to circuit module> I don't know whether they show up in the photograph but they are the actual initials of my team, that's it.

Robinson: Now the circuit board technology itself must have been pretty advanced for the time.

Porter: Yes it was, it was, it certainly was yes, I think a beautiful job they did.

<crew talk>

Robinson: Do you have any other little stories we should capture before we wrap up?

Porter: Let's see, you mean relating essentially to the differential analyzer?

Robinson: Not necessarily, just anything that you think is.

Hendrie: Maybe you could just go through a little bit more about what you did after, when you went to London

Porter: Yes, I think a tale worth telling is also related to the Ferranti Group. After I'd left the company and become still a consultant-- when I was professor at Imperial College and one day I was visiting their laboratories of Ferranti in Edinburgh, and I spotted a young man who they didn't seem particularly interested in, and it turned out he had a third class degree and of course if you have a third class degree in Britain, you keep it quiet, <laughter> that's right Tim. So however on this particular day when I met him, his boss was away and he was given the responsibility of telling me what was going on in this laboratory, I can't remember any details now but I was quite impressed with the way he did it. So I asked Sir John, that was Sir John Toothill the head of the Ferranti Edinburgh establishment, if I could spend a bit more time with him, in fact I would delay my departure to London if that was okay. He said "Of course." So I did, I spent a couple of hours with Jack Cowan and I asked him would he like to come to London and be a graduate student, do something called a DIC, a Diploma of Imperial College which corresponded essentially to a masters degree. But because he had a third class honors he couldn't go for a masters degree, they wouldn't take a third class honors degree, but he could go for a DIC and he said "Well if he could get any funding." And so I said I would see to it that Ferranti actually paid his salary for the year if he came.

However I got him involved with many valued logics. At that time why I was interested in many valued logics I'm not sure except that I always felt this on/off business as I do still in all things to do with the law where the lawyers said "I want an answer, yes or no" what about when it's in between-- the gray areas. So I figured that maybe many valued logics may give me some angle on this. However so we did publish a couple of papers, however I won't go into the full details of Jack Cowan but I got him a fellowship to MIT against very tough competition. All the other candidates had PhDs but Cowan I got him. However Cowan ended up and I believe is unique in the world, --in the world-- with a joint professorship in mathematics and neurology at the University of Chicago, Jack Cowan. So that I reckon is one of my greatest achievements in my career, finding Jack Cowan.

<crew talk>

Porter: The other in that area was finding Norman Moody but it was quite a different level,. He was Freddy Williams' right hand man and I got Norman Moody into biomedical electronics and he did very, very well.

Robinson: Perhaps you could comment, Arthur, I mean I know that multidisciplinary sort of things have been a big theme of your career you've sort of been a big believer in bringing together from different disciplines.

Porter: Yes and that really flourished at the University of Saskatchewan where as Dean I had the wherewithal and funding to do it and then of course when I got to Toronto there was McLuhan and then we made hay and that's how I got involved being things like a chairman of the Canadian Environmental Advisory Council and Chairman of the Advisory Committee for Science and Medicine for the Montreal World Fair. But this was all related to my getting together with Marshall McLuhan that I had got the model and the metaphor lined up, that's what did it.

Hendrie: How did you meet Marshall McLuhan?

Porter: Met him at a cocktail party and thought I'd met a crazy guy, and absolutely crazy, and it took about four cocktail parties for me to decide that this guy's a genius, it took four, but then I was sure. Yes,I was absolutely sure that-- and we really embraced each other and I really, I think I was his closest friend and I was head at his funeral, the head, whatever they call them at the funeral.

Robinson: So when you got involved at that time, and you mentioned a couple, but you also chaired a Royal Commission I believe on some nuclear energy?

Porter: Yes on electric power planning yes that was quite an event. Went on five years and it was a big, it was a 23 billion dollar capital, the Ontario Hydro Corporation with the nine nuclear power stations eventually. So it was a big project.

Robinson: Dealing with some fairly powerful groups I guess, right?

Porter: Yes right, a lot of political pressure.

Robinson: I can imagine.

Porter: Right. However-- but, you know, and throughout I've had a great deal of fun and Patricia was so great, she was just a wonderful consort to have, who was supporting me at every move. Fancy telling a young wife aged 26 with a two year old child, we were going to leave London or leave the Military College where we were so secure in every way with our family backing us, that we were going to Canada. You know, that took some real courage on her part to do what she did.

Robinson: She coped with all of those moves <inaudible>.

Porter: Yes all of those moves, she was the architect of the move, took control and was fabulous yes.

Robinson: And right now you're actually engaged in putting together her memoir so that contribution doesn't go forgotten?

Porter: Yes right, absolutely yes.

Robinson: So did you ever reach a point where you felt "okay I'm retiring at this point", do you remember?

Porter: Tim, that's a good question, that's why I feel this thing CIM "Calculus in Motion" I still feel, you know, that really they've got to do this virtual machine at Waterloo, I feel so strongly about that.

Robinson: Well just say a little bit about, I mean for the last two or three years now you've actually been pretty active in trying to get something to happen in this field of using the differential analyzer as an educational tool. So maybe you could just speak to some of the projects that you tried to make happen there.

Porter: Well the Waterloo group now, I think is the top software group, certainly educationally in Canada. One of the top in North America certainly, they are the RIM "Research in Motion" company sort of that spun off from Waterloo so it's really a very, very fine institution and I, as I say, felt that the time must surely be ripe to get a virtual differential analyzer so you've got a disk and which costs you about $2 or $10 or something, you stick it in there and you can.

Robinson: There on the screen you can see all the things that you would have been able to see on the physical machine.

Porter: You can manipulate it anyway you want yes. That was maybe a three integrator, and that's all you need for, you know, to learn about differential equations.

Robinson: Now you've also been a big inspiration for the group at Marshall University that are building a physical one, they decided not to go the software route but actually build this thing.

Porter: Actually build it that's right yes. Vannevar Bush had a lot of things--.

Robinson: Well he had a little story about his technician that understood how to solve this problem that his math professor didn't if I recall, do you remember that little story?

Porter: Vaguely, was this at MIT?

Robinson: Yes, yes and I think he put it in his autobiography, just something to the effect that the guy who built the machine worked with it every day, had no formal math training but he had this feel for differential equations that very few people did just because of the experience he'd had with the machine and so he described an occasion when some professor came in with a problem, had no idea how to approach it and he took one look at it and said "Here's how you do it."

Porter: Yes you know my father was a bit like that, quite incredible the way he came up with this two cycle engine which he used kerosene and super heated steam and it worked, I mean literally he could light the whole of our house, it was a very big house which he bought for a song, largely because of the cost of heating and lighting the place and he did this on about a pint of kerosene a week. So all this superheated steam, but he did it, he just sort of intuitive. That's right. Well Tim, you know, at this stage what-- I think of the golf. I had about 13 golf lessons in the Summit Golf Course, north of Toronto and there was this wonderful Scottish golf pro and at about the 30th lesson he looked at me and he said "Well Mr. Porter, I think we've gone about as far as we can gang" That's just what he said.

Robinson: Well thank you so much Arthur. Obviously it's been quite a strain on your memory here.

Porter: Well, it's been interesting. Yes, I'll probably sleep tonight!

Robinson: I'm sure you will. Thank you.

END OF INTERVIEW

