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Interviewee: James Wilkinson
Interviewer: Henry S. Tropp
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TROPP:

This is a discussion with Dr. James Wilkinson. The date is the 27th of June 1973 and we're holding our discussion at Argonne National Laboratory while he's here on a short visit. Why don't we start off, as I mentioned, by starting with your own personal career and period, your university education, and then the wartime period.

WILKINSON:

Well, I went out to Cambridge when I was rather young. In fact, I think I was still the youngest man in Cambridge in my second year. I went up in 1936 to read mathematics.

TROPP:

You were sixteen then?

WILKINSON:

I was a scholar. That's right, yes. I was sixteen for a short while. I was soon seventeen, actually. I read mathematics. In those days we used to take prelim to part two and the math tripos in part two. This was a mixed, pure and applied math, which was general at Cambridge, and then you took part three, which was not necessary for the BA degree, but most people who were really much good at mathematics did, in fact, take part three, either in their third year, or, if they weren't scholars--I was a scholar at Trinity of course--if they weren't scholars, they would take it in their fourth year.

Now although I, when I'd gone up I was probably expecting I would be more an applied man. I'd always liked practical things. You know, as I sometimes like to say, I mended my own bicycle and that sort of thing.

But rather to my surprise, by the time I took part three, I was, worked in what was mainly the pure area at Cambridge then. So it was mainly classical analysis which would tend perhaps nowadays to be regarded as physics, almost among the purest things.

The, such algebra as there was I did and such topology, which wasn't very much in those days at Cambridge, I also did in part three. That would be—

TROPP:

Do you remember who some of the lecturers were in analysis? ...

WILKINSON:

In analysis, of course, they rather dominated Cambridge at that time. In part three I had Hardy, who was giving lectures on divergent series; Littlewood, who gave lectures from that little monograph of his, on real functions; and Ingham, perhaps not quite so well known, but worked on prime number theory and wrote a rather nice tract on Riemann's zeta function.

There wasn't a great deal of functional analysis and, of course, it wasn't so called in Cambridge. And such as there was given by Steen. Just, it was the only course available in part three. I went to that and we used Stone's [?] Operators in Hilbert Space.

TROPP:

Is this the Marshall Stone?

WILKINSON:

Yes. The famous big branch of Stones. And Philip Hall was lecturing on algebra, and there wasn't really much modern algebra in Cambridge, as you know, at that time. I always think that the modern trend in mathematics took root rather late in Cambridge. Probably because of the dominance of the classical analysis. You know, with Hardy, Littlewood and Besicovich, all three of whom, whose lectures, I attended, they still tended to be the big noise. Particularly at Trinity.

TROPP:

Of course, Philip Hall was a pretty big name in algebra.

WILKINSON:

Oh yes. He already was. But you know, we hadn't got much of a school yet in algebra, and if you consider the position that algebra now has in a course, you might say it was fairly rudimentary then. I mean, for instance, at the part one and part two level there would be very, very little algebra at all. Just some matrix theory. Perhaps more of the Bocher type, you see. So it's-- Anyway, that was my position in part three.

My supervisors--oh, quite early on I found it was classical analysis that really interested me. Whether that was due to any great aptitude for classical analysis, than any other subject, I would rather doubt. It was just the stimulation of the people there.

I feel that they hadn't, if I hadn't happened to run into those people, I might well have found myself doing applied mathematics in part three. But as it was, I didn't.

So that took me up to '39. I won prizes in my first year. There was a prize called the Pentathon [?] prize, which was for the most promising freshman at Trinity. I got that. In the third year I also got the corresponding prize and that was the mus[?] . And, of course, I thought then in terms of staying on at the University.

You know, the Ph.D. at Cambridge, particularly in mathematics, didn't catch on very early. And it was only just about by my time that it had become almost a regular thing, but not even then quite standard in pure mathematics. Because you would take a fellowship and then, of course, you had to write a thesis for your fellowship. If you were made a fellow at Trinity it was perhaps a bit [declasse ?] to take a Ph.D. Now in physics, perhaps it wasn't quite so much like that. But by my time, it had just about reached the time when one would have done and if the War hadn't come along, I think there is little doubt that I would have started on the PhD.

TROPP:

Do you remember taking the tripos? There was a good deal of controversy, I remember, during Hardy's period on that.

WILKINSON:

Well, yes. He always thought it was a thing to be got out of the way, that's right, yes. Of course, sometimes Hardy spoke with his tongue in his cheek. A little bit, he liked to exaggerate in that sort of way. But I didn't really start really serious mathematics. When you start, get past the examination, of course, is in the period of examination stage. Hardy, the series of Hardy, the course of Hardy's that I went to in part three, was Divergent Series and that was, he subsequently produced this book on divergent series which was based on that course.

I remember Ralph Boas was up at Cambridge at that time. He is now at Northwestern. Of course, Birkhoff had been there a little earlier before, and I've often spoken to him about the contemporary scene at Trinity. He tells me, incidentally, that he was in, very important this is for the record, that he occupied Jellico's room, which now is more interesting than it's ever been before. And actually Jellico was one of the first people I met when I went to Trinity, with a strict contempt with my year.

Anyway, the War came in '39. So that by the time it came to go back to Cambridge, for my fourth year, I was just twenty when the War started, and so that was a very vulnerable age. Cambridge was in chaos. I was trying to start, you know, on my post-graduate work. It was really rather difficult to make any arrangements.

Besicovich would have been my supervisor. And he had gone to the States. Everything was really rather a mess and it soon became pretty clear that at the age of twenty, I wasn't going to be allowed to stay at Cambridge for three or four years and quietly take a Ph.D. in classical analysis.

If I had stuck it out too long, I probably would have found myself in the Forces. Quite early on, I think we had, the Government had quite an enlightened policy. It's nice to give credit when it's due. We were very fond of criticizing people at that time. Quite soon they started to encourage scientists to go into Government Research Establishments. So, round about the middle of that term I could see that this would probably be the strategic thing to do. The Ministry of Supply opened an outstation of, associated with what afterwards was called Howstead [?].

And this was at the Maths Lab in Cambridge by a remarkable coincidence, where Wilkes subsequently went after the War. I believe Wilkes was appointed, had some connection with that Lab. You see, it would have just started—

TROPP:

He had some connection with it about that time.

WILKINSON:

It would have just have started. The War came along and sort of inhibited it from being set up as the University—

TROPP:

But they had space allocated and some budget.

WILKINSON:

That's right. But he never took up a position there.

TROPP:

Not until after the War, when he really was Deputy Director or Assistant to the Director or something.

WILKINSON:

That's right. Leonard Jones was the person who—

TROPP:

Leonard Jones was the one who founded it.

WILKINSON:

That's right. He was the one that I worked with. So, Leonard Jones for a while, remained half time professor, and was the Director of the Computer Lab.

Well, it was very nice staying at Cambridge. You know, I rather welcomed getting a job there; but the setup at the Math Lab at Cambridge was really rather a poor one. We hadn't really got anything very definite to do and I think of all the places one might have gone at that time that was rather a poor choice. I sort of, well, I wasn't to know how poor a choice it was before I went there, and the attraction of being at Cambridge sort of made me jump for it.

TROPP:

Did they have the differential analyzer set up when you first got there?

WILKINSON:

I went there on January the l, 1946. Few people went there quite early on. Goodwin, for instance, and Crank went there almost immediately the War started. I suppose, perhaps, September of '45--sorry, sorry.

TROPP:

You mean 1940?

WILKINSON:

When did I say I went there?

TROPP:

'46.

WILKINSON:

I went January the l, 1940.

TROPP:

Right.

WILKINSON:

January the l, l940.

TROPP:

Right, and I know Leonard Jones had been trying to get a differential analyzer.

WILKINSON:

Yes. The differential analyzer, I think, arrived almost immediately after I went there. The little Meccano model was there and Leonard Jones always used to love demonstrating this. He loved demonstrating the torque amplifiers and it was very intriguing. People would be asked to, you know, to run the wheel and there'd be the enormous amplification. A source of great amusement to everybody.

But my recollection was that the differential analyzer came, the proper, the big Bush one, came quite shortly afterwards. Well, it was nice being at Cambridge. I worked at the Maths Lab, and we began to work absurdly long hours, rather foolishly I think.

But I also used to give a lot of supervision. Cambridge began to get denuded of its lecturers and professors, and so people like myself who stayed in Cambridge, found it fairly easy to get additional work acting as supervisors, we called it at Cambridge. In fact, I supervised, first of all with Emanuel and then gradually it grew until I was supervising sometimes at Pembroke, and sometimes at Suitland Southex [?]. I enjoyed that. It was the first time I realized that, you know, I'd always intended to stay in the academic world, but somehow I had never thought any form of teaching would appeal to me.

But I did like supervision. Of course it's a very intimate thing. You either just had one person or two and so—

TROPP:

More of a tutorial.

WILKINSON:

It's always called tutorial at Oxford, but at Cambridge it has always been known as supervision for some reason. But the usual pattern was you started off and you set your lad a tripos paper and then you looked through his answers. But then you began to establish what sort of standard he was and it would tend to encourage him to ask you questions or even try to get him to do questions.

I found it extraordinarily stimulating and being a sort of a young man who had just taken my tripos, you know, when the older people supervised, they were very fussy about what they were supervising. Besicovich would do questions in the classical analysis for me, but he wouldn't fiddle about with questions on astronomy

TROPP:

[Laugh].

WILKINSON:

or geometry or anything like that, you know. And as a young man I felt kind of on my mettle and so I liked to feel that I would deal with any questions in part two of the tripos. And so it even got that I could do questions in astronomy.

TROPP:

[Laugh].

WILKINSON:

A thing I hadn't bothered to do at the time I actually took tripos.

TROPP:

Well, at the Math Laboratory, what kinds of computational problems were you involved with?

WILKINSON:

We were, believe it or not, mainly concerned with computing trajectories and a little bit of internal ballistics as well, but it wasn't very exciting work. And Leonard Jones didn't give very much direction, and some of us got very restless in trying to get elsewhere. But it wasn't easy to move from the place where you'd been placed.

I mean, in a sense, it was a kind of privilege to be able to stay on during the War, doing scientific work instead of being in the infantry, as I like to put it.

TROPP:

Well, what kind of equipment did you have besides the differential—

WILKINSON:

Mainly desk machines, a differential analyzer, and the Mallock machine, which was a linear equation solver, which was based on an inductance net, yes, it was an inductance network.

TROPP:

Is that similar to a root finder that Thornton Fry, I think, had built at Bell Labs, was it that kind of machine?

WILKINSON:

No. This is just a simultaneous linear algebraic equation solver.

TROPP:

Just a system of linear—

WILKINSON:

Yes. It was very, very sensitive. You were never allowed to smoke a cigarette in the room. If it didn't work for about six months they would always say it was because somebody at the Campus smoked, you know.

TROPP:

[LAUGH]

WILKINSON:

But really, it was rather a poor job and some people made desperate efforts to get away. Goodwin, I know, was particularly unsatisfied and finally did manage to get away. And then finally, after about 3 and half years, almost all of us, except those working on the differential analyzer, and they always had a more interesting time than the rest of us did. They did a bigger variety of problems. We all got moved down to Fort Halsford and that was very much more interesting.

Leonard Jones himself was moved down to Fort Halsford. Became Chief Superintendent of Armament Research there. We were all moved down there and we moved into a division that had the delightful name, the person in charge of it was called, Superintendent of Theoretical Armaments.

TROPP:

[LAUGHTER].

WILKINSON:

Which I always think is rather a nice name. He was a Professor Knott. He later became the--sorry?

TROPP:

How do you spell that?

WILKINSON:

K-N-O-double-T. He has been Cavendish professor until quite recently. At that time he was professor, he came up as a professor from Bristol, and he worked in the Government throughout the War. By that time we had a great variety of work. Knott was a very interesting man. His second in command was a person called J. W. Maccoll, who is quite well known in applied mathematics circles. You will find people like Garrett who have been interested in hydrodynamics still will know him quite well. He wrote a very famous paper with G. I. Taylor. Taylor & Maccoll it's always known as in the trade. On shocks, attacked, attacked shocks: conical shocks.

Well now, the main topics I worked on then were, thermodynamics of explosives, rather reluctantly, because it's kind of a--it was a very important and interesting subject--but it's really rather an inelegant subject. And, of course, rather different from anything I had been associated with. And fragmentation. Fragmention, I suppose, I did more of than anything else. In fact, I wrote a joint paper with Knott on it. It was known as the paper to end all papers on fragmentation.

TROPP:

[LAUGH].

WILKINSON:

I had an amusing experience there. About the second day when I worked on fragmentation they sent me out to Shewburyness [?] which is an isolated place on the coast of Essex, where they carried out experiments on shells and guns. And this day was to be dedicated to bombs, "bombs versus guns." That's what the exercise was called.

So this was to gain experience on fragmentation patterns and so on. And I, as this expert on the subject, was to tell them what experiments were necessary. Well, of course, I didn't, by that time, know anything about fragmentation at all. And they would bring along a bomb and these army guys would say to me, "now, where should we put it?" Well, the first time they did this to me I sort of showed my ignorance and said, "Well, you know, you know more about it than I do." But I could tell this didn't go down very well and they really wanted me to know more than that. So then I moved along to the next twelve and they asked me where the bomb should be put up, I said, "well, about 67 degrees," and just about the time they got it up, I'd say, "make that 65," you know.

TROPP:

[Laugh].

WILKINSON:

And that went over rather well and so we would explode this bomb. We'd set the bomb up and then go and hide behind the wall, explode the bomb and collect the fragments and see the damage to the bomb.

Oddly enough, later on some quite interesting work developed from this. And on later occasions when I went there, they would suggest where the bomb really should have been.

WILKINSON & TROPP:

[LAUGHTER].

WILKINSON:

Well, the main areas of course, were internal/external ballistics and fragmentation. Thermal dynamics of explosives. So from there I began to work on propellants and, in fact, got caught up in the work on the V1s and V2s. In fact, some of the early information about the existence of V1s came there and people did a great deal of work. But I was never associated with that. Though oddly enough my wife was. Who is also a mathematician, a few years younger than me, who joined us at Fort Halsford [?]. That was her first job in the War. She came just after I did.

But quite often some of the general work, of course, there were army personnel there and they would come in with problems that we had to solve. And one problem, particularly amusing looking back on it, which didn't attract much attention to me at the time, a guy came up, one of the army people, and he'd asked me to do several problems and, you know, it's amazing how difficult random problems thrown out by people like that, are to solve.

You begin to feel a little small because they ask you to solve some enormously complicated problem which would involve solving a system of nonlinear partials. And, of course, they had no idea, you know, just how difficult a problem this is or not perhaps too much of the concept. So, you know, your failures were fairly heavily sprinkled among your successes.

And one day I remember being asked if I could solve a set of twelve linear algebraic--simultaneous linear algebraic equations, you see. Well, I was rather pleased at getting a job that sounded so easy and, of course, "I know all about this sort of thing," I'd had a course in linear algebra, particularly in the terms in which it was taught that day, it seemed to me, it was just my line, you see.

