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12th June 1972.

Dear Don,

I enclose the final version of my Turing paper, prepared for last week's Machine Intelligence Workshop at Edinburgh University.

Since the paper was completed I have had an official reply from the Prime Minister, refusing to declassify the machine; the only good thing about his letter is that it constitutes the only known official admission that such a machine existed. I have had some comments from Stibitz, including:

"In view of a large relay "computer" for cryptanalysis, I wonder whether the ENIAC report (p.18) really decided the US government to support computer development - and whether everything in cryptanalytic machines "derived from ENIAC-EDVAC." The large "Madam X" cryptanalyser was initiated well before the War; I don't know its present state of classification, but there may be trouble getting information on it, even now. Nor do I know its completion date, but it was certainly several years before ENIAC.

I do know, from personal experience that the existence of Madam X had at least a small influence on the decision to develop the early Ballistic computers."

Have you any comments on this?

I gather from the Stanford contingent at Edinburgh that you did indeed manage to finish Vol. 3 before leaving for Norway. Congratulations!

With best wishes,

Yours sincerely,

Brian

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On Alan Turing and the Origins of Digital Computers

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Abstract

This paper documents an investigation into the role that the late Alan Turing played in the development of electronic computers. Evidence is presented that during the war he was associated with a group that designed and built a series of special purpose electronic computers, which were in at least a limited sense 'program controlled', and that the origins of several post-war general purpose computer projects in Britain can be traced back to these wartime computers.

INTRODUCTION

During my amateur investigations into computer history, I grew intrigued by the lack of information concerning the rôle played by the late Alan Turing. I knew that he was credited with much of the design of the ACE computer at the National Physical Laboratory starting in 1945. His historic paper on computability, and the notion of a universal automaton, had been published in 1936, but there was no obvious connection between these two activities. The mystery was deepened by the fact that, though it was well known that his war-time work for the Foreign Office, for which he was awarded an OBE, concerned code-breaking, all details of this remained classified. As my bibliography on the origins of computers grew, I came across differing reports about the design of the ACE, and about Turing, and I decided to try to solve the mystery. The purpose of this paper is to document the surprising (and tantalizing) results that I have obtained to date.

This paper is in the main composed of direct quotations from printed articles and reports, or from written replies to enquiries that I have made. It does not make explicit use of any of the many conversations that I have had. The layout of the paper follows the sequence of my investigation, but does not attempt to give a complete record of the unfruitful leads that I pursued. Furthermore, for reasons that will become clear to the reader, the discussion of the later stages of my investigations has, deliberately, been left somewhat undetailed. A final section of the paper contains a brief discussion of the origins of the stored program concept.

THE START OF THE SEARCH

Official credit was given to Alan Turing as the originator of the design of ACE, in a Government press release (Anon 1946). The biography of Turing, written by his mother (Turing 1959) stated that he had proposed a computer design to the British Government, and that it was on the basis of this proposal that he joined NPL in 1945. The obituary notice for Turing (Newman 1955), written by Professor M. H. A. Newman, who was associated with the post-war computer developments at Manchester University, stated that:

> At the end of the war many circumstances combined to turn his attention to the new automatic computing machines. They were in principle realisations of the 'universal machine' which he had described in the 1937 paper for the purpose of a logical argument, though their designers did not yet know of Turing's work.

On the other hand an obituary notice (Menzler 1968) for E. W. Phillips (who before the war (Phillips 1936) had demonstrated a mechanical binary multiplier, now in the Science Museum, and proposed the building of a version based on the use of 'light-rays') claimed that Phillips and John Womersley of NPL had started to design ACE in 1943.

Faced with these statements, I realized that the question of where the ACE fitted into the chronology of electronic computers was not at all clear. Was it in fact the case that ENIAC, and the plans for EDVAC, were the sole source of the idea of first a program-controlled electronic digital computer, and then the stored-program concept?

At this stage I came across the following statement by Lord Halsbury (1949):

> [One of the most important events in the evolution of the modern computer was] a meeting of two minds which cross-fertilised one another at a critical epoch in the technological development which they exploited. I refer of course to the meeting of the late Doctors Turing and von Neumann during the war, and all that came thereof (von Neumann 1945; Burks, Goldstine and von Neumann 1946). In a sense, computers are the peace-time legacy of war-time radar, for it was the pulse techniques developed in the one that were applied so readily in the other.

