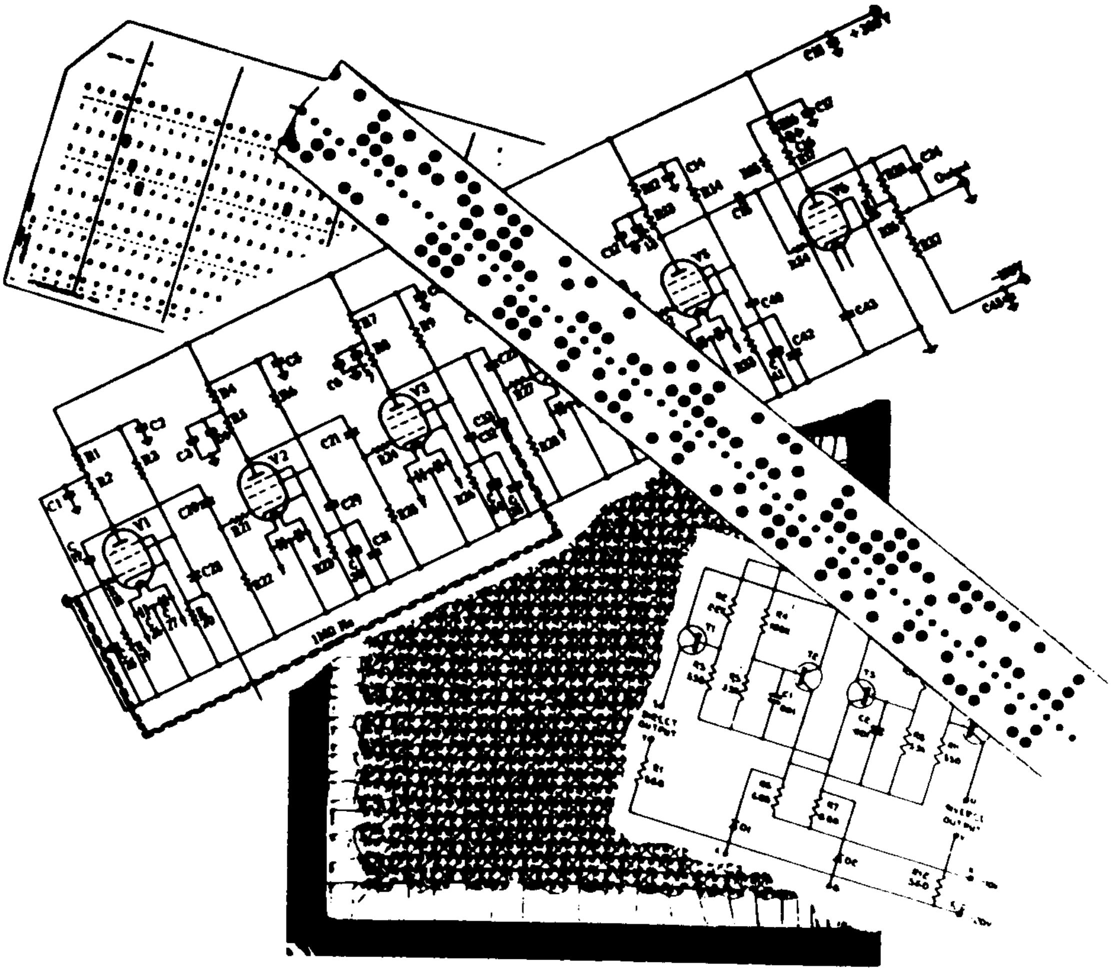
Issue Number 32 New Year 2004

Computer

RESURRECTION

The Bulletin of the Computer Conservation Society



science museum

BCS
THE BRITISH COMPUTER SOCIETY

THE MUSEUM OF SCIENCE & INDUSTRY IN MANCHESTER

Computer Conservation Society

Aims and objectives

The Computer Conservation Society (CCS) is a co-operative venture between the British Computer Society, the Science Museum of London and the Museum of Science and Industry in Manchester.

The CCS was constituted in September 1989 as a Specialist Group of the British Computer Society (BCS). It is thus covered by the Royal Charter and charitable status of the BCS.

The aims of the CCS are to

- ◊ Promote the conservation of historic computers and to identify existing computers which may need to be archived in the future

- ◊ Develop awareness of the importance of historic computers

- ◊ Encourage research on historic computers and their impact on society

Membership is open to anyone interested in computer conservation and the history of computing.

The CCS is funded and supported by voluntary subscriptions from members, a grant from the BCS, fees from corporate membership, donations, and by the free use of Science Museum facilities. Some charges may be made for publications and attendance at seminars and conferences.

There are a number of active Working Parties on specific computer restorations and early computer technologies and software. Younger people are especially encouraged to take part in order to achieve skills transfer.

Resurrection

The Bulletin of the Computer Conservation Society

ISSN 0958-7403

Number 32

New Year 2004

Contents

News Round-Up

The Society has received a donation of £3000 from the Roger Needham Trust. The Committee has voted to devote this generous gift to the Bombe Rebuild Project.

101010101

The 60th anniversary of the first running of the Colossus Mark 2 will fall on 1 June this year. Tony Sale's rebuild team at Bletchley Park are aiming to complete construction of their replica by that date.

101010101

We regret to report the death of Arthur Humphreys in August at the age of 86. Arthur was one of the most influential computer industrialists of the 1950s and 1960s, who played a major part in the negotiations which led to the creation of ICT in 1959 and then ICL in 1968. He became the first managing director of ICL, and was appointed CBE in 1971.

101010101

Congratulations to Doron Swade, who has been awarded a PhD by University College London for a thesis describing the results of his 18 years study of Charles Babbage's calculating engines.

101010101

CCS Secretary Hamish Carmichael experimented with using email to advise members about a recent seminar, as the post was at the time not functioning properly. He found he had email addresses for only around a third of members, and of these, a quarter proved to be out of date. Will any member with an email account who did not receive an email from Hamish about the Orion 2 talk on 29 October please let him know their current email address.

101010101

Donald Michie's talk to the Society on early artificial intelligence, covering the period from the 1940s to the 1960s, is now available online. The talk was one of those given at the Society's seminar "Artificial Intelligence – Recollections of the Pioneers", at the Science Museum on 11 October 2002. The URL for the event, which also contains material from other speakers, is <www.aiai.ed.ac.uk/events/ccs2002>.

Society Activity

Our Computer Heritage

Simon Lavington

Following a presentation on the *Our Computer Heritage* project at the Society's AGM in May (see *Resurrection*, issue 31, page 12), about 35 CCS volunteers have now begun a Pilot Study. The purpose is to compile outline technical information on a sub-set of British-designed early computers, in order to give the volunteers experience of collaborating by e-mail.

The *Our Computer* Heritage project is described at <www.ourcomputerheritage.org>. There is also a 'private' page of Pilot Study work-in-progress, intended for CCS members, at: <www.ourcomputerheritage.org/wp>.

The scope of the Pilot Study is limited to 17 groups of pre-1970 computers manufactured by Elliott Brothers (London) Ltd., Ferranti Ltd., and BTM/ICT/ICL. For simplicity, data is being collected for each machine under five headings:

- Delivery lists and applications
- Systems architecture
- Instruction set(s) and instruction times
- Software and sample programs
- List of references, manuals, etc

With five information-categories and 17 groups of computers, the Pilot Study database is divided into 85 cells. The cells are accessible at <www.ourcomputerheritage.org/wp>.

To take a specific example, the document containing the current state of compilation of delivery information for Pegasus is held at cell X1/F3 on the Pilot Study web site. CCS members are invited to browse the information at X1/F3. If errors are noticed, communicate directly with the relevant co-ordinator whose name is on the web site.

At the conclusion of the Pilot Study, the data on the Web site will be used to populate relevant parts of the *Our Computer Heritage* multimedia database. It is hoped that the data gathering exercise for Elliott, Ferranti and BTM/ICT computers will be finished in the spring of 2004. Funding permitting, it is then hoped to start the main *Our Computer Heritage* project in the autumn of 2004.

Pegasus Working Party

Len Hewitt & Peter Holland

Pegasus, after its long shut down due to problems with the refrigeration system, was brought back on line in mid-September after a repair was made to the cooling system. Apart from a blown fuse, Pegasus worked immediately it was switched on after being off the air for nine months. It has been run every two weeks since.

We have some problems with the Creed equipment as this is showing its age and a lack of TLC. We have recruited a Creed specialist in Mick Pearson but are handicapped by a lack of Creed manuals. May I appeal to anybody who may have maintenance manuals for Creed 54 and 75 Teleprinters and the Tape editing sets provided with Pegasus systems in their attics: please let us have a copy. Our equipment comprises:

- Automatic transmitter Model 6S / 6M and 6S / 5M 7 ½ Unit
- Keyboard/Printer/Punch Model 75RP.K4.MK3 and 75RPR.K4.MK3
- Punch Model 25 MK1V 290
- Teleprinter Model 54R

Contact Len Hewitt at <Leonard.Hewitt@ntlworld.com>.