So I went away oozing confidence to do these and then got into my room and, well, you know, instead of twelve by twelve listed written down on a sheet of paper, it suddenly looks awfully big when you sit down at one of the desk machines and start to do it. And, oh, I consulted the literature. There wasn't as much literature on linear equations then as there is now; and it wasn't very good advice. I remember reading Cramer's rule was the way to do it and deciding that it wasn't.

TROPP:

[Laugh].

WILKINSON:

Anyway, oddly enough I finally did it by ghastly elimination with what is now known as partial pivoting. A term I don't think had been invented.

TROPP:

Not then.

WILKINSON:

No. And the interesting thing about it, it was what we would now call mildly ill-conditioned. Only we didn't use such abusive terms in those days.

TROPP:

[Chuckle].

WILKINSON:

And we gradually lost figures as the numbers got smaller. As I say, I did this myself on the desk machine. And then I did the back substitution. The answers turned out to be of all the [?] unity, which was consoling because we knew that was the sort of size. And then I substituted them back in the equations, knowing that they couldn't possibly have more than six figures right, because the last pivot I'd started with ten figures and I'd lost four, so there were only six figures in the last pivot and that I was dividing by.

TROPP:

[Laugh].

WILKINSON:

But I continued to work with ten figures, and, to my amazement, the agreement between the left-hand side and the right-hand side, all the way down, was to the full ten figures. That sort of puzzled me, but in fact, it had an interesting story from the point of view it might [?] in connection with any equations. So I was a little bit chastened by this time, because I told you it had taken about fifty times as long as I'd imagined when I'd been given it.

TROPP:

Well, that's an incredible system that you ... had.

WILKINSON:

That's right. So I took them back to the chap, and he didn't seem to be as grateful as he was. In fact, there was a kind of indication that it had taken longer then I had suggested it might. However, in order to regain some of the ascendancy, I pointed out, you know, that the agreement between my solution and substituted in satisfying the equations to this remarkable extent. And he had to admit that he was amazed, but he said, "perhaps if you hadn't solved them with such damned accuracy, you might have done it in a more reasonable time."

TROPP:

[LAUGH]

WILKINSON:

Which I thought was a rather ungrateful comment. Anyway, that was my only encounter with numerical linear algebra during the War. At the time it made very little impression on me. We hadn't become pathologically worried about rounding errors and I didn't give it a moment's thought.

I later on became, worked a little bit on external ballistics, and in particular on the deformation of shells. And one problem that I did was a stick of TNT in an iron casing which was--RDX TNT--which was detonated, and I had to calculate the shape of the casing as the detonation went down.

Now this was a two dimensional partial differential equation in compressible flow with the shock wave. But a rather complicated equation of state, of course, because we have all these fantastic combustion gases which are formed behind the shock wave when you--Almost every conceivable compound of nitrogen, hydrogen, and hydrogen and oxygen and so on is in the detonation wave.

TROPP:

Just continuing along.

WILKINSON:

That's right. Well, it was done by the method of characteristics, which, again, on the desk machine, is a pretty chastening sort of process. That was really my first experience in solving properly a partial differential equation. I hadn't--partial differential equations, not from any satisfactory, not from any systematic point of view, I hadn't been taught at Cambridge. And I read Courant-Hilbert at that time, and was really rather impressed by it. Courant-Hilbert volume II, which I saw an early copy of it. It became available there. It wasn't actually the book itself; for some reason I didn't have the book. Anyway, I got interested in partial differential equations and read quite a lot about them then, and it seemed to me that solving partial differential equations by the method of characteristics by hand just wasn't the sort of thing that a person ought to do.

And that was the time people began talking a little bit more about automatic computing. Already that was, I suppose, late in '44. I didn't think much about computers in those days. It was interesting to know it was in the air and I thought well, you know, that was, some form of automatic computer would be the way to solve them.

But I continued there. The War ended, of course, the [VE] Day came in the beginning of '4-, early, fairly soon in '45.

TROPP:

May 8, '45. Yeah.

WILKINSON:

But we couldn't get a release then. And then the Japanese War ended in August, I suppose it was. And we couldn't get our release. I began to wonder whether I wanted to go back to the University. And at that time I heard that the Math Lab had been set up at Camb--at NPL.

TROPP:

Yeah.

WILKINSON:

Goodwin had gone there and Goodwin I had worked with, as I told you, for the three years, at Cambridge, in the Math Lab at Cambridge. We had always got on very well together and he wrote to me and asked me if I was interested in coming. And by that time I was interested. I must say that I didn't enjoy this sort of work at first. In fact, I ws just waiting for the Germans to stop messing about so I could get back to University and carry on, you know. But you can't go on feeling like that for six years. There comes a time when you have to face up to the fact and you might as well take an interest in it. Being perhaps a young and rather immature young man it did take me a while before I did become reconciled to it. But by the end of the War, I was genuinely interested in it. Particularly in the possibility of solving PDEs.

Then I discovered Turing was there. I don't, no he probably wasn't there by the time I discovered. So I decided to shelve going back to Trinity. The possibility arose again on the fellowship. Besicovich got in touch with me. But, well, there was one thing I had been out of classical analysis by now for six years. I began to get other interests. I was married and thinking perhaps I would like to start a family sometime. And actually it wasn't all that attractive to go back on a fellowship. Then there was the hope of going to NPL and working with Turing. All these things just about decided me against it.

But it wasn't easy to get away from Fort Halstead [?]. You see, it still had control of engagement. Perhaps one of the things that almost took me back to Cambridge was the fact that it was easier to get released to go back to the University. Particularly if you had already done, as I had done, about six years of Government service.

I decided to try to get away to go to NPL. They tried to hold on to the staff they wanted. Particularly the people who were reasonable qualified in mathematics, to retain some of them there. So they would say, well, you know, you are absolutely indispensable for the work.

TROPP:

[LAUGH]

WILKINSON:

And we would go up before a tribunal and I would say, "Well, you know, we are not really all that important," because I wanted to get away. Rather a silly situation arose, because quite soon, if I had stayed, the re-construction of the civil service was about to take place and then I would have become a permanent civil servant or an established civil servant. And of course, a time would have come when I would have had to stop saying that it wasn't very important work that I was doing and say how important it was so I could get established.

TROPP:

[Laugh].

WILKINSON:

And so the timing began to get rather tricky. I might find that just at the critical moment I would convince these people I wasn't really very much use to them, just about the time I would want to be established.

TROPP:

[Laugh].

WILKINSON:

Anyway, finally somebody managed to pull some strings at NPL. I think it was done via Snow in his central registry, and I think one of the reasons I was able to get away was because Snow had never been very fond of Leonard Jones.

TROPP:

This is C. P. Snow?

WILKINSON:

C. P. Snow, yes. Who had been in charge of the central registry during the War and I suppose was the Principal Civil Service Scientific Civil Service Commissioner after the War. Anyway, I got away and went to NPL. Fact is, there are other reasons, too, about the work on partial differential equations. The expanding shell case.

You know, we used to have exchange with, among other places, Aberdeen Proving Ground. We couldn't publish work during the War. We used to write Government Reports. And I, of course, wrote one up on my expanding shell and included some of the calculations and in particular, a drawing of the shape of this detonating shell, you see. Well, apparently they then did this experiment at Aberdeen Proving Ground and detonated just such a shell and they photographed it. And in due course, a report came back from Aberdeen Proving Ground showing the photograph of the expanding shell case, fitting really quite remarkably well on my curve. But there was one thing that they didn't know. In the meantime I found out I did it wrong. And I recalculated, and the recalculated correct calculation didn't fit anything like so well.

TROPP:

[LAUGH].

WILKINSON:

But it was...

TROPP:

[Laughing]. It was too late.

WILKINSON:

That's right. But by that time the War ended and then I got away to NPL and it seemed an awful pity to spoil it for want of another—

TROPP:

But, you see, it would be absolutely shocking to find the theoretical and the practical would fit that well. [Laughing]. It's an unlikely story. [LAUGHTER].

WILKINSON:

So I went there and now I suppose the interesting part of the story from your point of view really begins because, I mean—

TROPP:

There was one thing I was going to, to ask you about before you went on. Did you get involved at all in the groups of people from establishments like yours that went to Europe and, in particular, Germany, to interrogate scientists?

WILKINSON:

No. And I think probably the reason I wouldn't, I didn't, was because at that time I was struggling very hard to get away from one establishment to the other. So, I had at that time, a slightly uneasy relationship.

It was kind of a rather unhappy thing in a way, because Maccoll who by that--Knott left us the minute the War finished, I think, probably, shortly after VE Day. I mean, he didn't really want to work in a Government Department; he wanted to get back to being a University Professor, so as soon as the War finished you couldn't see his backside for dust. And that was that. And so Maccoll then was in charge. And it was he who was anxious not to lose all his people immediately. He was a permanent civil servant and he hoped to retain. He got really rather a lot of very bright people out there. It's amazing how many people did go to Fort Halstead [?]. If you look at where they are now, it was really quite an important group. So he was trying to hold on to them.

He always, I don't know, he quite mistakenly imagined that I wanted to get away because, you know, I didn't want to work with him. There was nothing personal in it at all, as a matter of fact. As a matter of fact, Maccoll was a very charming man. But I just wanted to go and work somewhere else for entirely different reasons. Just because I wanted to work somewhere else, that's all. So it wasn't the question you asked me.

TROPP:

I just--it was about whether you got to visit any of the interrogation?

WILKINSON:

No I didn't. As I say, for that reason. Quite soon, you know, I suppose Penney must, Lord Penney, or Penney, as you know, became associated with the bomb project in England, came to Fort Halsford [?] just about the time I was leaving. Probably before I left. He didn't have, then he began to have connections with people in the division that I was in, The Superintendent of Theoretical Armaments, and then he collected some of the best of them and later took them to Auldermasten [?] where they worked on the bomb. So the nucleus of his team came from some of those people at Fort Halsford [?]. He collected quite a number of useful people from there.

TROPP:

When did you actually join NPL?

WILKINSON:

So I finally got away in May l946. I went there and Turing was already there. Mathematics Division had been set up in '45, I suppose, Womersley was appointed as the first superintendent, probably about half way through '45. And then this was an entirely new division.

The idea of having sort of some central Government computing unit at, started to be discussed in the later years of the War, and there was good deal of in-fighting about where it would be. And finally they decided to make it one of the new divisions, to have it at NPL and to make it a new division. It was known as the Mathematics Division. I think, I don't know too much about the background of that fighting. I think Darwin was quite keen on having it there. I think Hartree probably wanted to have it there. Anyway, that was where it was.

TROPP:

Well, Darwin had an administrative role there, too, didn't he?

WILKINSON:

Darwin was the Director.

TROPP:

He was the Director of NPL at that time?

WILKINSON:

He was the Director of NPL, yes. And I think he—

TROPP:

And Womersley was the Head of the new, separate division?

WILKINSON:

Well, Womersley was, when it was decided to form the Mathematics Division, Womersley, who had worked as a statistician in, I think it was called, SR 17, which was a statistics group in the Ministry of Supply, which worked out in London, he was made first Superintendent; and he must have taken up his post probably in August, or something of that chain. He may have had the post before he went there because when Math was first formed there wasn't a building for it and they took over two old houses in Teddington Hall, which were on the periphery of the .. of NPL.

They were taken over temporarily by NPL and, needless to say, they are still there; like most things that the Government takes over temporarily, it has a rather different meaning in such a—

TROPP:

Well, to form a Central Computational Center, did they envision this as being a place where any agency, federal or private, could bring problems to?

WILKINSON:

That was the idea. You know, it was set up to do research in numerical methods, to give advice and it wasn't to be too much of a computing service. Though it was envisaged that we would do work from outside. It was felt that if a computing group was to make a proper sort of progress it should have contact with the physical world. It shouldn't do too much abstract. The term numerical analysis wasn't really in use in those days.

TROPP:

That's right.

WILKINSON:

They didn't want it to be too much of an abstract group, so they wanted people to go in who had contact with real physical problems. Now Womersley was put in charge of it, and he began to recruit his team. And he collected in particular a number of people from the Admiralty Computing Service. They formed this Admiralty Computing Service, which, certainly in later years, was at Bath. I don't know whether it had been at Bath right from the start. Goodwin had been there. Fox, who is now a professor at Oxford, and you, may have heard—

TROPP:

Wasn't Comrie there?

WILKINSON:

Not, I don't think at Bath.

TROPP:

Well, you said the Nautical—

WILKINSON:

Sandler was there.

TROPP:

Sandler.

WILKINSON:

Sandler, yes. Sandler, I suppose, was Head of the group. John Todd I think was there, not permanently, but—

TROPP:

He was part of the Admiralty group, that's right.

WILKINSON:

And S.W.J. Oliver, who is now over here and is a professor, a Research Professor at Maryland. He was also in that group. Quite a number of youngish, and promising--the average age of the Mathematics Division was very low, to start with.

TROPP:

You were all of twenty-six at that time.

WILKINSON:

Well, I was by no means a young man there. I mean, most people were younger, and Turing and Goodwin, who were in their early thirties, were really rather long in the tooth. We regarded them as the past tense. They had rather had their best day.

TROPP:

[LAUGH]

WILKINSON:

Certainly getting on. Well, at that time there was a Hollerith Section, that's a punched card section. The person in charge of that was T. B. Boss, whose, really, his main experience had been in connection with insurance companies and actuarial work. He took charge of the Hollerith equipment.

TROPP:

How do you spell his last name?

WILKINSON:

Boss? B-O-double S. Goodwin, who was in charge of the Computing Section with his second in command, was Leslie Fox. Leslie Fox, a very, very good desk computer. With a natural ability to use a desk machine and he achieved performance on a desk machine that not many people who haven't really experienced it would believe possible.

Olga also, by that time had quite a bit of experience. Now Turing was there. Turing had been persuaded to go there, I suppose, mainly by, it was Hartree and Max Newman, who had persuaded him to go there. He'd been, as we know, in his Foreign Office job during the War. That sort of finished when the War was over and with him the decision was again, as it was with me, was to whether to go back to Cambridge. Though, of course, he was a more established man. He had taken his Ph. D. and was already a Fellow at Kings. So it would have been more natural for him to go back to Cambridge. It was sort of an easier thing for him to do than for me.

But by this time he had already become interested in computing. And I always put this down to the fact that, well, he had this connection with computability, and so was interested because of his previous more abstract logical work.

Then during the War he had obviously worked a lot on electronic machinery and become reasonably knowledgeable about it, and saw the possibilities. He worked, he collaborated from time to time with people who worked in the Post Office. Two people known as Dr. (Coombs?) and Chandler whom you may have heard of in this business.

They had done electronic circuitry for him. They had a very high opinion of him. They, like most people who knew him during the War, always referred to him as the 'Prof', and in terms of very considerable respect, and affection, too.