I wrote to Lord Halsbury, but he could not recollect the source of his information (Halsbury 1971):

> I am afraid I cannot tell you more about the meeting between Turing and von Neumann except that they met and sparked one another off. Each had, as it were, half the picture in his head and the two halves came together during the course of their meeting. I believe both were working on the mathematics of the atomic bomb project.

From Dr Herman H. Goldstine, who was of course closely associated with the ENIAC and EDVAC projects, came the following comments (1971):

> The question about von Neumann and Turing as I remember it is like this: Turing came to the United States the first time in 1936. He went to Princeton and studied under Church from 1936–1938. There he was in Fine Hall, which is where the Institute for Advanced Study was housed at the time, so he met von Neumann. Von Neumann, as early as the mid-twenties, had been profoundly interested in formal logics and watched Turing's work with interest. In fact he invited Turing to be his assistant for one year. Turing turned this down and preferred to return to England and the war. There he spent the war period working for the Foreign Office. While I only know this in a very second-hand way, I believe he was working on cryptographic problems. . . . Womersley was heading up the Math work at the National Physical Laboratory during the latter part of the war when I first met him (I think he also was in the same Foreign Office group as Turing early in the war and therefore knew of Turing's capabilities). After Womersley saw the machine developments in America, he got much interested and started the project at NPL and persuaded Turing to join him. Hence Turing's work on computers post-dates that of von Neumann.

Inquiries of those of Turing's colleagues who are still at NPL proved fruitless, but through the kind offices of Mr D. W. Davies, who is now Superintendent of the Division of Computing Science at NPL, it was arranged for me to visit Mrs Sara Turing. Mrs Turing was very helpful and furnished me with several further leads, but was not really able to add much to the very brief, and unspecific, comments in her book that her son had, before the war, as part of a calculating machine project, got interested in the problem of cutting gears, and had begun to build a computer. The gear-cutting episode has been described by Professor D. G. Champernowne (1971):

> In about 1938 Alan was interested in looking for 'roots of the zeta function' or some much mathematical abstractions: I think he expected them to lie on 'the imaginary axis' or on 'the line $R(z) = 1$' or some such place; and to find them he had devised a machine for summing a series involving terms such as $a_r \cos 2\pi t/p_r$ where p_r is the rth prime. All I remember is that the machine included a set of gear wheels the numbers of whose teeth were prime numbers, and I liked to fancy that as soon as the machine had found a root of the zeta function its centre of gravity would pass over the edge of the table and it would fall off uttering a swansong. I went once or twice with Alan to the engineering laboratory to assist in cutting the gear wheels, but the second world war interrupted this ambitious project and it was never completed. During and immediately after the war I often went for long walks with Alan and he tried to teach me a good deal about computers based on 'mercury delay-lines'.

Various other leads proved fruitless, and my enthusiasm for the search was beginning to wane. I eventually had the opportunity to inspect a copy of Turing's report giving detailed plans for the ACE (Turing 1945). This proved to postdate, and even contain a reference to, von Neumann's Report on the EDVAC. However, Turing's report did allude to the fact that he had obtained much experience of electronic circuits.

But then my investigation took a dramatic turn.

A SECRET COMPUTER

One of my enquiries elicited the following response (Michie 1972a):

> I believe that Lord Halsbury is right about the von Neumann–Turing meeting.... The implication of Newman's obituary notice, as you quote it, is quite misleading; but it depends a bit on what one means by a 'computer'. If we restrict this to mean a stored-program digital machine, then Newman's implication is fair, because no-one conceived this device (apart from Babbage) until Eckert and Mauchly (sometimes attributed to von Neumann). But if one just means high-speed electronic digital computers, then Turing among others was thoroughly familiar during the war with such equipment, which predated ENIAC (itself not a stored-program machine) by a matter of years.
>
> However, it is at this stage that problems of security begin to close in. In the summer of 1970 I attempted to gain clearance, through the Cabinet Office, for release of some of this information. The matter was passed to the security people, who replied in the negative – incomprehensible after so many years.... It *can* at least be said that any Anglo-American leakage of ideas is likely to have been in the opposite direction to your suggestion [that Turing had known of, and been influenced by, the ENIAC and EDVAC projects].
>
> It may surprise you and interest you to know that during the war Turing was already not only thinking about digital computation, but was even speculating, in what we now see to have been a prophetic way, about 'thinking machines'. Many of the ideas in his 1947 essay... were vigorous discussion topics during the war. Some of his younger associates were fired by this, although most regarded it as cranky.

It turns out that at least three references have been made in the open literature to the work with which Turing was associated.