Bombe Rebuild Project

John Harper

There is less to report this time because nearly all of our progress is taking place on items previously reported. Our current main activities are in the form of volume, repetitive production, the progress of which can be summarised as follows:

Item	Approximate number required	Progress
Commutator Production	120	>70%
Letchworth Enigma Assemblies	40	>35%
Components for above		>70%
Drum Production	200	>10%
Components for above		>80%
Menu Cableform	75	>30%
Components for above		>50%

As can be seen the main item that is dropping behind is drum production, but this is soon expected to pick up. However as we can fully test and demonstrate the bombe with only 60 drums available, this is not quite as great an issue as it would appear.

The response to our call for additional ‘hands on’ help in the previous report was very encouraging. Four more volunteers are now busy, mostly on assembly of drums. This means that we now have enough effort available to progress the repetitive work at a manageable rate.

We do however have a new problem. We do not have enough Tufnol engraved indicator discs to complete the full set of drums. We have the program written for a CNC milling machine but no machine to run this on. We could go to a commercial firm but as with all our previous work we try to get our work done for free or at least as cheaply as possible. If anybody knows of the whereabouts of a Bridgeport EZ-TRAK series 1, with a 3-axis control, with an owner willing to help us we would be very pleased to hear from you.

Since our last report we have made very good progress with the lubrication system. All pipes, metering valves and junctions are to hand leaving just the multiway junctions to complete and assemble. Good progress has been made with the junctions. A member of our team made the mould from which the brass blanks have now been cast. A drilling and thread tapping jig has been made and a pilot machining exercise carried out successfully. We now have to complete the machining of the full quantity of 66 items following which the whole lubrication system can be fitted into the machine

Last but by no means least the team would like to thank publicly the Needham Charitable Trust for making a generous financial donation to our Project. Professor Roger Needham CBE, a computing pioneer, sadly passed away in March 2003. We are greatly honoured to be recognised in this manner.

Our Web site can accessed at <www.jharper.demon.co.uk/bombe1.htm>. Readers can find there the normal update on progress and also a strong plea for help with our repetitive assembly work.

Software Conservation Working Party

Dave Holdsworth

We have a beta-release of George 3 available. The documentation included with it is probably only suitable for someone who knew George 3 previously, or who at least can remember mouseless computing. There is detailed information on getting the system to load and run, and there is an extract from 1970s documentation describing how to run the system as a user, especially a detailed description of the George 3 editor. There are also scanned pages of some manuals, notably Plan.

The issued system is for PC running any Win32 system (ie Windows 95 onwards). The emulation of the ICL 1900 and the George 3 executive is written in C, and it has also been run on Linux, Solaris and Silicon Graphics. Compiled versions for these systems can be supplied on request.

The emulation of the 7903 communications processor is with a Java program. This will run on any platform that supports Java. Mop terminals can be run as windows on the same machine as George, or can connect over the Net. There is also an emulator for the ICL 7181 VDU, also written in Java.

Any CCS member who would like a copy should email <ecldh@leeds.ac.uk>, in order to be told a URL from which you can load the system. The download is a 26 Mbyte Zip file.

I personally have found that I had forgotten just how difficult computing was in the 1970s. This is not a criticism of George 3, which was typical of its time, but one of the better examples.

Contact Dave Holdsworth at <ecldh@leeds.ac.uk>.

Inmos and the Transputer (part 1)

Iann Barron

This is the story of the UK's attempt to become a force in the emerging microprocessor industry of the 1970s, told by the man who was the driving force behind it. In this first part Iann Barron describes how Inmos came into being and the thinking behind the revolutionary transputer.

My story starts before Inmos. After working on the Modular One computer, I retired gracefully when I was 37. That was in 1974. I stayed retired for four weeks before I decided that I ought to do something else. I set myself three objectives.

The first was fairly ambitious. I didn't think much of the team at the Department of Industry and I decided it would be nice to get a new one.

The second was even more ambitious; I decided that what we really needed was a Minister for Information Technology, so I went around advocating this vociferously for several years. We did eventually get one, although it proved to be just the one - Kenneth Baker was our only Minister for Information Technology. Fortunately he was around at just about the right time for Inmos. I don't know what he did for the rest of the world, but he was quite helpful to me.

Thirdly, it seemed clear to me that microprocessors were the future, so I wanted to become an expert in that field. I eventually ended up doing consultancy for Intel, Motorola and Texas Instruments, which fairly well covered the waterfront.

At the same time, I became interested in parallel processing and models of parallel processing. I spent some time at Queen Mary College and other places looking at how one might split up computations and implement them more effectively.

It was out of that work that the idea of the transputer grew, so the transputer was around before Inmos. I'm going to move backwards and forwards between the transputer and Inmos because it is necessary to take the two together in order to understand what happened.

Inmos was a complete accident, as far as I was concerned. It started when I was asked to organise a session on the future of computing for a conference in Toronto in 1977. I decided to ask experts from each of several different areas of computing to give their vision of the future.

I realised I needed someone to talk about semiconductors, which were clearly the driving force for computing at the time. I found a man called Petritz who had been at Texas Instruments and who had subsequently written a series of what I thought were very good papers looking at how the technology might develop. So I invited him to come along and talk.

Much to my embarrassment he never responded to my increasingly frantic communications. By the time I told the organising committee, it was a serious problem - they had already printed the programmes with his name among the speakers. Right up to the conference Petritz never made contact with me.

Just before the conference started, there was an air traffic controllers' strike in Canada. So I got to Canada, as did many other people, by a devious route; I flew to Buffalo and was bussed from there to Toronto. I arrived at 3.30 in the morning.

The conference was due to start at 12 o'clock that day. None of my speakers had arrived - it was complete chaos - and by this time I'd been on my feet for some 30 hours. The conference had opened before my speakers eventually arrived, but they did include this man Petritz, whom I'd asked to lecture and who had never contacted me.

My session was immediately after the opening ceremony and was really exciting. I had never managed such a large audience before - there were some 7,000 people. The hall was packed, the aisles were packed, the seats were packed - there were people everywhere. And then Edsger Dijkstra made some very provocative comments about microprocessors; there was pandemonium; people were fighting in the aisles trying to get to the microphones.

Afterwards, I invited my speakers for a drink. We went out, found a darkened bar and settled down. I just wanted to go to sleep. Petritz sat next to me and after a few minutes he said "Pssst! Want to start a new semiconductor company?".

Petritz explained that he was planning to start a semiconductor company. He had hired someone to handle the memory side of the business, and he wanted me to handle the microprocessor side. I never said anything - after 36 hours without sleep, I was dead.

I did not see Petritz again for the rest of the conference. When it was time to leave Canada, the planes were flying again - the air traffic controllers' strike had been declared illegal by the Government.

I wanted to go to Austin, Texas. Now, the way US Customs are handled in Canada, you have to go through US Customs in Toronto or Montreal, rather than when you reach your US destination. I was dressed fairly casually, and the US Customs people wouldn't let me through. They asked for evidence that I was going to Austin, and I didn't have any because it was in my case and my case was on the plane.

So they refused to let me on the plane. Impasse! Then Petritz turned up and said "I know him! You let him on the plane". So we got on the plane and sat down together, and he explained his idea for a semiconductor company. I said no, I wasn't all that interested, because I didn't want to leave the UK. His parting line was "If you can raise the money in the UK you can have part of the company there". I thought, "Maybe I can". That is how Inmos started.

In the UK I had been consulting for the National Enterprise Board (NEB). This was the vehicle set up by the Labour government to improve industrial performance. I'd done various jobs for them. They had asked me to look at reorganising the minicomputer industry and I told them it was a waste of time. They had also asked me to look at reorganising the semiconductor industry.

When I got back to the UK I told them, "Maybe I do have an idea how you can do something for the semiconductor industry. We could set up an operation in the US. We could acquire the technology, bring it back to the UK and, hey presto, you've got a semiconductor company." That idea turned into a two hour presentation two days after I had come back from the US. The Chairman of the NEB said, "Fine! Produce a credible business plan and we'll back you". So that was a pretty good start.

However, I had difficulty in producing a business plan. I had to go back to Dick Petritz, because he had the basic understanding of the costs of the business. And again, he went incommunicado - I couldn't get any information out of him. It wasn't until the end of November that I eventually made contact and he started to take an intelligent interest. He sent over a business plan, but it wasn't terribly good.

He was asking for $12,500,000, which didn't seem to be enough. So on the general principle that you need twice as much as you think, I doubled his figure; I also turned it into pounds for good luck. So, when I went to the NEB I asked for £25 million. They looked at me and said "No, people always get it wrong, you should ask for twice as much".