So Turing was persuaded to go to NPL and quite soon he put up a proposal to the Executive Committee to construct the machine which was called the ACE, I think. This was probably Womersley's contribution and it stood for <u>A</u>utomatic <u>C</u>omputing <u>E</u>ngine, and the "engine" was certainly a tribute to ..

TROPP:

Babbage.

WILKINSON:

Babbage. And so that's why it was called the ACE. Not an unreasonable name, a satisfactory name to have for a computer, but the 'E' stood for "engine."

So Turing put up this proposal and he wrote a report for the benefit of the Executive Committee. At that time there was an Executive Committee at NPL which sort of advised on the work of the Division. And the member of it, I think, had to be Fellows of the Royal Society; NPL, in those days, was still controlled, response, it was still responsible to the Royal Society.

And each division had, mind you, it was usually known as the 'father' of the division, he was the representative on the Executive Committee, who took special interest in that division and gave advice, made suggestions and recommendations.

And, of course, the one associated with the Mathematics Division was Hartree. So, anyway, I came there in '46.

TROPP:

I want to back up to this report. Is this the report—

WILKINSON:

Well, that's what I--yes, that I should say. That report for a long time didn't see the light of day. Two of all the copies exist at NPL. Now quite recently, it was dug out and reprinted and quite a number of copies exist now, and I think there's absolutely no reason why you shouldn't have one.

It's a very hurriedly produced report. It's the background of the talk that Turing wanted to give to the Executive Committee. And remember, although these would all be scientists, probably only Hartree was all that well informed about it. And it's a very interesting account. Quite disclosive in parts. Rather amusing in other parts, Turing's characteristic style. And perhaps I could arrange to get one sent to you.

TROPP:

When we are through, I'll give you my card.

WILKINSON:

Yes. Well, it is an interesting historical document.

TROPP:

I would like to have a copy of it, very much.

WILKINSON:

It was never published and I think never copied until recently.

TROPP:

Has it been published?

WILKINSON:

No. But I don't think there's any restriction on it. Certainly NPL would take it rather unkindly if you did anything with it other than used if for your history.

TROPP:

Right.

WILKINSON:

Because if ever it did publish, officially, I'm sure NPL would feel that it ought to do it.

TROPP:

Or else it should be done with their blessing.

WILKINSON:

That's right. But they did produce these, it, as a belated NPL Report and it's that that I'll arrange to have sent.

TROPP:

Oh that's marvelous. Terrific.

WILKINSON:

Well, when I came to NPL, Goodwin rather wanted me to work with him because we had our old friendship during the War and he realized that I had become quite interested in mathematics, in computation. He always felt it was a good idea for somebody with a reasonably good background in classical analysis that they do best in such problems. And Turing was getting a bit restless, because he'd been there a while and hadn't really been given any staff. Now, Turing was not an empire builder, but he didn't want to work on his own. And so it was decided I would work half-time with Turing and half-time in the computing section. And with Turing it was called the ACE Section. So Turing and I were the ACE Section. One and a half people. Me half-time, Turing full-time. And my job was to assist Turing in the design, the logical design of the computer. To get out logical design specification. To work on design and development of programming work, on getting out procedures for the whole range of numerical analysis. And, of course, as that was only a small part time job, the rest of my time I was to work in the Desk Computing Section,

TROPP:

[LAUGHTER].

WILKINSON:

And get familiar with current techniques in numerical analysis, one would say.

TROPP:

How would you characterize the early conception, architecturally and philosophically of—

WILKINSON:

Well, it had very much the Turing stamp, you know. It's sort of a brilliant but untimely concept; of course, I wasn't capable of appreciating that at the time. I was just beginning to feel my ground. To me it was an electronic digital computer. I didn't have much to compare it with. Though, not long afterwards, I did see a copy of the early report on the EDVAC. I saw it, a rather faded mauve thing that, mauve kind of thing that came to NPL, and I did read it.

But I was a very busy man at that time, and Turing kept me fairly busy. Now, although I was half-time with Turing, in theory, Turing's not the sort of man you can really work half-time for. And quite soon it was taking almost all my time, and Goodwin didn't object to that.

But this arrangement did have one advantage. Turing was a great man to work with. Very exciting, very stimulating. But he did have his bad days. The days when he was really not a pleasure to be with, and at that stage I could always say, well I think it's about time I did a bit of work with Goodwin.

TROPP:

[Laugh].

WILKINSON:

You know, I'm supposed to be half time there. And I think Turing realized on those days that he wasn't really awfully good company, and I would go and hob-knob with Fox and Goodwin. But Turing's relations with everybody in the division were really very good, although he was not always an easy man. But everybody liked him. Everybody appreciated that he was a man of quite exceptional ability.

He began himself to be interested in numerical analysis and he had very original, well, characteristically original, ideas, as he did on everything. And I suppose Fox was the man who had most experience in numerical analysis. I suppose he was the only one of us who was a professional, in a sense. He took his Ph.D. with Selfwell, who worked on relaxation, and would almost certainly have been in that field even if the War hadn't come along. A thing that was probably not true of a single other man in the department. All of them had got into it as a result of the War. I'm not saying that they disliked what had happened to them, but they hadn't gone in by design and I think almost certainly wouldn't have got into such a thing.

TROPP:

Tend to think of relaxation methods as a post-l950 phenomenon, forgetting all the work that was done earlier. [LAUGH].

WILKINSON:

Well, ... Fox had already solved some problems which were quite difficult, for anything that didn't have a display on. He worked, as I say, with Selfwell. And he and Turing used to argue, quite good-naturedly. Turing loved to argue. He was very stimulating. Extremely, very good for the division, I think, at that stage of his work.

As regards his, to come back to his design. Turing was certainly a genius, but a very untimely genius, I always think of him. Looking back on it now, it was a really rather complicated design with a touch of, you know the term Heath Robinson ?

TROPP:

No.

WILKINSON:

Heath Robinson used to draw cartoons which always had rather far fetched machinery involved, you know. There was a touch of the Heath Robinson about Turing. He was such a strange man. He always liked to do everything from first prints, of course. And would only consult the literature really after he'd really thought about the theme a lot. He would use his own ideas.

I know he would set me a problem to do or something to develop and I would work on it and then take it along to discuss it with him. He wouldn't look at it then. He would then proceed to have his own ideas. And not until he'd done this would he read what I had done.

And I must confess at first this rather nettled me. But I soon realized it was his style and it certainly worked rather well.

TROPP:

You would say he would almost re-invent the wheel if he had to.

WILKINSON:

He would have done, yes. But, you see, what would happen was--first of all Turing was very much quicker, he was very much better at having ideas of his own than at following other people's. And I think he did find this necessary as a preliminary before going through it. And he almost invariably, I began to be rather amazed how he would always come up with some new angle on it. That I found rather impressive. Although he would do this, when he finally read what I had done, or what anybody else had done for that, he was quite appreciative. I mean, he didn't do this to belittle your work. As much as to say, I set you this problem but I could do **i**t a darn sight better myself. That wasn't the spirit of it at all.

But he liked, you know, just to have a go at it. I almost invariably benefited from what he had done, and quite soon I didn't mind. At first, I was perhaps, was perhaps slightly affronted.

TROPP:

It's like having a professor assign you a problem, and you turn it in and before he reads it, he solves it for you.

WILKINSON:

That's right. But there was always a touch of that developing around. No, it was an untimely design I think. But, you know, there's a lot, well, I've never known quite how much he was influenced by what was being done in the States at that time. Or even if at all. But the influence couldn't have been all that great because the whole style of his design of the ACE was really quite a lot different from what Von Neumann was doing. And was fascinating, but as I say, it had this sort of untidy streak about it.

TROPP:

Other than classified wartime secretive business during the War, did he visit the States between the end of the War and this period when you were working with him?

WILKINSON:

No. I don't know of—

TROPP:

So then, any influence he had would have been strictly through the literature?

WILKINSON:

Yes. I'm sure he didn't go the States until 1947. No, I'm quite certain he couldn't have done. We were very busy during that time.

TROPP:

Well, about how long did it take from the time he presented this report to the Executive Board and you started working on a design before you had something that you could say, here's a preliminary design for a—

WILKINSON:

Oh well, when I arrived there he had already got some parts of the design. I mean, there were lots of things he had, he'd not, and he'd never completed the design of an automatic multiplier for it. In fact, that was one of the first things that I worked on. He'd got some sketches of how it might be done. And it was one of the first things I did, as far as logical design, after I began to understand what it was all about, mind you, which was a week or two, because I'd never worked on anything like that at all. We worked entirely with logical design then. We weren't having any connection with circuitry at that time. Because, Turing's characteristic thing, you know, with his notation for trigger circuits and for various units with thresholds for one, two, three, which, I think you will find, he discussed in this report.

But I quite liked, enjoyed this. And I think I had quite an aptitude for the design of things in the terms that he liked to think it of. We were joined shortly after I came, perhaps two months afterwards, by Michael Woodger, whose name you have probably heard of, and incidentally, it appears on the famous Argonne l960 Report, the Perlis et al. You'll find Woodger is also one of those much more interested in programming languages now than I am.

He, right from the beginning, established rather a different relationship with Turing than I did. I liked Turing and I admired him and I liked to argue with him, quarrel with him sometimes. With Woodger, there was always the hero, a touch of the hero worship and it's still there now, I mean.

TROPP:

Turing is untouched.

WILKINSON:

Yes, yes. I mean, he knew he was—

TROPP:

Kind of a Newton effect.

WILKINSON:

Yes.

TROPP:

[Laugh].

WILKINSON:

Well, it was rather charming, actually, because he's rather a charming man, is Mike Woodger. Anyway, he joined the team as full time. So right from the start, in fact, I was detailed to sort of bring him up to date and instruct him in the art. And, as I said, although in theory, I was half-time. So, by mid-'46, perhaps, it would be, or a little later, there were all three of us, as a maximum team.

TROPP:

When did Harry Huskey join you? Was it about that time, or was it later?

WILKINSON:

No. So we went on and we, by that time, by the time I arrived, he had already done what he called, four preliminary versions. I think the first version I worked on was version five.

The others weren't sort of carried to completion and version five we almost carried to the specification, to full logical specification of a machine, apart from things like input and output. The logical control and the arithmetic units and the way the storage would be done and transferred and all that sort of thing. That was completed for version five.

And then we started, that was what you would call nowadays, a two, well, it's what you call really, a three address code. It would take too long to give the full flavor of what it was like, but numbers were sent from a source to a destination. And--perhaps with an arithmetic operation or logical operation. He was always very strong on logical operations. The logical operations always fully represented even in the earlier versions of the machine.

But, in addition to that, he always felt the machine, because he decided he had to use mercury delay lines, he always felt the machine wouldn't be as fast as we would want it. Or really like it to be. If you relied on mercury delay lines in the natural way and put consecutive instructions into consecutive positions.

Right from the start, it had what was known in England, or what we called, optimum coding. That is, instructions were put in such relative positions that the next one emerged when the previous one was finished. Now that made coding really quite hard, as you can imagine. And also made it rather untidy, because there were thirty-two words in each delay line and you put the first one there and if it took five minor cycles then the next instruction was five minor cycles later instead of being the next position.

So by the time you began to get towards the end of making a program, it began to get a little touch of the crossword about it.

TROPP:

As to whether you could squeeze it in [LAUGH].

WILKINSON:

You see, it was rather untidy in nature that was really rather typical of Turing.

TROPP:

What from, I guess this goes back to his wartime work and maybe it's an unfair question, but what, in the way of technology that you saw during that period, you feel he brought out of the wartime work, in terms of experience with delay lines or electronic circuitry. Where did you find him pretty knowledgeable—

WILKINSON:

Well, he seemed to know about electronic circuitry. Sort of in-between times he would teach me about, you know, flip-flops and so on.

TROPP:

He was very knowledgeable then?

WILKINSON:

Oh yes. That's right. But now I should come to one of the more unsatisfactory aspects. How was the machine to be built? Here we were going on with the logical design. And it was there, I think, one of the most unsatisfactory things about it all. The Executive Committee accepted that we should build it and I forget exactly how much money was put to, was allocated towards it, but by the terms of those days, it was really quite a large sum.

But, Darwin said, "well, NPL has never been associated with radar in any sense of the word." We had quite a number of divisions like the Bureau of Standards, but we didn't have any division that had any expertise in pulse techniques.

And he said, "well, during the War, these Government establishments been set up in radar. These are the boys, they know all about it. Clearly we should get one of these places .. to build the computer."

I guess Turing went along with this. Well that, I don't want to appear over-wise after the event, but even in my imperfect knowledge of administration and almost a dismal ignorance of electronics, I could never see that working. Damn it, it seemed to me either, if you try to get a Ministry of Supply establishment interested, either you would fail and they wouldn't be interested. In which case that wouldn't be very good. Or you'd succeed, in which case they would be interested. In which case, if they had any sense, they would have become interested and wanted to build it for their establishment. I could never see any future in getting this done.

As an interim measure, we had a contract with the Post Office Engineering--is it the Post Office Engineering Lab? I think that's what it's called, at Dulles Hill [?]. And as it were, these two people, Coombes[?] and Chandler whom Turing had worked with, or collaborated with, on the electronics side. They worked on trying to achieve some, not to build a computer, but to build some of the units. For instance, they were detailed to make a delay line re-circulate with the sort of performance we wanted, a megacycle pulse rate. The megacycle pulse repetition rate was Turing's.

TROPP:

Which you now feel is probably too slow?

WILKINSON:

Well, since we find it is, I suppose, he was justified in saying, always saying all the time, which we could.

TROPP:

I don't think there was any question but that you could. I think the question was how long it was going to take?

WILKINSON:

How much longer it would have taken than if we had set on something less ambitious? Anyway, the Post Office boys had a lot of other things to do. There are little bits and pieces. Didn't really go at a vast pace. They were awfully nice chaps. We used to go up and see them occasionally. Actually Dollis Hill is this famous place, which Turing ran. It was the other side of London and this anecdote, you've heard about Turing—

TROPP:

What about the story that Turing suggested using gin?

WILKINSON:

That's right. I think, I believe, it's mentioned in that report, that I'm going to let you have. Mercury was suggested, and Turing, I never knew quite how seriously, he said, a certain gin, gin and water, with a certain mix had certain properties that appealed to him. [LAUGHTER]. There is a discussion of delay lines in this thing and a good deal of the mathematical physics in this paper.

TROPP:

I'm anxious to see it.

WILKINSON:

Yes. Very, very sort of versatile, very, very versatile man, but the term practical isn't quite the term I'd use.

Max Newman has said that it's vital of him, always just a touch of the gimcrack and that seemed--Max probably was more fond of Turing and perhaps more attuned to him than anybody else. So he didn't say it in any unfeeling way, but I think he did kind of hit the nail on the head.