In a brief account of the origins of computers, McCarthy (1966) states:

> During World War II, J. Presper Eckert and John W. Mauchly of the University of Pennsylvania developed ENIAC, an electronic calculator. As early as 1943 a British group had an electronic computer working on a war-time assignment. Strictly speaking, however, the term 'computer' now designates a universal machine capable of carrying out any arbitrary calculation, as propounded by Turing in 1936. The possibility of such a machine was apparently guessed by Babbage; his collaborator Lady Lovelace, daughter of the poet Lord Byron, may have been the first to

propose a changeable program to be stored in the machine. Curiously it does not seem that the work of either Turing or Babbage played any direct role in the labours of the men who made the computer a reality. The first practical proposal for universal computers that stored their programs in their memory came from Eckert and Mauchly during the war.

A more direct comment has been published by Michie (1968):

> During the war I was a member of Max Newman's group at Bletchley, working on prototypes of what we now call the electronic digital computer. One of the people I came most in contact with was Alan Turing, a founder of the mathematical theory of computation. . . .

(According to the book *The Code Breakers* (Kahn 1967), Bletchley Park was the wartime home of what the Foreign Office 'euphemistically called its Department of Communications'.)

Neither of these compare, however, to the amount of detail contained in a paper by Good (1970), which includes the wartime machine in a listing of successive generations of general purpose computers:

> Cryptanalytic (British): classified, electronic, calculated complicated Boolean functions involving up to about 100 symbols, binary circuitry, electronic clock, plugged and switched programs, punched paper tape for data input, typewriter output, pulse repetition frequency 10^5, about 1000 gas-filled tubes; 1943 (M.H.A. Newman, D. Michie, I.J. Good and M. Flowers. Newman was inspired by his knowledge of Turing's 1936 paper).
>
> . . . .
>
> MADM (Manchester), Williams Tube and Magnetic drum: 1950 (F.C. Williams, T. Kilburn and others, with early influence from M.H.A. Newman, I.J. Good and David Rees).
>
> . . . .
>
> M. Flowers was a high rank telephone engineer and his experience with the cryptanalytic machine enabled him later to design an electronic telephone exchange. Another telephone engineer, Dr A.W.M. Coombs, who worked with the machine, later designed the time-shared trans-Atlantic multi-channel voice-communication cable system. . . . In Britain there was a causal chain leading from Turing's paper through [the wartime cryptanalytic machine, and MADM] to the giant Atlas machine (1964), although the main influence was from the IAS plans. [Flower's initials are in fact 'T.H.' rather than 'M.'.]

Confirmation of the fact that the machine was program-controlled has come from another of Turing's war-time colleagues (Anon 1972):

> . . . Turing did visit America and may well have seen von Neumann there. Turing was an exceptionally creative man with a very far-ranging mind, and I would have expected a meeting between him and von Neumann to be very productive.

> I don't think that I can, or should, try to answer your questions on the reason for Turing's interest except as follows in the most general terms. The nature of our work was such that we needed to be able to carry out a series of logical steps at high speeds; this led in the first instance to a need for special purpose high-speed devices and then – because of the need to carry out a variety of different operations and to deal quickly with new problems – to the idea of the general purpose and thus the programmable equipment.
>
> In our field I would guess that in relation to USA, Turing gave a good deal more than he received – he was of a higher calibre than any of his opposite numbers.

However, Professor Good has indicated that this war-time computer was preceded by at least one other computer, although he too was unable to give an explicit confirmation of the reported Turing/von Neumann meeting (Good 1972):

> [Early in the Second World War Turing] made a visit to the States where I understand he met a number of top scientists including Claude Shannon. He was quite secretive about his visit but I had some reason to suspect that a small part of it was related to the atom bomb. It would not surprise me if he had contact with von Neumann during that visit. . . . Turing was very interested in the logic of machines even well before World War II and he was one of the main people involved in the design of a large-scale special purpose electromagnetic computer during the war. If he met von Neumann at that time I think it is certain that he would have discussed this machine with him.
>
> There were two very large-scale machines which we designed in Britain, built in 1941–43. One of them was the machine I just mentioned, which was mainly electromagnetic, and the second one was much more electronic and much less special purpose. . . . The second machine was closer to a modern digital binary general-purpose electronic computer than the first one, but the first also might very well have suggested both to Turing and von Neumann that the time had come to make a general-purpose electronic computer.
>
> . . . .
>
> Returning to Turing, in 1941 he designed at least one other small special-purpose calculator which was built and used, and later he worked on various speech devices. It is extremely difficult to estimate his total influence since, apart from all his classified work, he also had many conversations with many people about the automation of thought processes. In addition to this he anticipated, in classified work, a number of statistical techniques which are usually attributed to other people. I think that in order to obtain really hard facts, it would certainly be interesting to find out precisely whom he met in his war-time visit to the States. One problem is that the people with whom he spoke would have

been sworn to secrecy. Thus, for example, von Neumann might never have told H. H. Goldstine of the details of Turing's visit.