If you start off with a plan for $12.5 million and have to convert it into a plan for £50 million, it's quite difficult to find a way to spend all that additional money. Fortunately, just before I produced the plan a new piece of equipment had been announced called a wafer-stepper, which replaced the original technique for exposing the photographic patterns on wafers. The merit of a wafer-stepper, as far as I was concerned, was that it cost a million dollars as opposed to $50,000, so I put them into the plan.

It proved a great technical decision. That was the way the industry went, and we were there first. We not only had wafer-steppers, we actually cornered the market. When the other companies realised they wanted them, our orders filled the world's production capacity, so they couldn't get them.

So a $12.5 million business plan was escalated into a £50 million plan, which I happily constructed over Christmas while playing Monopoly with my children. The NEB read the plan and agreed to back it.

There was still the minor problem of convincing the Labour Government that they ought to back the idea, because it was obviously politically very sensitive. It took three months to square the system. The person who objected most was Tony Benn, the MP for Bristol - a bit ironic in view of what happened subsequently. He objected to Americans getting rich out of the British Government. Eventually what happened was simply that Jim Callaghan said "We'll do it". And they did.

So the funding arrived just a year after the original discussions in August 1977. The first thing that happened was that I was given a £5,000,000 cheque to put into the bank. I went home with it, and my kids rifled my wallet that night and found the cheque inside. They assumed it was my money; sadly, it wasn't.

That was how Inmos started, completely by accident, although there was a clear rationale about what we were trying to do.

Now, one of main the elements of the Inmos plan was the transputer. As I have said, this concept had arisen out of the work that I had been doing on parallel processing. It was an attempt to create a thoroughly modern microprocessor.

Strategically, I was trying to sidestep the two principal competitors in the market: Motorola and Intel. I knew that if we went head-on with them we wouldn't have a hope because the market dynamics favoured de facto standards.

At that time both Intel and Motorola operated in a very predictable way. They each had two microprocessor development teams; it took four years to develop a microprocessor chip; so there was a new generation microprocessor every two years. Everybody knew pretty much what the performance would be because the progress of the technology was so well understood. I also knew what the microprocessors were going to be like because both Intel and Motorola were locked into supporting machine code and they didn't have any manoeuvrability to change the underlying architecture or instruction set.

So I knew exactly what was the competition would do. What I was aiming to do with the transputer was to find new markets which would straddle the market that Intel and Motorola controlled, so that when we got ourselves properly established we could swing back in and attack them in their established market.

I aimed to compete in two areas. One was embedded applications, the second was parallel processing. These have a common theme because they both require a chip which is concerned with interacting with things outside itself. What I was trying to do was to find better ways to make chips that could interact with external events, and particularly to try to do this more consistently and more formally, so there would be fewer errors in the programs that were written.

The intention with the transputer was to make improvements on all fronts. We didn't intend to do anything revolutionary. Instead we wanted to get the odd 10%-20% gain from many different areas, although we probably went a good deal further than 20% in a number of areas.

The first thing that we decided, which was at that time revolutionary, was to use CMOS technology. Microprocessors had originally been implemented using p-channel technology and then using n-channel technology.

CMOS gave us a number of advantages. It gave us an important power advantage - chips at that time were getting pretty hot and power management was important. It gave us much easier design methods, and it gave us much greater predictability of the way the circuits worked. (The n-channel circuits of the day were pretty analogue in their behaviour, whereas CMOS contained real digital circuits which behaved in a predictable and controllable fashion, so you could put lots together without getting electrical problems).

The second thing we decided was to use a formal design process. People were having a great deal of difficulty in designing microprocessors and getting them correct. Most microprocessors did not work as intended, while a lot of them had serious design flaws. That meant that the development process had a lot of iterations. We figured that if we went to a formal design process we could get a useful development cycle advantage over the competition.

The third decision was to use a proper CAD system to design microprocessors. The way it was done at the time was largely using a circuit analysis tool which only did the analogue design. It didn't tell you anything about large scale digital circuitry. The only trouble was that there were no commercial design tools available.

So one of things Inmos did, which has never been visible to the outside world, was to develop a major suite of CAD software which designed everything down to the transistor level. That was the fundamental thing that enabled us to deliver our microprocessors.

We had to do even more than this because, at the time, workstations were not available. So, we had to design our own around the Motorola 68000 to provide us with the necessary computing power to put on the desk of the designer and deliver each aspect of the design process.

Sadly, about two years after we started, CAD workstations emerged in the marketplace. Had they been there earlier we would have been delighted to buy them. By the time they came out, we were locked into our own custom systems which we had to stick with for several years thereafter.

We were worried about clock speeds, so we employed a system where we generated clocks internally on a free-running oscillator. That was synched into an internal clock which ran at a much slower speed. This was an essential part of the design concept; if there are a large number of interconnected chips disseminated across multiple boards, we had somehow or other to design a system which was coherent in spite of the fact that we could not fully synchronise the clocks.

We also decided to do something about microcode. Typical microcode, which was the way that control systems and microprocessors were implemented at the time, was very sparse and essentially had a set of wires going one way saying "this is an event" and another set going at right angles saying "this is a control signal". This meant that the microcode was mostly space with nothing useful in it. We devised a method of compacting the microcode so that we could make a microcode ROM which was about a fifth the size of any of our competitors. That was a big space advantage.

Also, because the transputer was a computer on a chip, it was going to have a processor and memory, and so we had the enormous advantage that we had an internal memory interface. If you look at a microprocessor system, the thing that slows it down is moving information off the chip and into the memory system and then from the memory back onto the chip. If you can get the memory system onto the same chip as the microprocessor, you can run the two at a very much higher speed. So we were able to go a lot faster than anyone else because we put as much memory as we could onto the chip itself.

The last technical thing we did was to go to the idea of a Reduced Instruction Set (Risc), though it wasn't called that at the time. We went away from the Complex Instruction Sets (Cisc) that were in microprocessors at the time to something very much simpler and therefore very much faster and very much cheaper to implement. This also had the advantage that it reduced the size of the chip.

Essentially all of those techniques worked and we were successful in what we were trying to do.

There was a similarly well thought out strategy for the memory business.

Our first objective was to make static RAM, the high performance memory which made the computers of the time and subsequent microprocessors really work. This was an interesting market because there was only one company, Intel, making static RAMs. Intel controlled the market, and made an enormous amount of money out of their 4K devices. There were a number of Japanese companies looking at the market. We thought we could get in there and do a good job. Another factor was that it was much easier to design a static RAM, and required less process development, so that we could get to market more quickly. So our fundamental aim was to hit the 16K static RAM market first, before Intel or the Japanese. We thought the static RAM market was going to grow enormously with the computer business, as indeed it did.

Subsequently we intended to introduce specialised high performance dynamic RAMs, and then to have a third round of products based on innovative non-volatile memory technology.

However, as a business, what we were trying to do was severely misunderstood. At that time the next ‘great challenge’ was the 64K dynamic RAM. So everyone, particularly the UK press, assumed the objective of Inmos was to make 64K dynamic RAMs. Then, as now, this wasn't regarded as a very profitable sort of business so everybody wondered what the hell we were doing. We had the problem that we were being slated in the UK for our stupid policy, and yet we did not want to reveal what we were actually doing.

Our overall business strategy was to introduce a sequence of memory products and finally some five years out to introduce a microprocessor. Five years, because that is the length of time it really takes to produce a microprocessor, I'm afraid. It was a pretty good business plan which convinced everyone.

The intention was to build a pilot plant in the US, develop the process technology there, and then build a large-scale plant in the UK, transfer the process technology across, and build the UK expertise in a viable way thereafter. I inadvisedly described this as a technology pump from the US to the UK - that got me into a lot of trouble with a lot of Americans.

Good as the business plan might be, it had weaknesses that were to cause Inmos continuing grief over the years. The problems were to do with motivation and human nature. What we were trying to create was not one but two start up companies, one in the US for memory products, and one in the UK for microprocessor products. Starting one company is hard enough, starting two which need to be integrated in terms of management, technology and objectives is virtually impossible.

It might not be obvious to an outsider, but there is very little commonality between a memory business and a microprocessor business. The process technology is different, the design philosophy is different (one involves analogue circuit design, the other involves the management of complexity), and the marketing is different (memory is sold to the purchasing organisation, microprocessors are sold to the design group). Even in well established companies like Intel, there were continual conflicts between the memory and the microprocessor businesses; so you can imagine what it was like for Inmos.

Then there are the cultural differences between the US and the UK. Two nations divided by a common language is an understatement. These cultural differences affected Inmos in all sorts of ways. The most serious was the matter of expectation. Americans are taught to be positive, they sell themselves and their achievements, often talking about what is intended to happen as though it is already achieved. The British, by contrast, tend to be cautious, identifying all the potential problems and not making claims until they are entirely certain these are justified. This meant that the Americans continually underestimated the achievements of the British, and the British continually overestimated the achievements of the Americans.