Well, okay, we begin to get well through '46. And I must say, I was already, by the end of that time, to begin to feel my way about the project and was extremely uneasy about the arrangements that were being made to build it. I couldn't see it. I was a little uneasy and by that time Turing wanted to have a thing with 200 long delay lines. That's right. Which would have been 200 long delay lines. They would have been just about l millisecond long if you like to use that term. Which meant that they would--we were working with a 32 bit word, so they held thirty-two words. Now, it seemed to me that was awfully ambitious. Particularly measured against the pace that things were going. It may have been an awfully nice idea to have a machine with 6,000 words, but it did seem to me awfully optimistic. Now, of course, I didn't know whether that was just because I wasn't--didn't know enough to know any better. But it did seem to me far-fetched.

TROPP:

I was going to say, to Von Neumann, the l,024 words contemplated on his machine seemed almost infinite.

WILKINSON:

Yes. 200 long delay lines, this was the intention, anyway.

TROPP:

That was incredible.

WILKINSON:

When I could see the time it had taken to get one in good shape, the adder didn't go along all that fast, with one thing and another. Well, it gets us to the end of '46. The team was still consisting of me, in theory full time, in fact--in theory half time, in practice, full; Turing, and Woodger. Then, I would say, almost, possibly the first or second of January--well, then, Turing then went off to a meeting at Cambridge, was it, in '47?

TROPP:

In '47 there was one at Harvard.

WILKINSON:

At Harvard, that's right. Turing went off to that. And so left me temporarily in charge of the other man, Woodger. And during that time, I would have said it was almost January the lst, I would have said, round about that time, that Huskey came.

He seemed an awfully nice guy and because I was there and Turing wasn't there, I was the fellow he met and we struck up a very good relationship. I began to tell him all about the logical design. He picked it up very quickly. He clearly thought it was a kind of rather untidy design, as you probably know from what he said about it.

And, well, we struck up really quite a close friendship. And then, I suppose something like two weeks later, Turing returned. And I introduced him to Huskey and left them together. At that time we worked in adjacent rooms. And soon I could hear that they were having a, quite a heated argument.

But that wasn't uncommon. I mean, one argued a good deal with Turing, because he was a very excitable man. He could be quite excited without it meaning anything at all. I mean, some people coming into the room sometimes and seeing us together might imagine we were about to knock each other down. [LAUGHTER]. But it would not be so at all. We were just arguing.

TROPP:

Purely intellectual [laugh].

WILKINSON:

Yes. So it might have been that way with Huskey, but I didn't know. But, my guess was that they really must have had more than just the sort of argument that Turing and I had together. I think that kind of, rather an unsatisfactory situation arose.

I remember Huskey coming back, and, you know, did go out for a bit and then we continued our work together. And my general feeling was that Turing and Huskey never again established what I would call a really cordial relationship. Harry might tone that down a bit. Maybe he didn't feel that way, but there wasn't really that smoothness about the relation.

Well, Harry, of course, he was, he had much more experience in electronics than I did. I mean, he'd even pushed valves into pin holes. Which up to that time, I hadn't done. And, of course, joking apart, I mean, he knew the ENIAC and was a much more knowledgeable fellow, and he clearly wasn't at all happy about what was going on. He felt that it was time that something a little bit more definite, a plan were made to do some experiments, and so on.

And he began to become interested in building a thing which was to be called the test assembler. And we began to do electronic experiments ourselves at that time. In the Mathematics Division we had a very small amount of equipment and Huskey was very keen on this.

Incidentally, just on the side, quite early on he realized that I was quite inexperienced in computing and we got very interested in linear equations at that time. It's a rather, entirely different story which we might do sometime. But anyway, he said, "well, you know, what about solving them on the Hollerith machine?" And so we did. And we--Fox, Huskey and I--spent quite a bit of our time for a while, solving systems of linear equations on a combination of Hollerith machines.

TROPP:

Essentially a multiplier and—

WILKINSON:

A multiplier and, yes, that's right. Just unit operations and it was done by handling cards. But we did solve systems.

TROPP:

a sorter and—

WILKINSON:

That's right. And it was several times faster than what we could de on the desk machine without being exactly electrifying, but it was fun. Turing, you see, that was rather an uneasy thing. I worked with Turing, but I had this, struck up this relationship with Huskey. Can you imagine it was not a very comfortable position to me. I have a kind of feeling that guests should always be treated, unless they ask for trouble, with some sort of respect. I found Huskey very easy to work with, actually. But there, you see, we would go off. Turing, he was funny, it was kind of a loose rein in some respects. We would go off to the Hollerith Section and do this. I mean, although it was sort of in his section, he didn't altogether disapprove, but he wasn't very enthusiastic, because it took a lot of our time.

Then, all these strands going at the same time, Huskey getting interested in building the test assembly. Now, here Huskey, I feel, was almost as unbalanced, in a way, in the other direction, as Turing was in the one. I mean, Huskey was only going to be there for a year, damn it. He didn't sort of get going on the test assembly until he was half-way through the year.

Now, it was kind of optimistic, considering he was starting with very raw material, I think, for Harry to imagine we were going to get a long way with the test assembly while he was there. But the test assembly was designed, based on an earlier version, probably version five or six, of Turing's idea.

By that time Turing had moved on to version seven or eight, which was a three address code, plus, because it was optimum coding, the selection of the next instruction, so it was effectively a four address code.

TROPP:

A four address code. [LAUGH].

WILKINSON:

And we decided for the test assembly we would be much simpler than that. Go back to his two plus one, and that we would make it a very minimal machine. It was just a test assembly in effect. Just about a computer.

Huskey would get more and more ambitious and began to order equipment and we began to sort of work on it. Now, round about August of l947, even Darwin began to see that things weren't going very well. And that the time had come to face up to the fact that we weren't going to get a machine built by other branches of either Post Office or by other parts of the Ministry of Supply. And we better do something about having it in the Electronics Section in NPL.

Well he wasn't prepared to form another division, even assuming he could have got support for doing it, if he'd made that decision. And so he decided to form a, what was called an Electronics Group, which was just to be a, sort of a small sub-section in an existing division.

The only division it could reasonably go in would have been up in the Radio Division, which at that time was under the control of Smith-Rhodes. So this division was set up. Now they did recruit, the person in charge of it was Thomas and he died a few months ago. And Thomas was really much more interested in industrial electronics. My impression of Thomas was, he always thought that the ACE was perhaps a harebrained scheme. But he was put in charge of this group and began to recruit some people, mainly from NPL, but one or two from outside. And the most important two people from outside he recruited were E.A. Newman and Clayden, who'd worked together on, a little bit on radar, but also at EMI on television. So, they knew, they had—

TROPP:

Who was the second gentleman, please?

WILKINSON:

Clayden.

TROPP:

K-L-A-Y?

WILKINSON:

C-L-A-Y-D-E-N, I think it is.

TROPP:

Him, I don't know.

WILKINSON:

No. He and Newman came in and they were really quite knowledgeable about pulse techniques. Probably even worked on delay lines at one time or other. Most of the rest of the team were drawn from other divisions in NPL and had had some experience of electronics, but not too much. Some of them really remarkably little.

And so there was this rather, rather raw group with Thomas in charge, who was anything but really an enthusiast for high speed computers. Turing went over to see ... Thomas and almost immediately they established a very poor relationship, indeed. Thomas was a rather cocky, self confident sort of man. Could be quite likeable actually, but the last person in the world to get on well with Turing. And to say that they didn't hit it off, I think, would be the understatement of all times.

TROPP:

[Chuckle].

WILKINSON:

So there was beginning, really an uneasy alliance. Huskey had been pushing on, certainly without Turing's explicit approval, and, I suppose, underlying disapproval with this construction of the test assembly. With people like myself and Woodger and one or two mechanics working on it.

And we continued on that until about early November, when Thomas, who was the person in charge of the Electronics Group, felt, I think not unreasonably, that he really didn't like this sort of thing going on when he was in charge of this group. And, as far as I can remember, as far as I knew--I didn't think much about the higher echelons of administration in decisions of that type--I suppose what he really did was he complained to Darwin about it. That if he was going to be in charge of this project, by God, he was going to be in charge of it. And the work on the test assembly was stopped. Probably about six weeks before Huskey had to leave. So that was kind of a miserable blow and I guess Huskey did get rather dejected and a bit--well, I think he enjoyed his year at NPL in some ways, but he must have felt dissatisfied. Maybe he's—

TROPP:

He hasn't said anything.

WILKINSON:

No. I always thought Huskey was rather a good natured fellow in a way, considering that—

TROPP:

In fact, he talked about staying on at Cambridge after he finished this year at NPL, and he couldn't get permission from the Bureau of Standards to extend this period. He'd promised to come back there.

WILKINSON:

Yes. Of course he did used to go and visit with Wilkes during this time. So there we were, this test assembly. The test assembly is rather an important thing, in spite of the way I describe it, perhaps as a fiasco, which, I suppose, it was.

Well, then Turing decided to go back to Cambridge. I wish I could get the date of that exactly right. So he went back and there I was left with the three people: With Woodger, who had been there all the time; and by that time the Maths Division we'd been joined by Davies, who now is in charge of the Computer Science Division.

TROPP:

What's his first name?

WILKINSON:

Donald Davies.

TROPP:

Donald Davies.

WILKINSON:

Yes. And Gerald Alway. A young man from Cambridge who had just taken his degree and was such a bright man. It has always been a mystery to me that the didn't want to stay at the University, but he didn't, and he came and joined us and a great source of energy, and very enthusiastic. Not an immature man, but he would do anything anybody asked him to do with great enthusiasm, but wouldn't initiate anything. Very odd. You'd say: "Design a multiplier." Wheee! Like mad, you know.

TROPP:

[LAUGH].

WILKINSON:

"Do something else, mathematical." Yes, he'd do it. But, somehow, he would never start a thing himself. I never understood why he was like that.

Anyway, Turing went back to Cambridge on a sabbatical something or other. It wouldn't be as much as a year. And I continued with this group, and I didn't get on all that badly with Thomas. I mean, I didn't have the explosive relationship with him that Turing had had. But he wasn't my sort of man, either, and I couldn't see him, he didn't, you know, it wasn't his game at all. I'm sorry. But the whole thing looked rather black to me.

By this time I knew quite a bit about multiple design, was interested in programming and the problems associated with it, and had this small group of youngsters, who were all rather good and rather energetic, and we started to produce programs. In particular, I wrote at that time, quite a large report on the ACE, copies of which still exist, which gives some description of the logical design of version seven or eight. And we programmed all sorts of things for it.

We did a complete linear equation solver, with all sorts of things added into it. Made up a subroutines of inner [?] products and things like that, which, I imagine, at that time, in '47, you wouldn't find anything, well certainly nothing more sophisticated being done in programming.

And a very interesting thing was at that time, Alway and I more or less did it together, we programmed the floating point. All the floating point operations on version eight. And it's very interesting that all the round-off procedures and everything we did then were the ones that were finally in the Pilot ACE. And then in the KDF9 that was with all the roundings with the sort of best possible roundings.

And they were copied, well mainly because Alway and I later designed the floating point operations on the Pilot ACE. And then the person from English Electric came and worked with NPL and finally incorporated them, exactly the same design, only now, of course, by hardware and not by programs, in the computer KDF9.

So, we continued on for that time. Turing went to Cambridge and while he was there, of course, we naturally thought he would go and discuss things with Wilkes, and were rather surprised to find that he didn't. And I discovered later when Wilkes told me with some [trace], I'm sure, of irritation that Turing had called in to see him on the day that he left Cambridge. [LAUGH].

So Turing came back to NPL and, of course, there was an understanding that since he had been to Cambridge and, I think, had been partly paid by NPL or perhaps wholly paid from NPL, that he would have to come back and work at NPL for some, at least a year, I think, otherwise the deal isn't on.

But he came back to NPL and was so dissatisfied with everything that was happening then. Thomas wasn't his man and so on, that he decided he would leave and go the Manchester. He found it a very hard decision to take and he oscillated quite a bit.

I remember I discussed it with him and I said, "well, I can see, I can understand your being dissatisfied with things at NPL. I'm rather surprised you are going to Manchester, because it seems to me you are well liked at Cambridge. You are a versatile man, you can do all sorts of things. Certainly, if I were you and I left NPL, I would go back to Cambridge."

I said that, though myself, if I had left, I might well have gone to Manchester, but I'm not the same sort of person as Turing. Anyway, he still wanted to remain with computers and being with one of the early working computers and he thought ... that he and Wilkes didn't have sufficient rapport for him to go back to Cambridge and work at the Math Lab. I think Wilkes would have been happy if Turing had been cooperative, but I don't think Turing wanted to do it. And so he went to Manchester. I think one of the most unfortunate decisions of all time.

Well, there I was. Inherited the team, and by this we were all rather dispirited. We had got interested in high speed computers and we didn't really want to abandon it. But we couldn't see how it could really end very happily.

Goodwin approached me and he said, "you know, you've inherited it. It's not a very flourishing empire," he said. "You might feel that you would like to get off and come into my group before some calamity occurs and you take the canned back for the thing having failed. You see?"

It was kind of a tempting thing. I mean, I wasn't all that committed to all that extent on high speed computers and I was interested in numerical analysis. But, I thought it over and I thought, well, I don't know, high speed computers really, after all, are very exciting work. Why not take a risk?

And at that time a rather odd thing happened. Thomas left and went into Unilever, I guess. His heart had always been in industrial electronics at the Lab. And they had to find somebody to head up the, this Electronics Section of the Division, and they chose a man called F.M. Colebrook Whose name you may have seen.

Now, Colebrook was an old radio engineer. A charming man. A most delightful man. Probably had been quite a good radio engineer. Was getting on in years. He must have been all of my age now,

TROPP:

[Laugh].

WILKINSON:

at that time, an old man, you see. He inherited this thing from Thomas, and, being no fool, he could see that it wasn't exactly a going concern. And after a while, he came over to me and he said, "I would like to have a chat with you in confidence." We knew each other a little bit. He said, "I feel you are the kind of person I can talk to." He said, "you know the ACE isn't really going exactly like clockwork, is it?" he said. So I admitted that I didn't think it was.

And he said, "I know you chaps over here got very interested in electronics and you worked on it when Huskey was here. You certainly understand the logical design," he said, "better than many of us do." "And," he said, "a lot of my people haven't done much electronics anyway. And so consider the possibility of coming over and joining us," you see, "and working together on the design of some sort of computer."

Well, kind of a shock when he said this, you know. I thought, well, either I get out of it or I must take some such opportunity, you see. Well, I mentioned it to the others and they were sort of even younger than I was, and no great sense of responsibility, I suppose, at that age, and they were all quite keen.

Really rather surprising because they were all mathematicians. Not electronic engineers, except to some extent, Davies, who had taken both a Physics Degree and then a Mathematics Degree. They were all very happy to do it.

Now, the position was, how would the Mathematics Division react to it? Well, Womersley, by that time, had become rather a weak Superintendent. This was mainly because of domestic problems that he always had. So he was a fairly easy kill. He agreed to this. A fortunate decision, I think, as far as I was concerned. I am quite certain a more resolute Superintendent would have said, "Not on your life."

TROPP:

[Laugh]. It was breaking up the Empire. [LAUGHTER).