Professor Newman's role in post-war computer developments at Manchester, referred to in an earlier quotation, has been clarified by Professor Williams (1972):

> About the middle of the year [1946] the possibility of an appointment at Manchester University arose and I had a talk with Professor Newman who was already interested in the possibility of developing computers and had acquired a grant from the Royal Society of £30,000 for this purpose. Since he understood computers and I understood electronics the possibilities of fruitful collaboration were obvious. I remember Newman giving us a few lectures in which he outlined the organisation of a computer in terms of numbers being identified by the address of the house in which they were placed and in terms of numbers being transferred from this address, one at a time, to an accumulator where each entering number was added to what was already there. At any time the number in the accumulator could be transferred back to an assigned address in the store and the accumulator cleared for further use. The transfers were to be effected by a stored program in which a list of instructions was obeyed sequentially. Ordered progress through the list could be interrupted by a test instruction which examined the sign of the number in the accumulator. Thereafter operation started from a new point in the list of instructions.
>
> This was the first information I received about the organisation of computers. It may have derived from America through Newman but if it did it was pure Babbage anyway. Our first computer was the simplest embodiment of these principles, with the sole difference that it used a subtracting rather than an adding accumulator.
>
> . . . .
>
> Our first machine had no input mechanism except for a technique for inserting single digits into the store at chosen places. It had no output mechanism, the answer was read directly from the cathode ray tube monitoring the store. At this point Turing made his, from my point of view, major contribution. He specified simple minimum input facilities that we must provide so that he could organise input to the machine from five hole paper tape and output from the machine in similar form. Simultaneously with the assistance of J. C. West the synchronous magnetic backing up store was produced.
>
> These remarks must not be interpreted as suggesting that these were the only contributions to computers made by Newman and Turing, but only that they were the contributions of greatest importance to a particular group of engineers whose sole concern was to make a working machine.

(Turing had moved to Manchester from NPL, where he had been somewhat discouraged by the slowness of the progress being made towards the building of the full ACE computer.)

Another person whom I had contacted in an effort to check the story of the Turing/von Neumann meeting was Dr S. Frankel, who had known von Neumann whilst working at Los Alamos. Although unable to help in this matter, he provided further evidence of the influence of Turing's pre-war work (Frankel 1972):

> I know that in or about 1943 or '44 von Neumann was well aware of the fundamental importance of Turing's paper of 1936 'On computable numbers . . .' which describes in principle the 'Universal Computer' of which every modern computer (perhaps not ENIAC as first completed but certainly all later ones) is a realization. Von Neumann introduced me to that paper and at his urging I studied it with care. Many people have acclaimed von Neumann as the 'father of the computer' (in a modern sense of the term) but I am sure that he would never have made that mistake himself. He might well be called the midwife, perhaps, but he firmly emphasized to me, and to others I am sure, that the fundamental conception is owing to Turing – insofar as not anticipated by Babbage, Lovelace, and others. In my view von Neumann's essential role was in making the world aware of these fundamental concepts introduced by Turing and of the development work carried out in the Moore school and elsewhere. Certainly I am indebted to him for my introduction to these ideas and actions. Both Turing and von Neumann, of course, also made substantial contributions to the 'reduction to practice' of these concepts but I would not regard these as comparable in importance with the introduction and explication of the concept of a computer able to store in its memory its program of activities and of modifying that program in the course of these activities.

A fourth of Turing's war-time colleagues tended to discount the story of a Turing/von Neumann meeting, but gave further details of Turing's role at Bletchley (Flowers 1972):

> In our war-time association, Turing and others provided the requirements for machines which were top secret and have never been declassified. What I can say about them is that they were electronic (which at that time was unique and anticipated the ENIAC), with electromechanical input and output. They were digital machines with wired programs. Wires on tags were used for semi-permanent memories, and thermionic valve bi-stable circuits for temporary memory. For one purpose we did in fact provide for variable programming by means of lever keys which controlled gates which could be connected in series and parallel as required, but of course the scope of the programming was very limited. The value of the work I am sure to engineers like myself and possibly to mathematicians like Alan Turing, was that we acquired a new understanding of and familiarity with logical switching and processing

because of the enhanced possibilities brought about by electronic technologies which we ourselves developed. Thus when stored program computers became known to us we were able to go right ahead with their development. It was lack of funds which finally stopped us, not lack of know-how.