There was enormous conflict between the US and UK. It was an underlying structural problem that went on for years. My US partners regarded the UK company as a tax on the investment capital they had received, and they wanted to lose the UK company as fast as they could. Clearly my motivation was somewhat different.

Now you would think that the motivation of the UK shareholders would have been the same as mine. Unfortunately both the NEB and the Government took the view that the Americans knew best, that the British didn't know anything about semiconductors and that we had to defer to them in every respect. That made my life extremely difficult. I had a continuing battle to stop production, resources and money moving from the UK to the US, and I wasn't getting any support from my seniors. This was made more difficult because the microprocessor business was much longer term than the memory business, so there was a natural tendency to direct the available resources to the memory business, which meant to the US.

Initially there was a big debate about where Inmos should go in the UK and there was lots of political in-fighting about this. My basic management position was that it had to be somewhere near Heathrow because we were going to go backwards and forwards to the US *every* week. The political view was that the north-east or Scotland or somewhere like that would be better.

By the time the first recruitment advertisement was due to go out it had been agreed, very reluctantly, that we should locate in Bristol. I had been authorised to put the advertisement out, but on the evening before it was due to appear, I received a call from the Chairman of the NEB telling me the agreement had been withdrawn. I explained that the advertisement was already going out, so the Government machine went in motion and stopped the publication of the newspapers. The advertisement had been due to go into three or four newspapers; new copies were printed with a blank space where the advertisement should have been.

Unfortunately, the good old *Guardian* didn't quite receive the instruction properly and they printed some papers containing the advertisement. Even more unfortunately, those papers were distributed in Newcastle and the surrounding areas. All hell was let loose! The cat was really out of the bag, but the company did end up in Bristol.

The advertisement headline was "Go West young man", which led to further problems. Firstly, the sex equality people came along and said I should not be using the word "man". Secondly, we had asked for "computer architects"; the Royal Institute of British Architects pointed out that the word "architect" was controlled by an Act of Parliament passed in 1933, which specified that architects could design houses, landscapes or ships, but not computers.

We had a thousand replies to that advertisement. There were just three of us to sort them out, and that's how we got our original nucleus of 10 people.

But we really built the company on new graduates. I had done the same with Computer Technology previously; it's the best way to build a company because new graduates are keen and enthusiastic, and they do not know what is impossible. You can ask them to do anything and they just go away and do it.

The plan had been to set up a prototype semiconductor plant in the US which was due to cost $12,500,000 (you might recognise that number!).

The same thing happened; it turned into £12,500,000, and before you knew it there was £25,000,000 going into this US facility.

Well, that was not too bad. Sadly, something else happened during this time. We'd set the company up in 1978; within six months the Labour Government which had funded us was no more and Mrs Thatcher was in control. Mrs Thatcher took a very dim view of Inmos. It was an embarrassment and a nuisance to her and she didn't want to know anything about it.

I need to go back slightly to explain a detail of our funding. When it had been agreed that £50,000,000 was the sum of money which should be invested in Inmos, the NEB put in a clause which they explained was perfectly reasonable ... "Look, we know that it has no effect, but we are going to split the investment into two tranches of £25,000,000 because it would look more prudent to the Government if we had control over the second £25,000,000. We, the NEB, understand very well that there will be no measurable results after the expenditure of the first £25,000,000 so we will have really no discretion but to give you that second £25,000,000. However it will look good politically." We agreed quite happily.

We were not so happy when the Government changed. We hadn't received the second £25,000,000 by then, and the Thatcher Government immediately said "We're not going to provide it", which left poor Inmos with rather a nice US operation, some people in the UK thinking about microprocessors, but no other benefits or assets in the UK.

It took something like 18 months of very high profile argument before we got our second £25,000,000, and that severely damaged the UK aspect of Inmos because it delayed its progress by over a year while its US company was up, running and doing good things. However, we did eventually get the money.

Editor's note; this is an edited version of part of the talk given by the author to the Society at the Science Museum on 19 November 1998. The second part, describing the decline and fall of Inmos, will appear in issue 33.

Ian Barron is at iann@mynchen.demon.co.uk.

The Shocking Truth about Babbage and his Calculating Engines

Doron Swade

Charles Babbage (1791-1871) is equally famous for two things: for inventing vast computers, and for failing to build them. We tell and retell the story of his failures – his battles with government, arguments with his engineer Joseph Clement, his notorious public outbursts, his genius and eccentricity. This is familiar territory. But as questioning historians, we may well ask where the story we tell of Babbage and his engines comes from. Who framed our received perceptions of Babbage and his tale? If we trace the sources of the story it becomes apparent that our present-day perception of Babbage and his circumstances is founded on five main contemporary texts:

- a dauntingly long paper by Dionysius Lardner, published in the *Edinburgh Review* in 1834, that provides the most extensive contemporary account of Babbage's Difference Engine and its state of progress to that time;

- statement drawn up in 1843 by Sir Harris Nicolas chronicling the fraught circumstances of the engine project from 1822 until the final withdrawal of Government support in 1842;

- statement by Babbage published anonymously in the *Philosophical Magazine* in 1843;

- Richard Weld's *History of the Royal Society* published in 1849, chapter 11 of which is devoted to an account of Babbage's engine project;

- biographical work by Harry Wilmot Buxton written between 1872 and 1880, not published till 1988.

These sources largely recount the same set of circumstances and agree in their essential features: the inspired conception in 1821 of automatic calculation by machinery at a meeting between Babbage and John Herschel; Babbage's declared understanding based on an unminuted interview in 1822 with John Robinson, Chancellor, that the government had committed to fund the engine to completion; three favourable reports by Royal Society committees in 1823, 1829 and 1830, attesting the feasibility, progress and prospective utility of the engine; the halt in 1833 of the construction when Joseph Clement, Babbage's engineer, downed tools; and the final withdrawal of Government support in 1842 following a meeting between Babbage and the Prime Minister, Robert Peel. From inspired conception to final collapse took about 20 years and it was pretty well all over for Babbage by 1842 in terms of any practical prospects of delivering a physical machine.

All of these accounts, without exception, are directly based sources provided to the authors by Babbage himself. Lardner and Babbage collaborated closely on the content of the article; Babbage states openly that Nicolas's account 'was drawn up . . . from papers and documents in my [Babbage's] possession'; Weld invited Babbage to give his account and acknowledges his indebtedness to Babbage for an 'unpublished statement drawn up by Mr Babbage' and for the 'original documents which are in Mr Babbage's possession' that Weld examined; finally, Buxton's account was based directly on manuscripts given to him by Babbage.

Even though four of the five accounts are not by Babbage's hand, the voice in each of these accounts is unmistakeably Babbage's. The repetition of quoted sources and the similarity of the rhetorical positions brands them as coming from the same stable. The issue of authorship is further blurred by Babbage reprinting two of the accounts in full in his own published works: Nicolas's account is reprinted in *Passages from the Life of a Philosopher*, Babbage's autobiographical work published in 1864; Weld's account is included as an Appendix in Babbage's *The Exposition of 1851*. The historical canon is effectively dominated by Babbage's own account, and his voice, loud and strong to begin with, is amplified by the particular soft spot history appears to reserve for thwarted geniuses and for 'men ahead of their time' – a phrase which offends all but the crudest historical sensitivities. The upshot is that it seems clear that our received perceptions have a lineage directly traceable to Babbage himself.

The subtext of Babbage's self-portrayal is that of a wronged genius. He writes that he regards the Government's final and absolute withdrawal in 1842 from further support for the stalled engine project as a betrayal of the Chancellor's original commitment to fund the Engine to completion. He repeatedly wrote of his sacrifices and of his grievances at the failure of the scientific establishment as well as Government to confer honours or position in recognition of his labours. Grievance and resentment are running motifs in his writing.

And he publicly declared his conviction that he was the victim of a conspiracy rooted in professional jealousy and malice. Babbage's villain is George Biddell Airy, Astronomer Royal, *de facto* chief scientific advisor to successive governments for 45 years. Airy was consistently hostile to the utility of the engines and he emerges from Babbage's account as an influential behind-the-scenes advisor who allowed a personal grudge to bias his counsel to government against the engines.

Yet Airy's views barely feature in almost all historical accounts of the engine debates. When they do, they do so in a perfunctory and uncritical form. However, Airy had a defining influence in the fate of Babbage's first Difference Engine: in 1842 he wrote to the Chancellor expressing his view that the engine was 'useless', that Babbage was irrationally defensive of his invention, that the Royal Society committees, convened to advise government on the utility of the machines, were padded with Babbage's acolytes who were blinded by their friend's ingenuity, and that the sooner the engine project was abandoned, the better for everyone. Fortified with this damning rejection, Peel's government axed the project, offering the physical debris of the partially constructed engine to Babbage at no charge.