WILKINSON:

Yes. "Besides, this is a crazy scheme anyway. If that's how you feel, you better get out and do some of the ordinary work, honest work in the Mathematics Division." But, he, by that time being in some ways in a rather weak position, in the division, he accepted it. Well, I went to see Goodwin and he said, "well, it's kind of a surprising thing to do, but I can see how you feel." And he didn't oppose it. Now if he had opposed it that too would have killed it. But, I guess, we were old friends, and he could see I had decided to do it, and so we went.

And the two groups joined together under the control of this chap Colebrook. Now, a funny thing with Colebrook. He never really got to understand the ACE Project at all. He was an old radio engineer. I guess he'd got to the stage when he didn't learn new things very easily. But he was a man of extraordinary honesty and integrity. Everybody sort of respected him. And anyway, even if they didn't feel all that respect for him, they felt he was a man without bias and had got his heart in the right place and we began to collaborate rather well.

The electronics people weren't too happy about our coming over, at first. And there was a period of about a couple of months when, perhaps, there wasn't the utmost cordiality, but it eventually broke down and we began to work together.

And quite soon the decision was taken to build, what then became known as the Pilot ACE. Although some people say that Huskey's thing was called the Pilot ACE, it wasn't. It was called the Test Assembly.

TROPP:

That's interesting, because the literature credits Huskey with the decision to build the Pilot ACE.

WILKINSON:

Yes. Well, it's just I think that probably Huskey himself may have forgotten. But he did call it the Test Assembly and it was known as the Test Assembly. But don't attach too much importance to this, because when we decided to do it, we said, well let's take up where we had been on the Test Assembly. So, we took up the Test Assembly and we designed a machine called the Pilot Act, which had, was very much the same sort of level of ambition. It was just a little bit more sophisticated altogether.

TROPP:

Which then essentially was version five or six of Turing's—

WILKINSON:

Yes.

TROPP:

series of computers [?].

WILKINSON:

But, of course, versions five and six of Turing's already had two hundred, they all had two hundred delay lines.

TROPP:

They all had to be toned down considerably.

WILKINSON:

So this was going to be an eight delay line affair. And what was the philosophy behind it? The philosophy was the same as Huskey's Test Assembly. This was to be a computer. I used to think of it in terms, by that time--I haven't talked about my own interest in computing, which had then become very much in linear algebra for rather peculiar reasons--I thought, well, what is a computer at that time? What's the time, by the way? I don't know how much of this you want.

TROPP:

Oh, it's about ...Oh, I want all of this. This is marvelous.

WILKINSON:

So we decided on this thing and I think, crudely speaking, you can say, how did we take our decisions? We would say, well, could we still use it and do something reasonable with it without this in it? And if the answer was yes, then we didn't have that, in it.

For instance, we said, do we need a multiplier? And the answer was, well with optimum coding we could multiply reasonably fast. One of the nice things about optimum coding, it may have been a nuisance to use, but we could, by a program with optimum coding, did a multiplication in reasonable time without, by means of a subroutine.

Although it might have been a little bit of a nuisance to make, the multiplication was rather important. So you didn't mind taking a few days making up the subroutine. And when we had done it it wasn't at all bad.

A divider, we decided we didn't need at all in order to demonstrate the feasibility of the machine. The logical operations we had in full. I don't know they were really justified. That was probably a tribute to Turing who would, of course, would probably rather have had the logical operations in than any of the others.

TROPP:

Of course.

WILKINSON:

So it had a full battery of those. And that was the only, perhaps the—

TROPP:

The only luxury. [LAUGH].

WILKINSON:

The only luxury, yes. So there it was, an eight delay line machine. That's an eight long delay line machine so it was 256 words. And a number of short delay lines. If I had time, I still remember the design of it quite vividly. But clearly I would serve no purpose in trying to describe it here.

TROPP:

Perhaps we could this at a later date. [LAUGH].

WILKINSON:

In fact, I even remember a good part of the circuitry, even now. So there we were, we decided on this thing. We hammered it out together. We all, we all--the logical design took place rather quickly, really. We didn't attach very much importance to our decisions. And it's interesting to recall that none of us--some of them forget, I have a fairly photographic memory for things actually, but I guess a lot of them forgot--but we never, I know we never intended to use it as a computer.

The idea was to build it to solve a few small problems, as inverting a matrix of order eight was the sort of thing that we thought we might just about manage to do with this thing. We realized that the programming to do it would be rather nasty because of the paucity of the facilities. The need for optimum coding and the, well the, all the onus was

TROPP:

was the arithmetic

WILKINSON:

all the onus was placed on the programmer. He was given the full load, for the simple reason that we didn't expect to have to program for it except for, sort of just to demonstrate that we were a group with ability and then we could get on and build a full size computer.

Well, what amazes me when you look at it and, you know, some might think it's kind of a conceited thing to say, but looking back on it, I think building a high speed computer was a surprisingly easy thing. Here was a bunch of amateurs, we were quite a clever collection of chaps, I suppose, in a way. But, damn it, most of us weren't electronic engineers. Only one or two had much experience. And having taken the decision to do it, with the logical design soon settled itself and greatly helped by the Test Assembly and sort of measure of complexity which was very, very, close to what that had been. And then we settled down after a few months of experimentation over in the Electronics Division working together, to do the circuit design.

We divided up the circuit design among ourselves. We all worked simultaneously on getting out the circuit design of these chassis. As far as the sort of engineering design, the actual layout and so on, I suppose we had a good workshop over there. Quite a, the workshops in NPL are quite good. Of course, they are much better at high precision work really, than this sort of bashing, that we were, paddle bashing and that sort of thing. And so perhaps some of the skilled workshop people regarded it as incomplete [?] the snobs that any of us were. You know how a good mechanic is. Especially when he has done the sort of work that they do at NPL. Some of it is very, very fine indeed.

Anyway that was done. There, I suppose Davies must have been responsible for a fair bit of that. We had a man called Aulty [?] who had come from the Engineering Division. He could have been a lot more use than he was. But he always really wanted to do a lot of the things by mechanical means.

TROPP:

[Laugh].

WILKINSON:

He could never really shake off his mechanical engineering. I think he really wanted to do binary/decimal conversion by mechanical means.

TROPP:

[LAUGH].

WILKINSON:

By the way, it didn't take all that long to do the circuit design. We fixed on a few, we fixed on our basic gates and trigger circuit design and so on. Those had--the actual element that we used, particularly gates and things, owed a lot to Newman and his previous experience, and I suppose, our use of what we called a long-tailed pair. I don't know what it would be called over here. It's an electronic term. That sort of dominated the concept, and in its own, quite good, and very reliable gates. We didn't use diodes anything like so much as in SEAC or, for that matter, no, the Cambridge machine used diodes, but it used bulb, tube diodes. But over in SEAC they used a vast number of germanium diodes. We used some, but not quite so many and perhaps it's just as well that we didn't use more, which reasons I will come on to later. Well, I suppose from the time we really decided that we were going to build the Pilot ACE—

TROPP:

Which was about when? This is in 1947.

WILKINSON:

No, no, no, no. It must have been well into '48.

TROPP:

Was it well into '48? That's right.

WILKINSON:

It couldn't be. I can't--it's hard to get an est--Turing--Huskey left at the end of '47. Then there was the misery of the last—

TROPP:

Yes, it must have been mid to late '48.

WILKINSON:

The final decision must have been late '48. And it began to go quite well. The chassis came along and we all designed them. Alway and I had designed quite a lot of the chassis. We always worked together actually. And we enjoyed the engineering design. The delay line part was done by Newman and Clayden and some of the old radio engineer type. We got more standard sort of a circuitry. I don't mean standard, but it was the old amplifying technique, not the pulse techniques, which came into it a great deal. And so much more associated with the radio engineers. Those of us from Math Division got very familiar with all the pulse technique that was needed and in particular the conversion from logical design to the mechanical design of all the things like adders and, later on, modifiers, but that's later story.

And it went very well and then when it came to starting to assemble it, for some reason or other, it was decided that Alway and I would do the assembly, and as the chassis began to be built, we assembled them. We had various test frames on them, but we began to put them into this rack and it had plug-in chassis. Very compact, small, narrow little chassis, which fitted in with Turing's pins, top and bottom, that sort of thing. And we started to assemble it and debug it.

And after a while, Newman joined us and the three of us worked on this assembly and debugging. And it went very well until about December '49. And then we came to put in the three **chassis** which were, what we call, the control. Because of the nature of Turing's design it cannot be compared quite with any part of a standard computer as it is now. But it's kind of the main heart of the thing, controlled the movements of the numbers involved in the circuitry, sequencing, and all that sort of thing.

And at that stage, the project stuck rather badly. You know, when those were working it was going to be very nearly a working machine apart from getting the few delay lines that we added in, one by one. And of course, as soon as we got two delay lines, full delay lines in, we could have, you could try it.

TROPP:

Is that analogous to the master control idea in the ENIAC? Is that a fair—

WILKINSON:

I didn't know that it was. I mean, it was a fully sequenced control with stored programs. So it's really very difficult to—

TROPP:

This doesn't compare to anything.

WILKINSON:

Not quite, no. Not quite. Anyway, it stuck badly for that stage and I suppose, for the first time we did get slightly discouraged. I remember young Alway getting a little discouraged.

And we began to work really very, very long days. I remember we used to come in about 8 in the morning and sometimes worked until well after midnight, standing in front of this thing. And it was very compact and it had no cooling in it and it radiated about five kilowatts of heat. So it was not exactly--like standing in front of a rather large electric fire. So it was really quite a grueling time, but, of course, very exciting. The feeling that we were nearly there was in the air for a long time.

After the early chassis, which went in like clockwork, with not very long--the testing period. And then stuck; in about April we began to get a little discouraged. But it was pretty clear in April that we really were very nearly there then.

And at that time, I suppose Bullard came to the Lab as Director. I don't remember exactly when he did come, but he hadn't been there, I suppose he had to sort of settle in, it so happened, the place where we worked was immediately below--he lived in Bushey House, and the building where we were doing our work was part of the old Bushey House. So he breezed in to see us.

TROPP:

Was he from Toronto?

WILKINSON:

From Toronto, yes. He had been Professor of Physics at ...

TROPP:

He was involved loosely with a similar project there, too.

WILKINSON:

Yes, but of course, it hadn't got--I mean, it hadn't gotten—

TROPP:

That was the UTEC Project.

WILKINSON:

But he had heard stories of the bad things that happened, and the general disillusionment. So, when he came to see me he said, "Well, how's it going?" I said, "Well, you know, it might well be working next week." And he said, "oh come on, you know, I'm not a bloody fool, you know." He said, "I know something about this project." I said, "no, no kidding, it's nearly there, you see." And, after a bit, I sort of convinced him, and he was very, very surprised.

So, this, in a way, was rather good, because it made a very good impression on Bullard. There was Bullard coming along, thinking perhaps that the project would finally have to wind up and he learned to his surprise, that after all there were no other machines going yet. By that time EDSAC was. This was by now early in '50. did I say '49?

TROPP:

No. You said late '49 was when you were starting the control.

WILKINSON:

Late '49. Yes. And so he was there, this was early '50. I don't remember when Bullard came. For some reason I don't remember.

TROPP:

It would be 1949, '50 period.

WILKINSON:

Yes. But anyway I was so wrapped with this thing by then I probably didn't even notice that a new Director came. But quite soon he came down and we started having this conversation, you see.

So I said, it's going to work and he began to realize it wasn't all that far away. Of course I was very anxious because it had stuck for a long time. We were all getting a bit lazy and a bit irritable. We didn't have any input or output on it. But then finally, we realized, we thought the control was okay.

Now we had no method of putting a program in it. We had one long delay line. That's right. And we had no way of putting a program into the machine except from binary off a set of 32 keys. One word at a time. And we had to set up these instructions and then put them into the delay line, into the, right relative position because of the optimum codes. We made up a little program. It was a...... You're not going to have enough tape are you?

TROPP:

Oh yes. I'll turn it over in a second.

WILKINSON:

We made up a little program which was called; afterwards it became quite famous, called "successive digits". What it did was it read a number in from the input switches. Added it into the accumulator until the accumulator over-spilled, and when it over-spilled it put on the next light, and we had a set of 32 outlet lights. So, then we knew that the lights would come on at a speed that was controlled by the size of the number. So if we doubled the number the light should come up twice as fast. So....

TROPP:

Let me turn this off and turn it over.

WILKINSON:

So we came to this point when we began to think that it probably was okay, it probably would work. And so we tried to put in this little program called the successive digits. Would read a number from the set of input switches. Add it into the accumulator once per (metercycle?). Test whether the accumulator had overflowed. If it had, put on the next light on the output (statisizer?) as we called it, you see. And so the lights would come on at the speed controlled by the size of the number. Double the size of the number and the lights would come on twice as fast.

TROPP:

You has visual control?

WILKINSON:

That's right. No real computing, but of course, just a minimum program that would just test whether the thing worked. And so we started to put in this program which because the machine was rather inefficient, took about ten instructions, which it probably wouldn't have done, you know, if it had been a more sophisticated machine.

And the delay lines, the amplifiers weren't really good enough. So the delay line had really rather marginal size pulses for the memory. We knew something had to he done to improve the amplifiers, but we couldn't do that at that stage.

And so we kept putting this program in and we would see it. We had been looking up on the (oscilloscope?). And every now and then we could see it dying. So we would keep putting this damn program in. It took a hell of a long time to put in those eleven instructions. No input other than this, you see. To be put in binary on the keys. We could see it keep losing it and I said to Alway, well, we will just have another half dozen letters on it and if it does, I can pack up and I can go home, you see.

So, about the third time we put it in and suddenly all the light came on, you see. Well, of course, it could have been a fluke. So the number was, and we turned them off and they came on immediately you see. And we said, the number is so great, you know, it's coming up immediately. so we put a small number up and one light came on and another light, and we doubled the number and it went twice as fast. Now, we said, let's put a three up and it went three times as fast. And it went like that for about ten minutes and then one of the chassis blew up or something and so that was that for the day. And so we went home. We knew it had worked. I mean, we couldn't get any doubts about it working.

TROPP:

Oh, that's marvelous.

WILKINSON:

And the next day, of course, it didn't work at all. But by the end of that week we had it doing (_________________?). Just ordinary sort of program, but it was very restricted because we found we hadn't yet put an input/output. Input and output was being worked on. Again, I think Davies played rather a great part in that. He's a very versatile man. Quite a good mechanic........(voice fades out).

TROPP:

Yes. Then on the input/output you had to have conversion from, or were you going to put everything in it, binary and take it out in binary?

WILKINSON:

At that stage we were, yes. We weren't in the early days immediately going to do the binary decimal conversion. So we added the other chassis and soon it began to work. We began to do slightly more sophisticated programs. Factorize numbers and then finally we put the input and output on. Worked out decimal binary conversion programs. Designed the program for solving linear equations. And the machine didn't work well enough to do it. [Laughter].

TROPP:

When did you finally get all eight lines going?