THE SECOND PHASE OF THE INVESTIGATION

At about this stage I prepared a first draft account of my investigation, for use in the furtherance of my enquiries. One of the first reactions I obtained came from Professor Knuth (1972), who has spent much time investigating wartime computer developments in the United States:

> Some years ago when I had the opportunity to study the classified literature I looked at the early history of computers in cryptanalysis, and everything I found derived from ENIAC–EDVAC. An influential memorandum written by one of the attendees at the Moore summer school session in 1946 was republished (still classified) three years ago, and it seems that memo was (in America) what led to government support for computer development.

Then Professor Michie, first quoted earlier, amplified his comments considerably (Michie 1972b):

> 1. Turing was not directly involved in the design of the Bletchley electronic machines, although he was in touch with what was going on. He was, however, concerned in the design of electromagnetic devices used for another cryptanalytic purpose; the Post Office engineer responsible for the hardware side of this work was Bill Chandler.... Chandler also worked subsequently on the electronic machines (see below).
>
> 2. Good's statement quoting 10^5 as the pulse repetition frequency is wrong. The fastest machines (the 'Colossi') had 5000 clock pulses per second, and these were driven photo-electrically from the sprocket holes of the paper tape being scanned.
>
> 3. *First machines:* The 'Heath Robinson' was designed by Wynn Williams [who had published an important paper (Wynn Williams 1931) on the design of electronic counting devices well before the war] at the Telecommunications Research Establishment at Malvern, and installed in 1942/1943. All machines, whether 'Robinsons' or 'Colossi', were entirely automatic in operation, once started. They could only be stopped manually! Two five-channel paper tape loops, typically of more than 1000 characters length, were driven by pulley-drive (aluminium pulleys) at 2000 characters/sec. A rigid shaft, with two sprocket wheels, engaged the sprocket-holes of the two tapes, keeping the two in alignment. The five main channels of each tape were scanned by separate photo-cells. One of the tapes was 'data', in current terminology, and can be compared with a modern rotating backing store, such as a disk drive. The other

carried some fixed pattern, which was stepped systematically relative to the data tape, by differing in length by e.g. one unit. Counts were made of any desired Boolean functions of the two inputs. Fast counting was done by valves, and slow operations (e.g. control of on-line teleprinter) by relays.

Various improved Robinsons were installed – the 'Peter Robinson', the 'Robinson and Cleaver' – about four or five in number.

4. *Second crop:* The 'Colossi' were commissioned from the Post Office, and the first installation was made in December 1943 (the Mark 1). This was so successful that by great exertions the first of three more orders (for a Mark 2 version) was installed before D-day (June 6th 1944). The project was under the direction of T. H. Flowers, and on Flowers' promotion, A. W. M. Coombs took over the responsibility of coordinating the work. The design was jointly done by Flowers, Coombs, S. W. Broadbent and Chandler.

The main new features incorporated in the Colossus series, as compared with the Robinsons, were:

(1) Almost all switching functions were performed by hard valves, which totalled about 2000.

(2) There was only one pulley-driven tape, the data tape. Any pre-set patterns which were to be stepped through these data were generated internally from stored component-patterns. These components were stored as ring registers made of thyrotrons and could be set manually by plug-in pins. The data tape was driven at 5000 characters/sec, but (for the Mark 2) by a combination of parallel operations with short-term memory an effective speed of 25,000/sec was obtained.

(3) Boolean functions of all five channels of pairs of successive characters could be set up by plug-board, and counts accumulated in five bi-quinary counters.

(4) On-line typewriter in place of teleprinter.

The total number of Colossi installed and on order was about a dozen by the end of the war, of which about 10 had actually been installed.

5. A 'Super-Robinson' was designed by Flowers in 1944. Four tapes were driven in parallel. Photo-signals from the four sprocket holes were combined to construct a master sprocket pulse which drove the counting. . . .

6. Two or three members of the US armed services were seconded at various times to work with the project for periods of a year or more. The first of these arrivals was well after the first machines were operational.