Historians since Babbage's time have colluded with Babbage in his own self-portrayal. Anthony Hyman, in his influential biography of Babbage published in 1982, presents Babbage as a genius surrounded by fools.

Robert Peel, Prime Minister, with whom Babbage argued and out of whose audience he stormed, is portrayed as a scientific illiterate, a classicist on whom the bounty of science and invention was lost. Hyman calls Henry Goulburn, Peel's Chancellor, a 'mediocrity'. Dionysius Lardner, a prolific populariser of science, and who wrote at excessive length on Babbage's engine, is portrayed as a clown – 'a scientific Falstaff . . . even now . . . occasionally mistaken for a serious figure'.

Hyman is relentless and produces other deliciously derisory touches: 'Lardner was the comedy act of the show: he ballooned across the engineering landscape of the time sustained by an inexhaustible supply of hot air'. And Airy, Babbage's durable antagonist, is described by Hyman as the 'prototype of the scientific bureaucrat', pedantic and unimaginative. Airy is portrayed by both Hyman and Babbage as a mediocre Salieri playing to Babbage's Mozart. Hyman's portrayal of Babbage is that of the archetypal visionary (unsuccessful) inventor – misunderstood, and whose failures were the result of the stupidity of others.

The astonishing success of the computer as a product and a tool in our own age feeds this view and encourages commentators, unselfconscious about historical method, to suggest, as does Hyman, that it was defective vision on the part of Babbage's contemporaries that blinded them to the possibilities of Babbage's great invention.

Let me isolate two of the main features of the story we have inherited from Babbage and/or his subsequent chroniclers. (1) Historical Image – that Babbage was a genius surrounded by fools who were blinded by lack of vision, or malice, to the value of his calculating engines. (2) The role of errors – that a principal purpose and enduring motive for Babbage's engines was the elimination of the risk of error from the production of mathematical tables which were riddled with mistakes.

The first thing to do is to correct the notion that George Biddell Airy, Astronomer Royal, was some kind of influential nonentity. Airy was effectively chief scientific advisor to government, a role he fashioned for himself through distinguished service over decades. He was the most prominent civil scientist of his generation who held the most exalted position in science, that of the Astronomer Royal, from 1835 for the next 45 years.

Airy was not just an astute careerist but a brilliant star rising from an outstanding mathematics undergraduate at Cambridge who outshone his contemporaries by several grades, won all the coveted prizes for mathematics, and progressed to the highest position in civil science through steady and carefully calculated steps. He served on countless government commissions and there was rarely a major engineering project on which he was not consulted. As Astronomer Royal he ran the Greenwich Observatory, which he rescued from decline, with stern efficiency making it the envy of Continental astronomy. He was quite simply the most eminent consultant engineer/scientist of his generation and well-, and possibly uniquely-qualified as a mathematician, scientist, astronomer and, as the director of the Greenwich Observatory, to act as an arbiter of utility for the calculating engines.

Given his prominence in the middle decades of the nineteenth century it is astonishing that there is no published biography of Airy. In the absence of any major biographical work, what commentators invariably quote is a single succinct statement recorded by Airy that when consulted in 1842 by the Chancellor for his views on Babbage's engine, he pronounced it 'useless'. This one line dismissal has been quoted and requoted to reinforce Airy's image as an unimaginative bureaucrat whose off-hand dismissal is taken to demonstrate his lack of vision.

New archival sources provide clear evidence that Airy's consistent and intractable opposition to the engines amounted to more than a casual one liner. Airy was consulted by government on at least four occasions for his views on the calculating engines: in 1842 by the Chancellor on the value of Babbage's Difference Engine No. 1, and on the Scheutz Difference Engine in 1843, 1857 and 1859. He was also petitioned for his views by private inventors of new calculating devices, notably by Thomas Fowler in 1841 on his ternary logic calculator, and by William Bell in 1849 on his floating-point multiplier.

His reports to government and his correspondence with private individuals record his views on the merits or otherwise of calculating devices and machines. He also made his views known to colleagues in unsolicited pronouncements, some of which were reported to Babbage. In the presence of Thomas Robinson, an astronomer friend of Babbage he opined in 1835 that the Babbage engine was useless and that those who supported it would suffer some non-specific adversity. The actor William Macready, whose diaries provide an endless source of society gossip, recorded in 1837 that Airy had referred to Babbage's engine as 'humbug'. And in 1856 Airy volunteered his views on the Scheutz difference engine in a letter to the editors of the *Philosophical Magazine.*

The Astronomer Royal's main objections to the engines were that there was no need for them. He argued that manual methods were sufficiently accurate, that the demand for new tables was non-existent or at best very infrequent, and that there was no economic advantage compared to conventional methods. He also argued that the reliability of results still depended on manually computed starting values, and commented that the enthusiasm for the engines was led by 'mechanists' and entrepreneurs not by practical computational need, the procedures of which the engine advocates appeared to be ignorant.

In relation to the Scheutz engine he did offer some concessionary positives. He conceded that building the engine, as requested by the General Register Office, and trialling it there, would be an appropriate response to public interest. He also, rather far-sightedly as it turned out, suggested that the real value of the difference engines might not be to generate tables from scratch by repeated addition but to verify existing tables by repeated subtraction. Late in the day he also saw a use for the machine in the automatic sexagesimal subtabulation of existing decimal tables for astronomy.

His objections to calculating devices and aids (as distinct from automatic calculating engines) were different but equally jaundiced. His benchmark of convenience and accuracy was the slide rule, and his measure of utility was whether or not a new device would assist the tabulation procedures of the Greenwich Observatory. Of the mechanical calculators brought to his attention he commented that their operation was too complicated, that training his clerks in the use of the devices was impractical, and that there was anyway no improvement in accuracy offered by the slide rule.

So the shocking news is that whether or not Airy's opposition had its source in a personal grudge, he made known in correspondence and publication technical grounds for his rejection of the utility of calculating machines in table-making. And his position, with one temporary exception, was uniformly, consistently and overwhelmingly negative.

Airy's opposition was not an isolated stand. He was not alone in his scepticism. George Stokes, who chaired the Royal Society committee that evaluated the Scheutz Difference Engine, was equally bleak about the value of the machine. Nils Selander in Sweden and Leverrier in France similarly dismissed the value of calculating engines and produced arguments of their own. In Leverrier's case his rejection was based on trials using the Scheutz difference engine, which was inspired by Babbage's work.

The upshot is that expert opinion in England and on the Continent resoundingly rejected the utility of the engines for the purposes of tabulation. So the shocking news is that the notion of Airy as an influential nobody, who uttered ill-founded dismissals out of whim or petulance, is not sustainable. Nor was he alone in his considered opposition to the machines.

Let us now turn to the issue of the role of errors and the perception that a principal purpose and enduring motive for Babbage's engines was the elimination of the risk of error from the production of mathematical tables which were thought to be riddled with mistakes. Almost every account of Babbage and his engines grandstand the central role of errors in the motive for the calculating machines. There are two main sources that contribute to this perception.

Firstly there is the well-known vignette of Babbage and Herschel who met in 1821 to check manually calculated astronomical tables. Dismayed by the errors they found between the two sets of independently computed results Babbage famously declared 'I wish to God these calculations had been executed by steam'. This was Babbage's mechanical epiphany. That tabular errors provided the jumping-off point is not disputed. However, it seems that historians, endlessly charmed by the episode, have translated this initial stimulus into a permanent motive. That Babbage continued to work on the design of his machines, and remained largely mute about their purpose after his initial deliberations, has allowed us to sustain the notion that his motives remained unchanged.

The second factor giving tabular errors a central role is the article on the Difference Engine by Dionysius Lardner published in 1834. This article has had a defining influence on all historical accounts since.

Lardner cites the results of a survey of 40 volumes randomly selected from a large collection of tables, from which he compiled a list of the number of published errata, that is, entries acknowledged by the publishers to be in error. He announced with ill-concealed triumph that there were in excess of 3700 known errors. He argued that each error already found represented countless yet unfound errors and implied thereby that tables were generically flawed to such an extent that they were immune to improvement by correction. His further argument was that only the 'unerring certainty of mechanical agency', that is, Babbage's Engines, could remedy the problem.

The motivational landscape is clear: errors are the problem, machines the solution – a classic problem-solution pairing not unlike that in the narrative of Harrison's clocks a century before. Because Babbage collaborated with Lardner on the article and provided most of the technical material, and because the article immodestly championed the engines, it has been widely assumed that Lardner was Babbage's mouthpiece and that the views expressed by Lardner were Babbage's.