WILKINSON:

Oh, probably by about July I should say. I wouldn't like to be certain. Anyway, so the next day I told Bullard it had worked the night before and he said, yes, well that sounds a likely story. He said, well, if you get it working again you tell me, you see. So we did get it working again and so I said, well, we better get Bullard straight away, you know. We would like to see this. Tried to get Bullard, and he wasn't there. So I said, it's just like that man, you see. The bloody thing, you know, where in the hell is the bloody Director, the thing will stop working before he comes. [Laughter].

At that moment, he was a very informal chap. He steps in through the window. [Laughter]. Bushey House, it has those big low windows in this old house, you see. And of course, he obviously heard this but he's not the, Bullard is not the sort of man to be very much put out by that.

And so we showed him and it continued to work. And he said, yes, it's not exactly making a reputation on a program like that, but I do know it's working, you know. [Laughter].

So, we continued and by about July it was fairly obvious the machine was coming along and Bullard, who wanted to push things along a bit faster, said, you know, well we must set up a day for a demonstration of the machine. And have the Press in, the popular Press and the technical Press and some VIPs and (they can spend the day at NPL?) you see.

Well, I was a little anxious about this because the machine wasn't really all that good and it was not exactly the soul of reliability. and we decided to have, the ordinary daily Press was going to come in first. Then the technical Press the next day. And we decided to have three main demonstration programs. Two very popular ones in which you could insert a number in decimal from the set of keys. A six figure number and it would give you the highest factor or tell you whether it was prime.

And we were going to invite the Press to guess six figure primes and so on, you see. And the other one was, you could put in a date. If you put 23.6.1927 or, if you like, 1700 or 1203 and it would immediately punched card it and tell you which day of the week that would be, you see.

People could give you Trafalgar Day and so on ....(voice fades out)... and that sort of thing. But they were kind of popular programs. They were then dangerous because it was very easy for people to tell in general whether it worked or not. You know, they give you their birthday and if they knew which day it was, so you would be pretty much exposed.

I mean, if you said it was solving a partial differential equation and, you had them by the short (hair?). [Laughter]. But, you muck about telling people their birthdays and that sort of thing, you're tampering with dynamite then. I was kind of anxious about this.

TROPP:

Hardy could have walked in and given you a six digit problem and the machine could have said no. [Laughter].

WILKINSON:

That's right. Anyway. we decided of course, we ought to have a serious program as well just to show that we hadn't spent money, the tax payers money on a silly bag of tricks. And so, we decided to have as our serious program, the tracing of a set of waves through a serious of lens.

Again, you could put in the information and we would punch it out. Now, by this time that was a program that you could hoodwink, certainly the popular Press. By just making it punch out cards I guess, we might have conceivably got away with a leg pull, but of course, we probably wouldn't the next day when people like Wilkes were concerned. Coming around and well, of course, rather jealous of their reputation and therefore would have been quite happy to see us succeed, but on the other hand, didn't want to be fooled about it.

So, it was rather essential this program did work. And then the agony set in. First of all, it was a very difficult machine to program for. Because of the way in which it had been designed. All the load put on the program, you see. And we made this program and it wouldn't work. The day came nearer and nearer and it wouldn't work.

And two days before the Press showed, it had never worked. I mean, the program had never worked and we didn't know whether we still had a valuable program or whether the machine wasn't really in good enough shape. It turned out to be the latter. And two days before Alway and I did some long work on control.

Changing a few things here and there and improving some of the tolerances. And then, two days before, for the first time it did this little [ ] you see. So, I thought, well it's not exactly the soul of reliability. These two days are going to be a real (muck you know?). One particularly nasty thing about it and that is the amplifiers on the delay line still weren't good enough and they were rather unstable.

In particular, we had plug-in chassis with spares. And this worked pretty well, apart from the delay lines. As they warmed up, the amplifiers would go through an unstable stage and so you needed to put them in and leave them for about a half an hour and then retune them and then they were okay.

So, the morning came and that worked during all three program beautifully. And the arrangement was that, give an interview to the Press upstairs and then we would get a warning and know that they were on their way down. Well, to get warning, about five minutes before hand, we got it and the delay line goes down straight away. [Laughter]. So we dash in the spare, tune it up and we knew we had it now.

Because always before when the delay line warmed up, you put your thing in, line it up, get your pulses out nicely and about ten minutes later as it heated, the whole thing would go to hell.

So, we were doomed now, you see. So, they come down and they throw prime number at us. It work like a charm. It works like a charm. We put in the birth dates program. Doesn't turn a hair. It does birthday as though it has been doing it all its life. [Laughter]. Wave tracing. Tracing waves like mad. They were there about an hour. We couldn't get rid of the Press you know. Tracing these blasted waves. We said, it's a miracle. Never before had the machine worked as well as this. It's easy, by a long chalk, its best performance to date, you see.

Finally, the Press go out of the room. And we said, God, thank God they have gone. So we, to take this delay line, we put the probe on the delay line to look at it. The most superb pulses we ever had on the delay line up until that time. [Laughter]. Everybody tells their hard luck story about the press show. With us, it was a good luck story. Everything absolutely ridiculously good.

TROPP:

Do you remember the date of that popular.....

WILKINSON:

December. It would have been early December of l950, yes. I'm almost certain it was December 1950. And so the next day was the technical Press. Still very good. And then I think we had the next two days open house when members of NPL could come around. Very good. I think the machine probably went down once and we had to change a chassis, but not when, at the time when people were there.

So there everybody had the illusion of a very good machine. Unfortunately, a lot of the people associated with the computer thought we had a very good machine. I said, well no, you know, the machine, it's just a coincidence. That machine still needs a lot of improving before it's a good working tool. And in any case, I said, this is not a computer which is really, which you can really do serious computing on. Well, this didn't go down very well with the authorities. You see the only computer in existence in England that was working and it could be used, although the Manchester machine had done this early thing, it wasn't serious working computer. They were working on building the thing, which I suppose they called (Madame?). But EDSAC was the only machine. There was no machine in the Government department.

TROPP:

The Ferranti was still in the process of being built?

WILKINSON:

That's right. And so they said, no, you know, this machine is to be used. I said, well, okay I can see, you know, it's a much more useful thing than you thought, but it is too difficult to program for this. To take it back to the Math's Division and start on a program of work. There are many, a number of modifications that are necessary before that machine is a viable machine. This caused a lot of trouble and probably the only time I really had a big row with [_____?], I mean that.

Womersley had (Ladhenley?) or certainly (Bynol?) was looking after the Division at the time and I think that he got the impression that I liked working in electronics by this time and really didn't want to come back to the Maths Divisions you see.

That wasn't really true. I mean, I could see myself stuck with a thing which was going to be hellish hard to get results with. [Laughter]. So, we finally, although it was a pretty tough battle, agreed on a minimum program of modification. We agreed to make a multiplier and add to it. And Alway and I designed it logically. We put the final touches on it. Then he went away and did the engineering design. He did it really very fast and we built it and we added it and we modified it a bit.

And then it was, in my opinion, it was still a pretty tough machine to program for. The optimum code to start with is always a nuisance, you see. I don't know whether you know much about it. It's quite a nuisance and it's still pretty well minimum machine.

TROPP:

Well none of the early machines were what you would call easy to program.

WILKINSON:

Ah, yes, but I mean, say compare it with EDSAC say. It was really quite tricky to use. Although it was quite a lot faster than EDSAC. In fact the ratio of its speed to EDSAC was rather faster than Wilkes was prepared to admit at that stage. Particularly on some things. The ratio on some things was rather higher then that. It depended on what... Being optimum coding of course, it was very hard to say how fast it was.

TROPP:

Yes.

WILKINSON:

Because there was no sort of fixed ratio. You could say that the EDSAC had a certain speed, but it was hard to say what the speed of the ACE was. [Laughter].

Anyway we did these modifications we took it back to Math's Division. Then it was pressed into regular use. We added some more delay lines. Finally going up to ten. Or was it eleven? And then it was pressed into regular use.

It worked fairly well. Perhaps the outstanding weakness of the machine, or I suppose by the standards of those days, I would go a little bit more that, I suppose perhaps very well. But the price of success was eternal vigilance and it began to get very worrying for people. Particularly for people like Alway and myself.

By that time we had become users and didn't want to become... It wasn't that I minded servicing, but you know, I wanted to go away and get my programs running while somebody else got the machine. Whereas I often found myself working on servicing and then when it got going, other people would use it and I would go upstairs and get my program ready just in time for it to break down again perhaps.

So it was kind of an uneasy life. But it was a remarkably successful machine. We got pressed into doing all multiple work for it. We did a lot of work for aircraft companies. It was then that I suppose the interest I developed in (_____________?) algebra was pressed more and more into service because we worked very much on (______________?) problems.

Then he requested that we would do work for Aldermasten and although we could have refused, we probably wouldn't have got away with it.

I remember Bullard saying, "well, do you really want to do these jobs?" And I said to him, "well, can you really resist it?" I mean, you can say you will now, but when it comes to the crunch, won't Paddy win? Oh, it went against the grain I think, for Bullard really to admit this, but I think he realized that this was true. And my view was, we either do jobs at Aldermasten on a pleasant basis or we did them on a less pleasant one, it seemed to me. But they were interested jobs.

To me, any job was interesting at that time. We just wanted to see what the computer was like and kept using it. So, in fact, we did a lot of work on the Montebello explosions and so on when we worked for Aldermasten.

TROPP:

Well this wasn't unique to your laboratory because this was true in many of the early computers in this Country as well as the first Ferranti that Canada bought. The atomic energy needs had great pressures and they utilized whatever was available.

WILKINSON:

Yes, that's true. Well anyway, I mean the machine did begin to work very well. We soon got (________________________?) in linear algebra. Things seemed to conspire to give us problems in that area. I had been interested in it before I went over to electronics. I got interested, particularly in (aero?) analysis. I'd read Von Neumann's paper and Turing had given his lecture. In fact, I heard his lecture once or twice. I never actually read his paper until later on.

And surprising really, we became rather established in linear algebra, sort of which, these things are progressive. Because we got the reputation in linear algebra, people sent us problems. So the disparity between our way to do linear algebra and other section, tended to grow in that rather unstable way.

TROPP:

Because this was a period when linear analysis was becoming a great center of interest.

WILKINSON:

That's right, yes. Well, after we had had it for a while, they decided, of course if we are going to use it as much as that, we better have some more store in it and so a magnetic drum was added.

Very compact little magnetic drum. But the decision was taken which I think made me rather miserable, they decided that one of the things that made drums so tricky was the head assembly. You know, you get all the heads. And they said, well we can make it a much easier problem if we have many fewer heads and have moving heads.

And so we had a moving head drum. Well, I'd always had a mistrust of mechanical things, at least a combination of them. And I was convinced it would never work. I was wrong, it did work and it worked very well.

TROPP:

It did work well? surprising.

WILKINSON:

Yes, it surprised me. But I think it made it harder to build really. I think it was easy to face up, I wouldn't know, I wasn't working on electronics at that time. I could be wrong about this. I was certainly wrong in saying it wouldn't work because it did. It didn't work at all badly.

TROPP:

At the point when you got the, what you now call Pilot ACE going, was there a thought at that time to building the larger scale machine.....(voice fades out).

WILKINSON:

Well, I was always keen on building a bigger computer. Because you know, it was, still put rather more weight on the program than you should. I mean, I programmed the other machines, SEAC and EDSAC and thought about programming other machines. Thought about programming the Von Neumann machines but didn't because they weren't going. They were appreciably easier to code for than ours was. There was no question about that. Mind you, many people who didn't use it, were always telling us we couldn't possibly cope with that machine. Actually that wasn't true. [Laughter].

It wasn't as difficult as it looked, but it was much more difficult to people who were not doing it than it was to people who were. But it was rather awkward and I was quite keen. I didn't want to go back and build computers any more. That was something I had enjoyed, but I regarded as a temporary phase, so ..... But I was very keen on getting on with the building of the new machine. I was quite happy to have a drum added and that extended the power of the machine enormously. But I didn't ....

TROPP:

How many words did the drum add....

WILKINSON:

Oh, almost immediately what.......

TROPP:

Somewhere in the order of ten thousand?

WILKINSON:

12,096 I think it was, on our first one. But of course, compared with what we had had before, which was ....

TROPP:

256.

WILKINSON:

256 originally, I mean, it's virtually infinite. And we made the transfer autonomous so that you could call for a transfer from the drum and continue working. And so, in some matrix work we didn't lose much time because we had the rows in that we wanted and the next one coming and so on and so forth. So, we could iterate with the matrix and provided the time it took you to use the row, was longer then the time taken to feed in the next one to the drum, which it was, it was quite effective.

Well, that was that and it went on for some time. And soon after the Pilot ACE was obviously a success, the English Electric Company felt that it was time it became interested in computers.

Bullard became interested in getting a tie-up with some industrial firm. We looked at several and English Electric decided that they would be interested in doing it and an agreement was made that they would begin to work with us.

And then, for me, the terrible blow occurred. English Electric said, well, here the machine has been very successful, we would like to copy it. Now, that just was the one thing I didn't want to happen. I mean, I didn't mind them having the Pilot ACE, but I didn't want several Pilot ACEs and I didn't want to go on using it for a long time. And so this decision was very much against my natural inclinations, but there wasn't very much I could do about it. And they decided to build it.

Perhaps even worse than that the group that decided to build it wasn't all that big and they took on two jobs simultaneously. One was to build a (_________?) simulator which was an analogue machine to solve the (________?) problem and to build the ACE. Engineered versions of the Pilot ACE. These were to be called DEUCE. But because it was rather a small team and because English Electric Company was of course, in the aircraft business, they were very keen on the (____ ______________?) simulator. It was pressure behind the (____________?) simulator. The (_____________ ____________?) simulator. The (___________________?) simulator really got the main share of their effort as it were. That was even more depressing. Anyway they did ultimately build the DEUCE and I suppose, I have to admit, it was a pretty successful machine.

TROPP:

Where did they get the (acronym___________?) from?

WILKINSON:

Well just the digital electronic something or other. The reason they called it DEUCE was because it followed ACE, or followed part of ACE.

TROPP:

Then they made up as they went......

WILKINSON:

Yes. I can't for the life of me remember what it was. The kind of full names they gave computers. But they built it. It was a pretty successful machine. I mean, it was an engineered version of it. They had a bigger drum on it, still with moving heads actually. And they sold, I think, 30 to 35, which I suppose by the standards of those days for the UK was quite good. It sold, I think its linear algebra package helped to sell it. By that time we had got quite a battery of rather successful programs.

Because of optimum coding, the ratio of our speed on linear algebra relative to say EDSAC or even to the Manchester machine, was higher than its general ratio say, then what it would be during a (________________ ________?) for example.

We had got this connection with various aircraft companies and also with physical chemists (_______________?) and lots of (_______________________?) problems. And that helped to sell it quite a lot.

So, it was a success but I still, I don't know, if I'd been told in 1948 that's when we finished the design of the machine, that all that year later they would be making engineered versions of the machine which really wasn't all that different, I think I would have packed it up. [Laughter].

TROPP:

Did you get involved at all in weather prediction problems?