From this description it is possible to attempt an assessment of the Bletchley machines, and particularly the Colossi, with respect to the modern digital computer. Clearly the arithmetical, as opposed to logical, capabilities were minimal, involving only counting, rather than general addition or other operations. They did, however, have a certain amount of electronic storage, as well as the paper-tape 'backing storage'. Although fully automatic, even to the extent of providing printed output, they were very much special purpose machines, but within their field of specialization the facilities provided by plug-boards and banks of switches afforded a considerable degree of flexibility, by at least a rudimentary form of programming. Their importance as cryptanalytic machines, which must have been immense, can only be inferred from the number of machines that were made and the honours bestowed on various members of the team after the end of the war; however, their importance with respect to the development of computers was two-fold. They demonstrated the practicality of large-scale electronic digital equipment, just as ENIAC did, on an even grander scale, approximately two years later. Furthermore, they were also a major source of the designers of the first post-war British computers (the links to the ACE and Manchester University computers have been described already; in addition there is a direct link to the MOSAIC computer (Coombs 1954, Chandler 1954), a three-address serial machine whose design was started at NPL after the war, and completed at the Post Office Research Station).

There is, however, no question of the Colossi being stored program computers, and the exact sequence of developments, and patterns of influence, that led to the first post-war British stored program computer projects remains very unclear. The years 1943 to 1945 would appear to be particularly important, and for this reason I returned briefly to the investigation of E.W.Phillips.

The claim, made in the obituary notice (Menzler 1968) for Phillips, that he started work on ACE in 1943 with John Womersley turned out to be a repetition of claims that Phillips had made himself:

> Team work on the Pilot ACE commenced to be pedantically precise, at 10.30 am on Friday 15th January 1943. Turing did not join the team until the autumn of 1945. (Phillips 1965a)
>
> On 12th March 1943 John Womersley came down to Staple Inn to speak to the Students' Society, and one of the members of the Committee gave him a copy of the paper on binary notation. Thus after seven years I acquired at last – and at once – a very sterling collaborator. (Phillips 1965b)

This second remark was accompanied by again typically cryptic comments by Phillips, indicating that his 1936 paper had been based on a memorandum which he had prepared for the Department of Scientific Research, and that due to the insistence by the government that certain patents be filed, all references to thermionic valves had to be deleted. He did indeed file two patent applications relating to a calculating apparatus on 24 December 1935. However, no patents were ever granted, and there is no record of the contents of the original applications.

In a letter to the Editor of the Sunday Times (Phillips 1965c), Phillips revealed the debt he owed to Babbage, but made no further mention of events following his 1936 paper:

> After completing actuarial examinations in 1913, I turned back to a boyhood interest in Babbage's 1834 dream of an Analytical Engine, a self-operating, self-recording calculating machine – and during the 1914–18 war I was still thinking in terms of gear wheels.
>
> . . . .
>
> Still thinking in terms of mechanism until in 1928 or 1929 learned that in 1919 Eccles and Jordan had demonstrated that a thermionic valve could be in either of two stable states, and made to change with the speed of light.
>
> 1931: Wynn-Williams produced designs for electronic valves as counting units – all the binary electronic computer needed was a battery of such 'counters'.
>
> 1934: plan of electronic computer working in binary, but with octonary (digits 0 to 7) input and output completed to make the human operator's task easier. Babbage's 1834 sleeping beauty had awakened – after the proverbial hundred years.

Doubt has been cast on Phillips' claim to have collaborated with Womersley by people who knew both persons concerned. For example, Dr Goodwin has stated (1972):

> John Womersley was appointed Superintendent of the newly formed Mathematics Division at the NPL in early 1945 but the division did not really get under way until the autumn and it was then that Turing, in common with a number of us, joined the Division. I understand that it was Max Newman who suggested to Womersley that Turing should join us. He of course knew of Turing's development of the logical theory of a universal machine . . . and also of his war work. I imagine that Newman would have known of the aims of the new division from Hartree, who had played a big part in its setting up and who also knew of the work then going on in the USA on computing machines such as the ENIAC.
>
> I understand that Womersley did have a meeting with Phillips during the latter part of the war but certainly they did not do any work together. So far as I know, Phillips merely talked to Womersley about the importance of the binary scale in the context of automatic computing. While Phillips was a man of great talent, with an innovative turn of mind, I do not believe he was directly involved in the development of modern computers.