There is evidence that the situation is somewhat different. Lardner's article was published in 1834, 13 years after Babbage's first conception of the Difference Engine, and a year after the collapse of the construction project. Lardner's argument in favour of the central role of errors as the primary motive and enduring purpose of engines had no influence whatever on Babbage's original aspirations for the engines.

To uncouple the historical conflation of Lardner's views and Babbage's, and to free Babbage from the backward projection of Lardner's advocacy, it is necessary to study Babbage's earliest writings on the engines, recorded before Lardner framed utility the way he did. Babbage wrote five papers during the six months between June and December 1822 shortly after the first trials on his prototype model of the difference engine, and before he entertained any serious ambitions to build a full sized machine.

It becomes clear from these papers that while the elimination of errors in tables does feature, it does so alongside several other benefits and justifications. Further, that errors were not necessarily Babbage's main preoccupation.

To Babbage the engines represented a new technology of mathematics. He wrote of computation as a systematic method of solution for equations. The roots of functions occur when the value of the function passes through zero. Babbage wrote about mechanisms for the automatic detection of the all-zero state to identify roots. If there were no roots, the machine fails to halt. (The halting feature is how Alan Turing later solved the problem of computable numbers.)

Babbage also wrote of the ability of the engines to compute functions for which there was no analytical law, and he predicts the advent of a new branch of mathematics (which we now call numerical analysis or computational analysis) devoted to the preparation of algorithms to minimise computation in the solution of any given problem. The elimination of errors feature in his earliest speculations, but not nearly as prominently as historical accounts have suggested. The elimination of errors did begin to feature in the arguments as a device of persuasion to portray the engines as having practical utility when support for the construction project began to be sought.

Lardner was a brilliant lecturer who made his living from lecture fees and writing. If we unpick the circumstances and background to Lardner's article it becomes clear that the pairing of errors as 'problem' with machines as 'solution' was a rhetorical device designed for the lecture hall. During his lecture tour of the northern industrial cites in 1834 Lardner tried to sell his lectures on Babbage's engines to branches of the Mechanics Institute and of the Royal Society. His hosts rejected the lectures on the grounds that the material was too 'difficult' and too 'scientific' for their audiences. Lardner hijacked a prebooked session promising one lecture and delivering another and to the surprise of his hosts the lecture on the Engines was a great success.

In this lecture Lardner removed Babbage's mathematical aspirations for the engines and framed the engines as the solution to the problem of errors. He did this to dramatise their purpose, and the false prominence he gave to errors was a piece of deliberate showmanship. Flushed with this success he expanded the number of lectures on the engines, and thousands packed the lecture halls to hear him during his three-month tour.

The published article was a write-up of the lectures and was hastily authored on his return to London in April 1834. The success of using errors as a device to justify the engines was already proven on the podia of the north. Grandstanding the role of errors and editing out Babbage's original intentions remained a defining feature of the eventual publication. If Babbage had reservations about this portrayal he does not appear to have voiced them, perhaps with good reason. The construction project had collapsed the year before and there was always the prospect that Lardner's publicity machine might revive Babbage's fallen fortunes.

In casting Babbage's grand venture as a problem-solution pairing Lardner dumbed down the richer aspirations for the engines. In doing so he did Babbage's interests fatal damage. By identifying their utility as the practical utility of eliminating errors in the production of tables he forced the engine advocates to defend the machine from a position of weakness. The utility of the engines as a solution to table making was rejected by experts in England and on the Continent.

Babbage never publicly defended the engines on the grounds of their practical utility to table making. There is clear evidence that his views of their worth went well beyond the relatively mundane capabilities in table making. So another shocking truth is that dumbing down has had a defining influence on our historical perception of Babbage's motives. Making the purpose of the engines accessible for public consumption in this way did Babbage's interests fatal damage and has misled historians since.

Editor's note: this is the edited text of a talk given by the author to the Society at the Science Museum on 20 November 2003.

Doron Swade is at <doron.swade@blueyonder.co.uk>.

North West Group contact details

Chairman Tom Hinchliffe: Tel: 01663 765040.
Email: tom.h@dial.pipex.com
Secretary William Gunn Tel: 01663 764997.
Email: william.gunn@ntlworld.com
Science & Industry Museum representative Jenny Wetton, Museum of Science & Industry, Liverpool Road, Castlefield, Manchester M3 4JP.
Tel: 0161 832 2244. Email: j.wetton@msim.org.uk

Timesharing History: the UK Story

Alan Thomson

This article aims to give an overall story regarding timesharing and its evolution in UK-developed computer systems. It has been stimulated by the earlier article by John Deane on ‘Timesharing History’, in *Resurrection* issue 31.

In the early sixties the computer world was characterised by innovation, enthusiasm, and openness. This article focuses on the UK, and on the first wave of computers that had timesharing – in the period between 1958 and 1963. While the author has endeavoured to get the story correct, he may not have properly taken account of all the events or developments in the period concerned. Comments are welcome (or dig out your evidence for when we have a CCS seminar on the subject!).

The cross references are to John Deane’s article in *Resurrection* issue 31 in the format [Ref: year], or to his new references in the Update following this article, in the format [Ref: n].

The term timesharing had various meanings, depending on which group of people you were with. In this article I use a definition provided by George Felton:

“Time-sharing is the ability to obey several independently written jobs (programs) apparently simultaneously in one mainframe computer with complete protection from accidents and bugs in other jobs.” (Note these jobs did not need users to be present.)

This was the meaning used originally in the UK. Subsequently this was also called multiprogramming.

Timesharing gave benefits in overall computer utilisation, and was a step forward in the way of looking at processing workloads, particularly for workloads involving much slow input/output (such as reading cards or printing) or updating files on magnetic tape. The workload could be split up into jobs, and several jobs could be loaded and run together, to achieve higher overall throughput.

Elsewhere in the world ‘timesharing’ had a different meaning, to do with shared access for many users; that was subsequently called multi-access.

Multi-access meant that researchers, students and academics could simultaneously submit and run their own jobs on a central computer from remote terminals (originally Teletype units, and later VDUs with a CRT monitor and a keyboard). (*See also the letter in this issue from Brian Hardisty on these two distinct meanings.*)

From late 1957, the concepts of timesharing were being evolved. A starting point can be taken as the S Gill paper on Parallel Programming [Ref: 1957], which explained the idea, and used the term ‘Time-sharing’.

The hardware technology was advancing, and in 1958 several new large computer projects exploiting transistors had been started. Disc storage was not yet commercially available – the projects used drums and magnetic tape. There were distinct markets for computers for scientific applications, and for large business and government applications (EDP).

One of the main technical advisers to the UK computer industry was Christopher Strachey, who was a consultant to both EMI and to Ferranti. Strachey had also been working for the NRDC, which had a brief and funding from Government to encourage the growth of the UK computer industry.

So when Strachey presented his paper on ‘Time Sharing in Large, Fast Computers’ in June 59 [Ref: 1959], that probably included the concepts that he had earlier recommended to his clients. There were also published documents from the USA at that time, which presented ideas for timesharing (in their sense).

So there was a lot of talk about timesharing by mid 1959, but no-one said how to actually implement it.

EMI developed the Emidec 2400 with NRDC support, with timesharing, as a large scale EDP computer.

Manchester University and Ferranti developed the Atlas, with NRDC support, with timesharing, as a very large scientific computer. The ‘Atlas Operating System’ was designed to achieve a high utilisation of the fast CPU when processing a mix of jobs, and used input and output wells on drum and magnetic tape. The designs for its supervisor, scheduler, and operating system were presented at several conferences [Ref: 1961 and Ref: 3] and were influential in the design of other large computers worldwide. A very readable retrospective article by David Howarth on

‘How we made Atlas useable’ was published in *Resurrection* Issue 13 [Ref 4], available on the CCS web site.

Ferranti developed the Orion as a medium size scientific and commercial computer. From the outset it had been decided that Orion would be a timesharing system, so the challenge then was to implement the concepts – and how far to generalise for any workload mix, and for a range of peripheral configurations.

Implementation needed hardware and software facilities, to hold the details of the active jobs and to switch between them efficiently. The overall design was resolved during 1959 – there were people with roles as system designers and system architects, as well as engineers and programmers. At that time Ferranti had a Computer Service Bureau in London, with a group of programmers led by George Felton. George and his programmers were involved in the hardware specification, and started work on what was to become the OMP operating system.

Ferranti boldly announced the Orion in late 1959 [Ref: 1] as a large commercial data processing system with timesharing.

At this time those involved with computers had various discussion forums - there was great interest in ways of advancing computer design, from industry, research establishments, and also academic involvement from universities. Specifically the Cambridge University Maths Lab ran widely attended colloquia, and there were talks on timesharing on the Emidec 2400 by N Brown (19 March 1959), and on the Orion by G Felton (14 January 1960). The London Group of the early British Computer Society had meetings at which timesharing was discussed, specifically in presentations by S Gill on 16 December 1957 [Ref: 1957] and by H Goodman on 15 February 1961 [Ref: 5]. Manchester university staff worked with Ferranti staff on Atlas, and presented several papers at conferences in the USA.