WILKINSON:

No. We gave some advice I remember, but quite soon, by the time we would have had enough time to put effort on our computer, Met Office had machines, has much bigger, and for some time. And of course this happened in a few years at Aldermasten. Soon Aldermasten began to buy big machines and by 1957 it had much more computing equipment than we did.

TROPP:

I realize now that the questions that I have stored in my mind are going to last hours, but there are a few I just have to ask. And those are about your views and experiences with the other projects. I would like you to talk about Manchester for example, the machine that you saw going, what it did and who was involved?

WILKINSON:

Well, the machine I saw going.....

TROPP:

This was 1948.

WILKINSON:

In 1948 it was just a little machine using the Williams' store. It couldn't have had very much memory. I suppose just one Williams' tube. Since it was a serial machine, it didn't have the disadvantage that all the machines had over here that tried to use it. They tried to use it as a parallel store didn't they? Except in the Bureau of Standards which ultimately added it to SEAC.

TROPP:

Were you thinking of the Von Neumann machine and its project and they were all parallel?

WILKINSON:

Yes. And even SEAC, which I don't think so much, I suppose it is a Von Neumann machine some, but it didn't come direct from the Von Neumann machine. It was a parallel machine.

TROPP:

Right.

WILKINSON:

No, this I guess at that time, had one Williams' store, but I mean, Williams and Coleman they are bound to remember better than I do. But there it was holding its stored program and it could find the highest common factor of two numbers. And later on, I think it was used to establish (_______ ___________________?). In fact, I know it was because when Cambridge had that very nice International Meeting that they held in June 1949, shortly after their machine had worked, Max Newman was one of the people who attended it and he described what he had done with my same numbers and that had been done on the preliminary Manchester machine.

Now shortly afterwards, I used to go to Manchester from time to time, and I was very busy and they were very busy so I didn't bob up there every five minutes. But later on I saw a drum there too and this, they always used a much bigger drum than us and they used a nickel coated one. The nickel coating used to give a certain amount of trouble. I think it used to have little pin pricked holes in it and they used to have bad spots on it. And because of this, we decided we would have ours coated with magnesium oxide. Was it magnesium oxide? Yes, I suppose it is. Anyway, it's an oxide, I don't know, but it's an oxide coating.

TROPP:

I can check that in the literature.

WILKINSON:

Yes.

TROPP:

How about Booth's work at Birkbeck?

WILKINSON:

Well, the trouble with Booth's work is that I have difficulty in finding the time of it. You see, his first machine was going to be a relay machine but using drum storage wasn't it? I went to see that. I'm sure that wouldn't have been, that might have been comparatively early on. I could have been '48 or '49. But I never saw Booth's machine, I don't want to malign him. I doubt whether I ever saw it working at all.

I can't recollect seeing it do a thing. I certainly saw the Manchester machine working as a stored programming electronic computer in 1948. It worked when I was there quite well. They hadn't licked the shorting problems. So every now and then it would pick up pulses. Clodding, as it was always called up there. [Laughter]. Which they always seemed to regard as some benevolent act of God. [Laughter]. And they would blame it on the local trams and that sort of thing.

I very much admired Kilburn's work and I know when I think, Tom was always very grateful to me because in Australia they didn't seem to know or to believe that the Manchester machine had worked storing a program before the one at EDSAC.

TROPP:

ADAM came much, came much.....(voice fades out).

WILKINSON:

ADAM was quite, oh ADAM first worked later than the Pilot ACE, yes. But this hook-up, as Arthur liked to call it, did work in '48.

TROPP:

It was kind of a bread board model?

WILKINSON:

Yes.

TROPP:

Or would you call it more than that?

WILKINSON:

A little bit more than that, yes. Yes, a little bit more than that. After all the (?) number was something that.......

TROPP:

Oh yes. Even today we only know twenty three .... (voice fades out). [Laughter].

WILKINSON:

Yes and Newman wanted to do it. He couldn't have done it without it. No, don't belittle it. Certainly I was very encouraged by it.

TROPP:

I think some of the early programs.........

WILKINSON:

Because you see, I saw it at the time when we were just pulling out of that trough. And it was nice to know that at least somebody had got something going. It hadn't been too big a success story anywhere up to that time.

TROPP:

When did you first see EDSAC run?

WILKINSON:

Not until he had the (colloquium?) and at the time,....

TROPP:

Yes, about a year later.

WILKINSON:

A symposium, what you like to call it.

TROPP:

Yes, I know what it is.

WILKINSON:

It was in June....

TROPP:

I think they call it a symposium.

WILKINSON:

Yes. It was an international, although not very many American people. And that was not long after it had first worked. In fact the only demonstration they gave of it .....

TROPP:

It first worked in May I guess, of '49. That's right, I got the years mixed up. And then they had the ....

WILKINSON:

So, if you want the sort of facts, if you recapitulate, you have the hook-up working at Manchester in '48 probably in July or round about then. Cambridge machine working in '49 and the Pilot ACE round about May. Though if you want to make it a little bit more sophisticated, June/July.

TROPP:

Of '50?

WILKINSON:

'50 yes. When I went to Cambridge to the meeting, the only thing that was offered as a demonstration was the production of the squares of numbers and it was not performing very well at that time. Although it had performed better before, it didn't have a very good, they had a bad luck story and we had a good luck one, on ours.

TROPP:

Well I, when we turn the tape off, I'll tell you a funny, a similar story on Whirlwind. [Laughter].

WILKINSON:

It used to get the square 16 wrong for some reason or other. Associated with the pulse pattern that occurs with 16 which was still a bit pulse sensitive. Later on actually, I saw it doing things which I can't remember what they were, but they were real calculations.

TROPP:

Yes. Some of the problems that Professor Wilkes has described were pretty sophisticated.

WILKINSON:

Oh yes, and after of course, quite early, he encouraged his students to do, oh yes. I mean, EDSAC was a real machine. Of course the Pilot ACE, we did jobs just as sophisticated. A lot of things we did he couldn't have done. I mean we used to find (
?) of matrix of all to 64 while we only had eleven delay lines.

TROPP:

That's very impressive. [Laughter].

WILKINSON:

And you may be interested to know how we could do it. He fixed some punched tape as an input. We fixed on Hollerith cards. And how did we work with a matrix of all of 64? Well we had it on binary on cards, one per row. And to do iteration, the cards were read at top speed through the reader. And it would read the first number AIG and then it would use it in the gap between the two cards.

It took about six milliseconds to do all it had to do with its AIJ and multiplication, it read the tape and everything, adding input ...... And by that time, then it would wait, it would call for the next number and wait until it came. And so, there was one reading of the card. You did one iteration.

And what was the speed? Well it was 15 milliseconds between cards and it would take 6 milliseconds to compute anyway. And so it was about half as fast as it would have been as if all those numbers had been in the high speed store, which was not at all a bad speed. And so we could deal with asymmetric matrixes of that sort of order at a time when nobody else could have done, just for this very reason.

In fact, many of the peculiarities with the ACE, actually turned to rather good advantage. Although we had a multiplier, it was a very elementary one. Didn't even do sign corrections. So it regarded both numbers as positive and you had to compute the correction. But it was autonomous. You sent the two numbers there and said, multiply it. Then you could do what you liked.

And if you called back two milliseconds later, tht would be the product. [Laughter]. You could call back earlier, and there would be half the product. [Laughter]. And thereby hangs a tale.

It was later used to multiply short number together. Because you sent two short numbers to the multiplier and said, multiply and it started multiplying. In less than half the time, it would have the product there and you could take it off and you could send two more short numbers and the multiplier was still going.

So that in the two milliseconds that it took to multiply ordinary size numbers together, it would multiply two short numbers together.

TROPP:

You know, when we have time to talk about the real circuitry of this machine, I would be interested in the timing. The way you timed your operations. Because that has to be very fascinating.

WILKINSON:

Yes. So this was used enormously in crystallography where they tend to have rather short numbers. So of course, it was a very tricky program to read, to write. And you have to understand how the multiplier worked, and where the numbers would be, half way through. And how to take them out at the right time so as to get.....

TROPP:

The timing sounds fantastic.

WILKINSON:

But, this was done and of course, it then doubled the speed of the program effectively on crystallography. That made it really quite fast. I mean, on all the other computers you couldn't get any speed up by sending short numbers.

TROPP:

It was essentially then an asynchronous machine but with your programming you could almost do anything with it?

WILKINSON:

Yes. No, it was a synchronous machine. Various parts of it were autonomous.

TROPP:

Oh.

WILKINSON:

You see, it was clock controlled in fact. But when you set it to, you sent a number to the multiplier, into the multiple can, and then said, multiply. Okay, it started, and it would take exactly 65 millicycles, which was just a fraction over 2 milliseconds, and then it would turn itself off and there would be the product. And so if you liked to give an instruction any time 65 millicycles or later, there was your product. If you called for the answer at 32 millicycles you could feed the thing out of the accumulator but of course it was a halfway house.

In fact, it was very useful. Because it was so flexible, it was a very good thing for servicing, the multiplier. How did we service the multiplier? We sent two numbers there and we said, multiply. And then we fed out all the time and multiplication was taking place, the contents of the accumulator. Incidentally, we had the ability to transfer for long periods if we wanted to. Not just for one millicycle. So we could set it transferring all the time out of the multiplier.

TROPP:

You could watch everything as it was happening.

WILKINSON:

And it would go into a long delay line and then you could print out. And so you would not only see the final product, but all the things that occurred in the multiplier at every millicycle during it. All were available you see, because it was so unsophisticated you could do anything. [Laughter]. If only you knew how. [Laughter]. Really incredible.

TROPP:

Of course the environment of that period was interesting because we were talking about that conference at Cambridge and then later came their fortnightly colloquia. And during this whole period you were pretty well immersed in your environment.

WILKINSON:

Yes, not all of them had done, had worked as engineers to the same extent that we had. But it was quite common. I mean, Wilkes used the machine and he designed it too. You know there were lots of people like us about.

TROPP:

Wilkes once made an interesting comment about that period that you might want to react to. He said that one of the characterizations of that whole early era was the tension between the mathematician and the engineer. A kind of a feeling once the mathematician had said, here's how it, here's what needs to be done and why is it taking you so long to do it?

WILKINSON:

Yes. It wasn't so much tension in our place for that cause because we were more the same people. You see I was in the Electronics Division. Perhaps when we came back to the Mathematics Division that tension developed to some extent with the people who, the older members of mathematicians, who gradually came to use the computer.

They hadn't been associated with it. And of course, you did notice a totally different attitude that you had, even though I thought I thought of myself really as a mathematician, or at least as perhaps a numerical analyst by that time because I did see it as an engineer too. And I would make, you see, a skilled mathematician wants to use the computer, either the damn thing works or it doesn't.

I mean the fact that it doesn't work at all, the excuse that absolutely minute and rather unimportant, is not very impressive to him. It doesn't work. And so, Fox was coming along and he said, is it working? I said, well yes, more or less. And then it would turn out that it wasn't really working at all.

But I knew what was wrong and it was a very slight thing. It was going to take a bit (of a while to put right?), but I knew what it was. I knew that we would put it right. And I knew that when we had put it right it would work. And therefore I would feel that, you know, we were, when it was wrong and you didn't know what was wrong, that was a miserable thing. It might go on forever for all you knew.

But when you knew precisely what was wrong with it, there was a tendency to feel, you know, okay, here it comes, it's coming.

TROPP:

Okay. Suppose I'm a perfectly strange physicist or mathematician from another agency and I walked into your laboratory and I've got a problem. And you're not going to solve my problem for me. I've got to solve it myself. Are you going to hand me a manual or will you just give me a ....(voice fades out).

WILKINSON:

Well, that was really one problem you have with a machine that was as difficult to program for as the Pilot ACE. It made it a good deal more difficult for the really casual user to use successfully. But that didn't matter so much in our environment; so much as it would have done in the university.

Because we were all professional staff there associated with permanently. We had some programmers and we did jobs, when Aldermasten sent us jobs, we did them, they didn't

TROPP:

Yes. But if I really wanted to learn how, I was going to have to read the circuit diagram?

WILKINSON:

No, because you didn't have to use it in quite those fussy ways. I mean, such things as I described, as putting two numbers in the multiplier in order to get, oddly enough that was done by a crystallographer. He did become very fascinated. He's now at Oxford University and he's Leslie Fox's, one of his right-hand men, who is still an expert on machines. He's always remained an expert on machines.

TROPP:

I guess I'm curious as to how long it would have taken me to learn to program something on the Pilot ACE.

WILKINSON:

Not as long as people imagine. I mean, Wilkes would probably imagine that it took longer, much longer than it did. In spite of the fact that he know a lot more about it. I think that people that didn't have optimum coding, didn't use the Pilot ACE, always imagined it was much harder than it was.

I think Harry, probably Harry Huskey to this day, probably imagined it was harder than it was. In fact, anybody who used it much began to write programs really quite fast. I mean, I doubt whether I wrote, well as a matter of fact, I kid myself that I probably wrote programs a lot faster than those people at Cambridge.

But I was very interested in programming. As a young man, was full of enthusiasm. It didn't make much difference to me, but I could see there were easier systems and it would be a good thing to go over to them. And that after a while I was going to get fed up with it. My enthusiasm would wane. I would get an older man and the excitement of programming was going to wear off one day.

I could see that I had been in rather an exceptional position and was bound to be more interested than other people having been so intimately associated with it. Clearly it was a thing that had to come to an end and in a way, the sooner the better.

Soon we began to have automatic programming schemes associated with it. We had some very sophisticated matrix (package?) which was very easy to use, it was sort of early attempts at high level language, I suppose you might say.

TROPP:

They were solving the 64 x 64 system in that time period.

WILKINSON:

This was (________________________?), not just solving linear gradings.

TROPP:

I say, that kind of boggles my mind.

WILKINSON:

In fact, it was round about that time (________________?) first met me and he went there when I wasn't there and they told him that we did such, in fact the young girl who worked for me said that she was finding eigen values in the matrix, asymmetric matrix, in the matrix (_______________?) of 64. And I'm pretty certain (Ralston?) didn't believe her.

TROPP:

I don't think I would have at that point in time. [Laughter].

WILKINSON:

No. I don't think he believed her. After a while, when we got to know each other and I convinces him and I, when I began to tell him how we do it. But then he still was in the stage where we used to find (___________________________?) in our (
? and deflate them. It was still in the time when a lot of people thought deflation was unstable.

Actually, it was extraordinarily numerically stable if it's done in the right way. I knew it was stable. I hadn't yet proved why. Later I proved why and just how stable it was. It's interesting in those days probably deflation had one of the most evil reputations for instability of all things. But it does happen to be, if done the right way, and in fact, the way we were doing it, still to be one of the stable things in numerical (
?).

It's funny how, just as (___________________________?) elimination seemed to be suspected as being particularly bad. But it turns out to be really a more stable process ....

TROPP:

One another topic, the Von Neumann paper that you refer to I think made (dier?) predictions about (________________?) which were soon proved just the opposite.