Mr J. G. L. Michel, who had attended the 1936 meeting at which Phillips had extolled the virtues of the binary system (Phillips 1936), and who had known Womersley from early in 1943, confirmed Dr Goodwin's comments, adding (Michel 1972):

> My impression was that it was I who acquainted both Womersley and Hartree with Phillips' paper . . . however since Comrie was present at the original paper in 1936 and was known by both Hartree and Womers-

ley, I may be wrong... Despite these remarks (Phillips 1965b) by Phillips, my impression from weekly lunches with Womersley was that he himself took no direct interest in digital computers until his appointment as Superintendent of the new Mathematics Division – about January 1945. Electronic computers were in the air – ENIAC had been built, a decimal machine – Eckert, Mauchly and von Neumann were discussing EDVAC, probably the first binary machine. Nevertheless in general the idea of universality of a general purpose digital computer took some grasping, and until the idea of the 'boot strapping' operation of a machine doing its own binary to decimal conversion, the furthest most people went beyond a purely decimal machine was a biquinary representation; hence one of the reasons for the delay in recognising Phillips' contribution.

Clearly, my hope that a brief investigation of Phillips' career would explain how the early British computer projects came into being has proved over-optimistic. It is, however, to be hoped that, perhaps stimulated by the present paper, a more complete investigation of the work of the British pioneers during and immediately after the war will be undertaken.

THE STORED PROGRAM CONCEPT

The suggestion that Babbage had conceived the idea of a stored-program computer apparently rests on one brief passage in Lady Lovelace's notes on the analytical engine (1843):

> Figures, the symbols of numerical magnitude, are frequently *also* the symbols of *operations*, as when they are the indices of powers . . . whenever numbers meaning *operations* and not *quantities* (such as indices of powers) are inscribed on any column or set of columns, those columns immediately act in a wholly separate and independent manner, becoming connected with the *operating mechanism* exclusively, and reacting upon this. They never come into combination with numbers upon any other columns meaning quantities; though, of course, if there are numbers meaning *operations* upon *n* columns, these may combine amongst each other, and will often be required to do so, just as numbers meaning *quantities* combine with each other in any variety. It might have been arranged that all numbers meaning *operations* should have appeared on some separate portion of the engine from that which presents numerical *quantities*; but the present mode is in some cases more simple, and offers in reality quite as much distinctness when understood.

This rather obscure passage is somewhat contradicted by other statements in Lady Lovelace's notes. I do not know of any of Babbage's own writings which would throw any further light on this question, other than perhaps a brief passage in one of his sketchbooks dated 9 July 1836 which has been drawn to my attention by Mr M. Trask. In this passage Babbage's comments on the possibility of using the analytical engine to compute and punch out modified program cards, which would later be used to control a further computation. However, Professor Wilkes' investigations of Babbage's notebooks (1971) have led him to the view that Babbage did not have a really clear idea of the notion of a program, rather than to any confirmation that Babbage had thought of the idea of a stored program. My own opinion is that, considering its date, Lady Lovelace's account of programming (which even includes an allusion to what we would now call indexed addressing) shows a remarkable understanding of the concept of a program – the question of the extent to which she, rather than Babbage, was responsible for the contents of her notes is not at all clear.

Other than perhaps this vague paragraph by Lady Lovelace and of course the implications of Turing's 1936 paper the earliest suggestion that instructions be stored in the main computer memory, that I know of, is contained in the 1945 report by von Neumann. This describes the various purposes for which memory capacity was needed – intermediate results, instructions, tables of numerical constants – ending:

> The device requires a considerable memory. While it appeared that various parts of this memory have to perform functions which differ somewhat in their nature and considerably in their purpose, it is nevertheless tempting to treat the entire memory as one organ, and to have its parts even as interchangeable as possible for the various functions enumerated above.

On the other hand, a later report by Eckert and Mauchly (1945) claims that in early 1944, prior to von Neumann's association with the project, they had designed a 'magnetic calculating machine' in which the program would 'be stored in exactly the same sort of memory device as that used for numbers'.

The earliest accounts imply that the idea of storing the program in the same memory as that used for numerical values arose from considerations of efficient resource utilization. The question of who first had the idea of, and an understanding of the fundamental importance of, the full stored program concept, that is of an extensive internal memory, used for both instructions and numerical qualities, together with the ability to program the modification of stored instructions, has been for years a very vexed one.