Leo Computers developed the Leo III as a medium size commercial computer, and included timesharing after several members of John Pinkerton’s development team attended the colloquium on the design for Orion timesharing presented by George Felton at the Cambridge Maths Lab. Timesharing was demonstrated on Leo III in January 1962, and on Orion in April 1962. The Leo systems were first delivered to customers in April 1962. Orion, having been delayed by hardware problems, was not delivered to a customer till March 1963.

English Electric announced the KDF9 in 1960 as a small scientific computer, with timesharing as an optional extra [Ref: 2] involving additional hardware and a specific version of the 'director' software. That was done independently of the Orion and Leo developments.

The level of timesharing facilities as designed and implemented for these early systems varied, reflecting the target markets and key customers.

Small teams were needed to design and write the system software, to deliver robust and reliable systems to run ongoing live workloads in business and government. Hardware reliability caused problems for some systems. Some of the people involved at the time are members of the CCS, and we should capture their knowledge.

I suggest that a future CCS seminar be held on the subject of the 'Early UK Operating Systems', to get the major developments positioned in the history – particularly regarding the event timeline published in *Resurrection* issue 31 by John Deane. That would also tie in with CCS activities in the 'Our Computer Heritage' project which includes capturing details and documentation for individual UK systems.

That seminar should cover the systems that ended up in large scale use over many years, specifically Atlas, KDF9, Leo III and Orion. We best get an understanding of the timesharing operating systems on each before going on to any comparison and discussion on which was first, or better, or whatever – my view is that we are likely to end up comparing apples and oranges.

There is no argument that the early sixties was a period of innovation in what would now be called 'operating systems', and that the UK had several pioneering implementations, and had grown much practical expertise.

How that expertise was applied in the next generation of UK systems is very much the story of the UK computer industry – with mergers and product rationalisation [Ref: 6]. The 1900 series executives and George operating systems, and the System 4 supervisors, drew much from earlier systems.

Ref: 1 Ferranti's ORION System, *Computer Bulletin* Dec 59, p72.

Ref: 2 The English Electric KDF9 Computer System, *Computer Bulletin* Dec 60, p119.

Ref: 3 The Atlas Scheduling System, Kilburn Payne & Howarth, *Computer Journal* 5/3 Oct 62, pp238-244.

Ref: 4 How we made Atlas usable, D Howarth, *Resurrection* issue 13, 1995, pp18-25.

Ref: 5 The Simulation of the Orion Time-Sharing system on Sirius; H Goodman, *Computer Bulletin* Sep 61, pp 51–55.

Ref: 6 ICL A Business and Technical History, M Campbell-Kelly, Oxford.

Editor's note: The author thanks CCS members who have commented on the initial draft, as well as John Deane in Australia. He points out that there are many relevant source documents (written at the time) in the ICL archive – see the CCS Web site for access details. The author can be contacted at alan.thomson@iclway.co.uk

Timesharing History: an Update

John Deane

My timesharing article in *Resurrection* issue 31 was intended to draw a response, and it certainly did. Peter Bird was quick to point out that a major omission was the Leo III system. Next, Alan Thomson sent corrections to the Ferranti Orion entry, added the EE KDF9, and outlined exciting work he is involved in to understand timesharing development in the UK. An Australian colleague, Max Burnet, gave me an entry for Digital's PDP-1. After that I received quite a lot of information about the Leo III system from Colin Tully and John Lewis. So, here's the update:

1959 Ferranti ORION system was announced in late 1959. See *Computer Bulletin* Dec 1959, p72.

1960 English Electric announced the KDF9 with optional timesharing. See *Computer Bulletin* Dec 1960, p119.

1961 Ferranti Orion "Programmers' Reference Manual" with detailed timesharing details was issued in August.

1962 Leo III was demonstrated with up to 15 processes in January 1962 in London. The timesharing aspects of the design were inspired by work at Ferranti. This was debatably the first British commercial timesharing system. See JW Lewis "Timesharing on LEO III" *Comp J* 6/1 Apr1963 pp.24-28, David Caminer in *Resurrection* 28 and 29, pp8-14, as well as the books by Peter Bird and David Caminer et al.

1962 Ferranti Orion prototype first ran timesharing in April 1962. Source: Ferranti documents.

1962 Emidec 2400 system for the UK Ministry of Pension & National Insurance ran about April 1962 with some form of timesharing. See J Drummond in *Comp J* 6/1 April 1963 pp1-4.

1962 Digital PDP-1 was set up as a timesharing system by Bolt, Beranic & Newman Inc in September 1962 in Boston. See RG Smart "Time Sharing in computer systems" *Data Trend* Nov 1964 pp.10-11, 21 and <www.mids.org/mn/710/histbook.html>.

Thank you to everyone who responded, and please keep the stories coming in to *Resurrection*, Alan Thomson or even me (at <john.deane@csiro.au>). This is very much a starting point, not a finalised timeline.

Letters to the Editor

Dear Editor,

The question posed in issue 30 of *Resurrection*, followed by John Deane's article and Dave Lillywhite's letter in issue 31, prompts me to point out that two very different historic meanings of the term "timesharing" are, I believe, being confused.

In Europe in the late 1950s and early 1960s the term timesharing was used to mean what we subsequently called multiprogramming - ie the facility for a computer to store in its memory the elements of several programs simultaneously and to switch execution between them continually under some kind of internal algorithm. This was the characteristic of Orion, Atlas and other computers of the era, and of course became virtually universally adopted by other subsequent computers.

In the USA, there was at that time much less effort put into this form of multiprogramming, and the term timesharing was, I believe, first introduced by Dartmouth College on the GE235. What was provided there was a facility for a number of students at terminals (basically simple teleprinter-type devices) to appear to have simultaneous access to a central computer. It was assumed that each student only required short bursts of access, and, as far as I recall it, there was no "multiprogramming" capability on the central computer.

This was later developed by GE and Honeywell into a complex operating system called Multics and also by Project MAC (Multi-Access Computing) at MIT, which provided similar facilities on much more powerful machines which were in fact equipped with multiprogramming capability. Incidentally this latter was metamorphosed in the UK by ICT under pressure from the UK universities into Project MOP (Multiple On-line Programming), which was initially intended be a separate operating system on the 1900 Series, but was later subsumed into George 3.

I believe that the different emphasis in the USA arose for two main reasons. Firstly they were further ahead in the provision of computing facilities for students at universities, and so their need for access was greater; in Europe at the time the only facilities for students were essentially via batch systems, or one-at-a-time sequential direct “hands-on” access - literally queuing at the computer.

Secondly there was enormous funding in the USA from the US Government for computer development, and most large-scale systems were being developed for running very complex secret computer programs (eg for nuclear weapons development), and any question of sharing resources would have been anathema.

Their preferred approach was to “front-end” the main computer (typically an IBM 7090) with a small-scale computer (eg an IBM 1401), which collected all the input from slow devices, mainly card readers, and transferred this to magnetic tape or disc. This was then used to feed the main computer at high speed, and this ran one program at a time on a dedicated basis. Output was dealt with in a similar fashion.

So the question of which computer first had timesharing should actually have two answers, reflecting the two different meanings of the term.

Yours faithfully
Brian Hardisty (Dr)
By email from <brian.hardisty@tiscali.co.uk>
22 September 2003

Dear Editor,

Can I seek your, and the Computer Conservation Society Members', help in trying to trace copies of the following early reports related to compilers that I authored or co-authored while I was at English Electric:

The Whetstone KDF9 Algol Translator, Randell, B, Atomic Power Division, English Electric Co, Whetstone, Leics, June 1962

Discussions on Algol Translation at Mathematisch Centrum, Randell, B and Russell, LJ, W/AT 841 Atomic Power Division, English Electric Co, Whetstone, Leics, 1962

Deuce Algol, Randell, B and Russell, LJ, W/AT 844 Atomic Power Division, English Electric Co, Whetstone, Leics, February 1962

Easicode, Kelly, MJ and Randell, B, W/AT 585 Atomic Power Division, English Electric Co, Whetstone, Leics, October 1960

I was prompted to start searching for these reports mainly because of the one entitled *Discussions on Algol Translation at Mathematisch Centrum* that I mention in the paper (or more exactly the banquet speech) that I gave at the recent IEEE International Workshop on Object-oriented Real-time Dependable Systems (Words 2003F), a workshop that was dedicated to the memory of the late Edsger Dijkstra. The interest aroused was such that I really would like to find this report again - and with it the various other reports I wrote at that period. (Needless I to say, I've tried already all the obvious libraries and archives I can think of.)