WILKINSON:

The early paper he wrote....

TROPP:

Yes, the very first one.

WILKINSON:

The second paper he wrote he'd already shown, you know, that it was nothing like as bad as we thought.

TROPP:

Sure.

WILKINSON:

Things were actually slightly better than he said, or appreciably better. But that paper makes one of the decisive moves. There are other interesting things about that computer. Computing was strongly interactive. We had displayed on the cathode ray oscilloscope, we could look at the contents of any delay line and it had 32 words and they would appear as 32 numbers in binary on the 32 lines of scope.

And to show (__________________?) problems which we did by iteration, we used all sorts of techniques to accelerate conversions. These were all done by user participation and we insisted that it would be done by a way in which the operator did not play a critical part. You mustn't let the operator sort of compete against the machine in the sense that he's got to do a certain thing by a time and if he fails all is lost.

What we did, we had acceleration parameters. I don't know how much you know about the (________________________________?), but we had all sorts of acceleration techniques. And the amazing thing is, that although they are really quite sophisticated, you could teach intelligent young girls to use them very well.

I had one or two young girls who worked for me and what they could achieve by doing acceleration themselves was really quite remarkable. That's been quite hard to convince other people that they could achieve these things.

In fact, you know all sorts of people who were there were telling us that they couldn't be doing it. [Laughter]. It's rather amusing you know, this facet of human nature. How people who are not there and don't see it are so certain that something which is happening, really didn't happen.

TROPP:

Can't be happening. [Laughter].

WILKINSON:

Yes. It's a very odd comment, but some of the girls were really very good. Again, to me I must admit it surprised me. With their limited mathematical knowledge, they could cotton-on to what it was they had to do and do it and do it so remarkable well.

TROPP:

I'm going to shift gears on you and we'll close this off. When Turing left to go to Manchester, what role did he play in any of the computer developments there?

WILKINSON:

Oh, very little. He was a fairly generous sort of man. I mean, we would never have built the Pilot ACE if he'd stayed there. I mean, he always wanted to build something ambitious, but when he built, it was built and it worked.

There certainly weren't any sour grapes about it. I remember he came to see the demonstration and he came in to talk to me afterwards and he said how pleased he was and it was nice to see his ideas had come to fruition. And he congratulated me and the other people on what had been achieved. And shortly afterwards when I got promotion to a special merit post, one has to get recommendations from people outside, and it was usually considered a good thing to get recommendations from (FLS?) if possible, if he knew something about you, and naturally Turing was the obvious one to ask.

And I know, though I shouldn't do, that he gave me a very generous recommendation. I know because Bullard was a man who always liked to talk about (the recommendation?), so I know that he did. [Laughter].

And I think it was very characteristic of him. Oh yes, he was honest. He was a generous, honest sort of man in that sort of way.

TROPP:

Well, how, are there any other comments that you would like to make about Turing other than very personal ones, that you made to me .... (voice fades out).

WILKINSON:

Well I think really it was the extraordinary breadth of his knowledge was what struck me. And in spite of the fact that he has this great reputation, and I'm very pleased to see it now, it's funny in a way because he has that reputation in spite of the fact that I think most people don't realize just how good he was.

Because if you look at what he's published, it's only about twenty papers and that is if you throw in everything. I mean, some of the things are not what you would call papers. Now that's not a lot for a genius to write even if he does die at the age of 42.

But (______________________?) a number of papers on quite diverse topics, so you know you can see some measure of his bright-ness. But it doesn't do justice to him because he was much broader than that. He knew a devil more than math's over the whole spectrum of pure and applied.

And it was not only the mathematics, it was so sort of deep-seated. That's what I felt. You kind of felt that he'd always known it and if he didn't sort of remember it, he just knew it, you know. [Laughter]. It would be impossible not to forget that sort of uncanny feeling about him. The way in which he learned it, the way in which he mastered it, was different. This tendency to do things from first principle.

It was a handicap to him to some extent. In some ways, but also gave his work characteristic originality I think. Oh yes, he was enormous. Now see, he hadn't done much on numerical analysis. Though he had worked, thought about finding zeros at (?) to function before the War. There was a paper he wrote when he came back from Princeton.

But he became interested in numerical analysis and soon began to ask very, very pertinent questions about it. Now I can come back now perhaps more to the mathematical side.

When I got to NPL this sudden furor about linear equations and the extra-ordinary instability was sort of at its peak you know. And I arrived and every-one was talking about this. Turing and Fox and Goodwin and so on. So I hardly liked to mention my experience with the twelve equations I'd done during the War. And how the only thing that had puzzled me about them was the extraordinary agreement between the right and left hand side which was much greater than anything I could account for.

The climate was such that I didn't actually mention this because I felt that it might have exposed me to ridicule in the climate.

Anyway, Turing was an arch pessimist at that time, as was Von Neumann about the danger and at the drop of a hat he'd run through his error analysis showing how unstable it was, you know. And then finally, seven or eighteen equations came in which were considered to be ill-conditioned and we walked around them and argued about them and walked around them again. And then one day, I said to (Cross?), well why in the hell don't we solve them. You know, let's see this instability.

And we agreed that we would do them a joint effort sort of in serial. You know, we'd take on from each other so as not to make it too laborious. And we persuaded Turing to come in with us. But he was really rather scornful about this. He said, you know, well a waste of time, it's going to be very unstable, you see.

So we did it. It was a big of a mistake to cut Turing in because des computing wasn't his forte. Fortunately the checks, the row checks, made it almost impossible to make an error. But he did know that he kept making the error and so he had to keep doing the row again. [Laughter]. So we paid a heavy price for his collaboration. But we wanted him, he was the brains of the outfit, so we wanted to keep him in it.

Anyway, I did the final leg of which he did the back substitution and then substituted them into equations. And he got this extraordinary agreement which we now understand of course, but we didn't at that time.

And when Turing came in I said, oh, I've got the solutions, I said, and I calculated AX, how do you think it agrees with B? And he said, oh, it doesn't agree much at all. I said, it agrees with all the figures we started with. It's the (______________________?) and the ones beyond.

So he said, no, they are pulling your leg, you see. I said, no, they are. So he looked at them and he did one himself and he had to agree that they did, you know, admit that they did agree. And I think Turing was a little bit nettled by this you know. He was a little bit spiteful for a day or two you know. But you know, saying things, it's very characteristic you know. As soon as he knew that something happened that this big build up wasn't taking place, he set to work and he produced his paper in quite a short time. Really rather a fine thing, but never attracted quite the attention it should do.

Especially over here, it was over-shadowed by Von Neumann's paper. In England it was not so much overshadowed by Von Neumann's paper. I think the interest wasn't shifted, but I sort of shortly afterwards went over to electronics and got into an entirely different area.

And so, even with my interest, I didn't really come back to Turing's paper and I didn't appreciate it fully until I had done quite a lot of air analysis myself.

TROPP:

Did you ever get around to publishing anything on your early work in that wartime 12 x 12?

WILKINSON:

No. I just solved the 12 x 12 system and that wasn't much to put in a journal. Anything we did publish in the War went in Government reports.

I somehow didn't get into the habit of publishing very quick. I suppose, well I was pitch forked into the Government department before I'd done a period of research, so I didn't absorb the flavor you see. And then there we wrote Government reports and Government reports are rather different from published papers in a journal. When you do something you write up a report almost at once. It's not usually a very polished thing. It couldn't be published anyway even if you wanted to.

So I didn't sort of acquire a habit of publication. And then I got caught up in the electronics design, sorry, nautical design and then electronics design. Not regarding it really as my trade, but as an intermediate thing. Even if it had been something I could publish, I didn't think in terms of doing so.

TROPP:

You didn't get back to it until later?

WILKINSON:

Then when we got back to the Math's Division we were over-whelmed with this pressure to do jobs. Now, I gained enormously from doing all that (constellation?). I got a vast amount of experience of all sorts of jobs. I under-stood numbers and their relationships and saw (rounding?) errors and what they did and so on. But I didn't get any time to write anything out.

And in fact, I didn't start to write up until; I enjoyed that part of my life. I enjoyed the (?) team and we did computing and we worked very intimately together and I liked it. And then I was (married?). Although we did jobs for aircraft firms i.e. Aldermasten and so on, so we were very much a service group which was rather sort of frowned on by many of my mathematical colleagues.

I enjoyed it and then I got this special merit promotion which meant that I, I suppose as a graduate man you ought to be more strongly on the research side. And after I got it I started to continue as I'd done before and Bullard said, well look, you know as a special merit man you shouldn't really run the team in that sort of way. You should give more of your time to research and in particular to writing up some of the things you've done.

So if you look at my publication record you will find virtually nothing until '54 and then a continuous stream of papers.

TROPP:

Is Bullard still alive?

WILKINSON:

Oh yes.

TROPP:

Where is he located?

WILKINSON:

He went back to Cambridge and became a Fellow of Churchill and still is a Fellow of Churchill. He has some special post there. He was Director of the Geophysical Department I suppose it was for a while.

TROPP:

Wasn't he involved in the International Geophysical Program?

WILKINSON:

Oh yes. In the International Geophysical Program. He comes over here to La Jolla [?] quite frequently. I don't whether he's stopped now but.......(voice fades out).

TROPP:

He is somebody I should probably see if I get to Cambridge.

WILKINSON:

Went to the, what's the thing called out there in La Jolla ______________________?)?

TROPP:

Oh Scripps oceanographic?

WILKINSON:

Scripps oceanography that's right. He used to come over there every year, yes. He's a very likely man. I wrote on Bullard's problem too. He worked a great deal on these problems on (magnesia hydro dynamics?). Everything seemed to press us into doing linear algebra because he had big (______ _____?) problems associated with that.

TROPP:

Well I'm interested because of, as I say, before he joined you, before he became Director of NPL, he was the Head of the Physics Department in Toronto where there was also a Pilot project going on that ended up stopping pretty much when they bought the Ferranti.

WILKINSON:

Well when thy bought the Ferranti their problems didn't finish [Laughter].

TROPP:

Oh no. The Ferranti is an interesting story in itself. That's when the Government, I guess the Conservative Party, took power?

WILKINSON:

Yes.

TROPP:

And the Ferranti which was supposed to go to one of the Government laboratories became available because they lost their funds, and so (____ ____________?) was able to buy it.

WILKINSON:

I met Willy Watson there. He took over from Bullard didn't he?

TROPP:

I'm not quite sure who took over. When did you make your first trip to the States?

WILKINSON:

'54.

TROPP:

'54. It wasn't until really there were quite a few machines on the scene?

WILKINSON:

Yes. It was rather funny about that. I began to get invitations to come to the States. I was so wrapped up in what I was doing and I wanted to get more things done. Although I enjoyed the electronic (thing?), I could see it had held up the continuity of my career. Since I had given it all up and had no intention of doing it, it was kind of lost years in a sense as far as my career was concerned.

So I was very keen to get on to something and it seemed to me if I went to the States that would kind of interrupt things. And so I had several opportunities but didn't take them. And then at that time we used to have an exchange program between the Bureau of Standards and the NPL. From time to time we would exchange personnel.

And I think it was Huskey who made the suggestion that I ought to go and be the next person to be exchanged. And they wrote suggesting it and once again I said, well, no, I thought I'd rather not go. And Bullard, in rather a characteristics fashion said, well look here, Wilkinson, either are you going to go to the bloody States some time or not. [Laughter]. And if you are, it's about time you did it. And that was that. I decided I would.

TROPP:

Did you come to Washington or did you go to Los Angeles to the......(voice fades out).

WILKINSON:

No. I went to Washington for more than half of the time and made sort of the grand tour.

TROPP:

Did you get out to the Institute for Numerical Analysis or…

WILKINSON:

That was just (warned off?) then you see. But I did go out to Los Angeles to some of the people that were still there. The (SWAT?) was still being operated. Huskey had by that time gone to Berkeley actually.

TROPP:

That's right.

WILKINSON:

George Forsythe was there and Tompkins was there. Oh, I met quite a…although it had broken up, they were still with UCLA you know.

TROPP:

That's right. It was no longer part of the Bureau of Standards but.....

WILKINSON:

The Bureau of Standards, I met quite a lot of people who have become quite well known. But of course, the Bureau of Standards itself has you know, the Todds were there, weren't they, at that time? Phil Davis was there, the Bennetts were there, Alan Frost was there. They had a lot of very good people there at that time.

TROPP:

Yes. I guess Gertrude Blanch had left by then.

WILKINSON:

Gertrude Blanch had left by that time. I met Gertrude Blanch in England and people had a very high opinion of her.

TROPP:

I saw her just recently.

WILKINSON:

She had the right field and now what's happened?

TROPP:

She's retired and living in Los Angeles and said she's writing some........[voice fades out].

WILKINSON:

[...] Mina Rees came through with Courant in tow. That would have been round about, oh I suppose before '54, yes before '54 probably yes, because she won.......

TROPP:

Yes, she's retired now as the President of the University President that she had in New York. And that was an interesting period because by 1954 there were quite a number of machines in operation. And on the West Coast you must have seen all the 701s and the .....

WILKINSON:

That's right, yes. The 650s were sort of beginning.

TROPP:

Our program calculators were all over the place. [Laughter].

WILKINSON:

That's right. The 650s made me feel at home in some way because they had what they called, latency coding. Which was our old optimum coding.

A rather amusing story. I went to the Bureau of Standards and by that time I suppose my reputation was in numerical analysis as it was in linear algebra. And it turned out that although there had been a lot of people there interested in linear algebra, they didn't have much in the way of code. And so they said, well while I was there perhaps I would work on developing some separate things and routines of interest.

So I did and SEAC wasn't the easiest machine in the world to use, as a matter of fact, although it was somewhat easier than the Pilot ACE, not too difficult. And we used to program, do you remember well maybe you don't. They used wire cartridges. You would produce a program on a punched tape I suppose it was, and then you would put it on to the wire cartridges. It was rather a shaky process this wire cartridge. In particular, it was difficult to erase what had been put on there before. At least the erasing......

TROPP:

You are talking about the magnetic ....

WILKINSON:

The magnetic wire.

TROPP:

Right.

WILKINSON:

Not tape. Wire cartridge that's right. And you had to erase it twice before you used it. And you were given one, it was your own personal copy and it had all your programs on it. And I built up quite a nice little battery of things. And about a week before I left, you used to correct them on this wire, you know you feed them in and you used to use an instruction by instruction, testing of the things and you would correct them on the machine and so there was your tape with the good version of your program on.

And about, well it was a few days before I was due to leave, I was going to put another program. I had to clear a patch on the wire and somebody came to chat to me and when they had finished, everything I had got was gone. [Laughter].

It was only about half a wire full but it was such that I, if I wanted to use it I would have to wipe it off again, but it wasn't on there hard enough to feed into the machine. [Laughter].

TROPP:

Oh!

WILKINSON:

So at that stage I said to hell with it and I left the Bureau of Standards without a single thing to my credit. [Laughter]. But I don't think really that was the important thing.

TROPP:

I'm going to turn this off for now and thank you very much.

[End of Interview]