The earliest published discussions of the stored program concept and its impact on computer design and programming that I have seen include those by Goldstine and von Neumann (1963), Eckert (1947), Mauchly (1948), and, interestingly enough, Newman (1948). The IBM Selective Sequence Electronic Calculator (Eckert, W.J. 1948), which was started in 1945, first worked in 1947, and was publicly demonstrated in January 1948, was almost certainly the first machine which could modify and execute stored instructions (Phelps 1971). However, this machine was basically a tape-controlled machine, which had much more in common with the Harvard Mark 1 than with modern electronic computers. The earliest fully electronic stored program computer to operate was probably a very small experimental machine, referred to earlier, that was built at Manchester in 1948 primarily to test the Williams tube type of storage (1948). It was apparently not until 1949 that any practical electronic stored program computers, incorporating input/output devices, became operational. The first of these was, I believe, the Cambridge EDSAC (Wilkes and Renwick 1950), whose design had been strongly influenced by the plans, described at the 1946 Moore School lectures, for the EDVAC.

I do not wish to enter this controversy, but am instead content to attribute the idea to Eckert, Mauchly, von Neumann and their colleagues collectively. Certainly the various papers and reports emanating from this group, from 1945 onwards, were the source of inspiration of computer designers in many different countries, and played a vital part in the rapid development of the modern computer.

CONCLUDING REMARKS

The initial major goal of this little investigation, which was to check out the story of a decisive war-time meeting of von Neumann and Turing, has not been achieved. Instead, and perhaps more importantly, I have to my own surprise accumulated evidence that in 1943, that is, in the year that work started on ENIAC, and 2–3 years before it was operational, a group of people directed by M.H.A. Newman, and with which Alan Turing was associated, had a working special purpose electronic digital computer. This machine, and its successors, were in at least a limited sense 'program-controlled', and it has been claimed that Turing's classic pre-war paper on computability, a paper which is usually regarded as being of 'merely' theoretical importance, was a direct influence on the British machine's designers, and also on von Neumann, at a time when he was becoming involved in American computer developments. Furthermore, at least three post-war British computer projects, namely those at Manchester University, at NPL and at the Post Office Research Station, can be seen to have direct links to the wartime project. Unfortunately, from the computer historian's point of view, the technical documentation of these war-time British computers is, nearly thirty years later, still classified. One wonders what other war-time machines, either British or American, have yet to be revealed!

Acknowledgements

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Note added in proof

Since this paper was written I have obtained from Professor M.Lehman a two page account that Phillips prepared, probably in 1963, with the view to using it as an introduction to a reprinted edition of his 1936 paper. The account describes his early work, and gives further details of his claims to be 'the earliest of the pioneers'. The statements it makes include:

(i) Womersley, after seeing a copy of the Binary Calculation paper in 1943, wrote to Phillips saying that, stimulated by Turing's 1936 paper, he had started to build a decimal telephone-relay computer in 1937, assisted by G.L.Norfolk from 1938.

(ii) Phillips and Womersley had war-time offices in Baker Street, London, which were opposite one another, and they began to collaborate. Womersley already knew that after the war he would be joining NPL, and he resolved that he would try to convince NPL to construct a binary electronic computer.

(iii) In November 1944 Womersley read a paper embodying the ideas so far formulated to the Executive Committee of the NPL.

(iv) Before Womersley joined NPL in April 1945 he spent three months in the States, where he learnt about the still secret Harvard Mk. 1 and the ENIAC, and 'reminded his hosts of Phillips' 1936 advocacy' of binary notation.

(v) After his return to England he resumed his joint efforts with Phillips, and they were joined in August 1945 by Turing, at Womersley's invitation.

Prompted by these more detailed claims Mr J. G. L. Michel kindly investigated the files of the NPL Executive Committee. These showed that the proposals for a Mathematics Division were not agreed until May 1944 and that Womersley was not selected as Superintendent until September of that year. However, it is recorded that Womersley in his report to the Executive Committee (in December rather than November 1944) suggested that one of the sections of the Mathematics Division should be concerned with 'analytical engines and computing development'. He recommended the building of a differential analyzer and a machine using megacycle electronic counters, stating that 'all the processes of arithmetic can be performed and by suitable inter-connections operated by uniselectors a machine can be made to perform certain cycles of operations mechanically'. (Intriguingly, Dr Southwell, a member of the Committee, is recorded as having mentioned that 'some three years ago Professor Wiener in the U.S.A. was considering the development of analytical contrivances on the lines now advocated by Mr Womersley'.) Thus although we now have no reason to doubt the existence of a factual basis to Phillips' claims it would appear that the early plans for an NPL 'analytical engine' were very rudimentary indeed compared to Turing's 1945 proposal for the ACE.