Best wishes
Brian Randell
School of Computing Science, University of Newcastle
By email from <Brian.Randell@ncl.ac.uk>
14 October 2003

Editorial contact details

Readers wishing to contact the Editor may do so by email to <wk@nenticknap.fsnet.co.uk>.

CCS Collection Policy

The Committee of the Society has formulated a policy statement concerning procedures for dealing with computers of historical interest that come to the Society's attention. This is published in full below.

The Society has no Collection of its own, and no premises in which to house one. There is no intention to change this.

When the Society hears of historic equipment which is becoming available for conservation, it will attempt to find a suitable home for it in one of the following major collections:

- The Bletchley Park Museum Trust
- The Science Museum, South Kensington
- The Museum of Science and Industry, Manchester

The Society will also alert other collections to the availability of surplus equipment, where the major collections are unable to offer to house it, if it fits the appropriate area of interest. Members who know of such collections are asked to ensure that the Secretary is aware of their location and subject matter.

CCS Web Site Information

The Society has its own World Wide Web (WWW) site: it is located at <www.bcs.org.uk/sg/ccs/>. This is in addition to the FTP site at <ftp.cs.man.ac.uk/pub/CCS-Archive> (please note that this latter URL is case-sensitive). The Web site includes information about the SSEM project as well as selected papers from *Resurrection*. Readers can download files, including issues of *Resurrection* and simulators for historic machines.

Forthcoming Events

Every Tuesday at 1200 and 1400 Demonstrations of the replica Small-Scale Experimental Machine at Manchester Museum of Science and Industry.

22 January 2004 London meeting: "Twenty Years of the Apple Mac – or What the PC should really have been like". Various speakers.

27 January 2004 NWG meeting: "Software and User Experience of Orion 1".

24 February 2004 NWG meeting: "History of Communications". Speaker John Vernon.

26 February 2004 London meeting: "Pioneers of Payroll on Computers – Leo, the Army, the Navy Dockyards and De Havilland". Chair: John Aris.

23 March 2004 NWG meeting: "Use of Computers in TV". Speaker David Lyon.

Details are subject to change. Members wishing to attend any meeting are advised to check in the Diary section of the BCS Web site, or in the Events Diary columns of *Computing* and *Computer Weekly*, where accurate final details will be published nearer the time. London meetings take place in the Director's Suite of the Science Museum, starting at 1430. North West Group meetings take place in the Conference room at the Manchester Museum of Science and Industry, starting at 1730; tea is served from 1700.

Queries about London meetings should be addressed to Hamish Carmichael, and about Manchester meetings to William Gunn on 01663 764997 or at <william.gunn@ntlworld.com>.

Committee of the Society

Chairman **Dr Roger Johnson FBCS**, 9 Stanhope Way, Riverhead, Sevenoaks, Kent TN13 2DZ. Tel: 020 7631 6709. Email: r.johnson@bcs.org.uk

Vice-Chairman **Tony Sale Hon FBCS**, 15 Northampton Road, Bromham, Beds MK43 8QB. Tel: 01234 822788. Email: tsale@qufaro.demon.co.uk

Secretary **Hamish Carmichael FBCS**, 63 Collingwood Avenue, Tolworth, Surbiton, Surrey KT5 9PU. Tel: 020 8337 3176. Email: hamishc@globalnet.co.uk

Treasurer **Dan Hayton**, 31 The High Street, Farnborough Village, Orpington, Kent BR6 7BQ. Tel: 01689 852186. Email: Daniel@newcomen.demon.co.uk

Science Museum representative **Dr Xerxes Mazda**, Manager of Collections Access, Science Museum, Exhibition Road, London SW7 2DD. Tel: 020 7942 1100. Email: x.mazda@nmsi.ac.uk

Museum of Science & Industry in Manchester representative **Jenny Wetton**, Museum of Science & Industry, Liverpool Road, Castlefield, Manchester M3 4JP. Tel: 0161 832 2244. Email: j.wetton@msim.org.uk

National Archives representative **Jeffrey Darlington**, Digital Preservation Department, National Archives, Kew, Richmond, Surrey TW9 4DU. Tel: 020 8392 5268. Email: jeffrey.darlington@nationalarchives.gov.uk

Computer Museum at Bletchley Park representative **Michelle Collier-Moore**, The Computer Museum, Bletchley Park, Bletchley MK3 6EB. Tel: 07748 981391.
Email: michellecolliermoore@hotmail.com

Chairman, Elliott 803 Working Party **John Sinclair**, 9 Plummers Lane, Haynes, Bedford MK45 3PL. Tel: 01234 381 403. Email: john@eurocom-solutions.co.uk

Chairman, Elliott 401 Working Party **Chris Burton CEng FIEE FBCS**, Wern Ddu Fach, Llansilin, Oswestry, Shropshire SY10 9BN. Tel: 01691 791274.
Email: cpb@envex.demon.co.uk

Chairman, Pegasus Working Party **Len Hewitt MBCS**, 5 Birch Grove, Kingswood, Surrey KT20 6QU. Tel: 01737 832355. Email: leonard.hewitt@ntlworld.com

Chairman, DEC Working Party **Kevin Murrell**, 25 Comet Close, Ash Vale, Aldershot, Hants GU12 5SG. Tel: 01252 683503. Email: kevin@ps8.co.uk

Chairman, S100 bus Working Party **Robin Shirley**, 41 Guildford Park Avenue, Guildford, Surrey GU2 5NL. Tel: 01483 565220. Email: r.shirley@surrey.ac.uk

Chairman, Bombe Rebuild Project **John Harper CEng MIEE MBCS**, 7 Cedar Avenue, Ickleford, Hitchin, Herts SG5 3XU. Tel: 01462 451970.
Email: bombe@jharper.demon.co.uk

Chairman, Mil-DAP Working Party **Brian M Russell CEng MIEE**, 5 Briarmere Walk, Chadderton, Oldham OL9 6SH. Tel: 0161 652 6475. Email: bmrussell@iee.org

Chairman, Software Conservation Working Party **Dr Dave Holdsworth CEng Hon FBCS**, University Computing Service, University of Leeds, Leeds LS2 9JT. Email: ecldh@leeds.ac.uk

Chairman, Preservation Policy Working Group **Professor Simon Lavington FBCS FIEE CEng**, Lemon Tree Cottage, High Street, Sproughton, Suffolk IP8 3AH: Tel: 01473 748478. Email:lavis@essex.ac.uk

Committee of the Society (contd)

Chairman, North West Group **Tom Hinchliffe**, 44 Park Road, Disley, Cheshire SK12 2LX. Tel: 01663 765040. Email: tom.h@dial.pipex.com.
Editor, Resurrection **Nicholas Enticknap**, 4 Thornton Court, Grand Drive, Raynes Park SW20 9HJ. Tel: 020 8540 5952. Email: wk@nenticknap.fsnet.co.uk
Dr David Anderson, The Woozle, 1 Oatlands Road, Boorley Green, Hants SO32 2DE. Tel: 0239284 6668. Email: cdpa@btinternet.com
Peter Barnes FBCS, 10 The Broadway, Gustard Wood, Herts AL4 8LN. Tel: 01438 832906. Email: barnes@peterbarnes.freeserve.co.uk
Dr Martin Campbell-Kelly, Department of Computer Science, University of Warwick, Coventry CV4 7AL. Tel: 01203 523196. Email: mck@dcs.warwick.ac.uk
George Davis CEng Hon FBCS, 4 Digby Place, Croydon CR0 5QR. Tel: 020 8681 7784. Email: georgedavis@bcs.org.uk
Harold Gearing Hon FBCS, 14 Craft Way, Steeple Morden, Royston, Herts SG8 0PF. Tel: 01763 852567.
Eric Jukes, 153 Kenilworth Crescent, Enfield, Middlesex EN1 3RG. Tel: 020 8366 6162.
Ernest Morris FBCS, 16 Copperkins Lane, Amersham, Bucks HP6 5QF. Tel: 01494 727600. Email: Ernest.Morris@btinternet.com
Dr Doron Swade CEng FBCS, 54 Park Road, Kingston-upon-Thames, Surrey KT2 6AU. Tel: 020 8392 0072. Email: doron.swade@blueyonder.co.uk

Point of Contact

Readers who have general queries to put to the Society should address them to the Secretary: contact details are given on the page opposite.

Members who move house should notify Hamish Carmichael of their new address to ensure that they continue to receive copies of *Resurrection*. Those who are also members of the BCS should note that the CCS membership is different from the BCS list and so needs to be maintained separately.

Resurrection is the bulletin of the Computer Conservation Society. Copies of the current issue are available from the Secretary for £5.00 each.

Editor -- Nicholas Enticknap
Typesetting -- Nicholas Enticknap
Cover design -- Tony Sale
Printed by the British Computer Society