The Cavendish Laboratory Remembered - 1950 to 1952

I graduated in Physics in 1952. Computing was already (just about) included as an option (which I chose), delivered by Maurice Wilkes, but with nothing 'hands-on'. One year later the Diploma in Computer Science was introduced at Cambridge - a world first.

There are obvious links between the early history of Computing at Cambridge, and the history of the Cavendish . Haroon Ahmed has written a fine history of Cambridge Computing (1), including reference to the early mechanical and analog machines. But like other historians, he is sparse on the methodology of scientific computation before the electronic computer arrived - methodology which played a significant part in the evolution of modern computing. So let me say a bit about that methodology.

I still have my personal Cavendish Laboratory notebooks from the 1950-1952 period. They cover my 33 undergraduate experiments. Except for a few post-war additions such as "The Waveguide", all would have been familiar to Rutherford and to George Searle in the 1930s. Of course that was due to change very soon, but I think that I was very fortunate in receiving an outstanding training in experimental physics, with pragmatic mathematical skills.

Most experiments related to theory and involved numerical computation. For me, the most ambitious and time-consuming was Millikan's Oil Drop Experiment, performed by many undergraduates. In those days without computers or scientific calculators, it was computationally quite arduous. My 22-page notebook account on 'Millikan' contains many hand-ruled tabular worksheets. There are some 1600 numerical entries, which eventually led, step by step, to 'my' value of e. It is all based on the evaluation of an equation based on Stokes' Law, and clearly with much data to analyse it was important to be systematic and accurate.

The only practical way of doing fairly precise scientific computation of this kind was to use books of mathematical look-up tables. The equation was substantially rearranged as a sequence of steps, (a 'program') each step coming from a limited 'Instruction Set'. That set consisted of addition and subtraction, together with the 14 functions provided by look-up tables in the (Cambridge) Four-Figure Mathematical Tables. (Logarithmic, Trigonometric, Reciprocals, Squares and Square Roots ; Godfrey & Siddons booklets had some additional functions.)

This rearrangement led to large tables in which data, many stages of partial results, and final results were laid out in rows and columns. Quite evidently, this is a paradigm of a stored program computer, closely mirroring the logical organisation. But it is rooted in the computational methods of the pre-computer era. The methods I have described will have been familiar to applied mathematicians and scientists of the period.

What is not clear from my notebooks is the extent to which my own work resembled that of other students. Unfortunately I gave no references. Did I read background material such as Millikan's papers? (I think not) Did I follow closely prescribed guidance by the Cavendish on the experimental and computational procedures? Or did I have some scope for originality? That would surely have been encouraged and expected.

Of course the computational difficulties of the time influenced scientific experiment - the practicability, the planning and the procedures. There would have been almost inevitable numerical mistakes as well as the propagation of rounding errors from the use of four-figure tables. (In fact I've run my own 1952 data through a spreadsheet, discovered just one logarithm incorrectly entered, and small differences caused by the rounding).

It is interesting to find Millikan's Experiment used as an example in the modern "Introduction to Computational Physics". (2)

There's more to say about the influence of the old methods. Here I must declare myself! I am one of the targets of the mathematician S. Barry Cooper in his scornful and dogmatic "... **But to this day, there are engineers who find it hard to excuse (or even understand) Turing's reputation as the 'inventor' of the computer.**" I cannot resist responding!

The real trigger for practical computers was the assembly of practical ideas in von Neuman's 1945 report (3), followed by the 1946 Moore School lectures. The introductory discussion in von Neumann's report, particularly relating to the use of memory, was strongly linked to ' computation'. He defined eight different uses of memory in computational problems, and analysed their memory requrements in some detail. It would all have gone down well with Maurice Wilkes and Douglas Hartree (and L.J. Comrie, together with many others with a background related to practical 'computation'). Douglas Hartree himself delivered one lecture in the 1946 Moore School series, on the Solution of Problems in Applied Mathematics.

The abstractions of the Turing Machine were (then) at most a sideshow. And it is nonsense to suggest, as some do, that the early developers were consciously building 'Turing Machines'. Turing himself said that "**A man provided with paper, pencil, and rubber, and subject to strict discipline, is in effect a universal machine.",** and in his own 1946 technical proposal he refers to memory as the analogue of 'computing paper'.

Enough said on that topic.

Let me now jot down a few hazy memories about the Cavendish personalities who I remember from 1952. About a dozen were - or became - Fellows of the Royal Society, Five received Knighthoods and Three Nobel Prizes. And it was a very small Department in those days - just 1 Professor, 3 Readers, 6 Lecturers and 7 Demonstrators (or thereabouts).

Today's Cavendish Staff List is headed by some 34 Professors! And yet the Undergraduate numbers do not seem to have changed very much since 1952. So today's Cavendish Laboratory is obviously a very different place from the one I remember in Free School Lane.

My part II course in 1952 had about 80 students. Our lectures, and (especially) the supervision of our experimental work, formed a substantial part of the heavy work load for the academic staff - assisted by several post-graduates. Our Course Photograph includes Shoenberg, Pippard, Wilkinson, together with several post-graduate supervisors , and white-coated Fred Linsey.

Professors

Professor Bragg. **"Bragg lectured by charm"** (attributed to Searle). We were privileged to attend his lectures on X-Ray Crystallography, delivered in the Maxwell Lecture Theatre. Enthusiastic and supremely lucid. But then we'd find that, misled somewhat by the apparent simplicity, we had not made careful notes, and it wasn't as easy as Bragg made it appear!

Professor Hartree (not actually part of the Cavendish). Douglas Hartree was a very familiar figure to me in Christ's College, but (unfortunately) I had no personal contact with him. I believe he usually gave fine lectures on Quantum Mechanics, but not in our year. The substitute (name forgotten) was very poor. Hartree was a hugely important figure who ought to be better remembered, in particular for his seminal and practical role in early computing (acknowledged by Maurice Wilkes).

Readers

David Shoenberg (Reader, 1952) He was the (somewhat reserved?) 'father-figure' to our Part II course, giving lectures on Low Temperature Physics, and appearing in practical classes. Though not a great communicator, he gave full attention to his undergraduate teaching. I found it impossible to accept a recently published Wikipedia statement that he was 'a notoriously poor lecturer', inserted anonymously. It wasn't true and I successfully deleted it, acting as a Wikipedia editor!

Eddie Shire was the other Reader, notably introducing us to 'his' van de Graaff generator.

Lecturers

Brian Pippard and Denys Wilkinson had been recently appointed Lecturers. Both were excellent, but Pippard took a bigger part in my own laboratory classes (marking several of my experiments generously!). I was then pretty keen on Atomic Physics which must have been taught by Wilkinson. And Albert Kempton, last of Rutherford's associates, was my Director of Studies at Christ's, and my 3rd year tutorials were with him. So I did visit Harwell with thoughts of taking up atomic/nuclear physics, but didn't continue in that direction. (Serendipity perhaps; I have no regrets)

V.E. Cosslett also lectured to us, but left me with no long-term memories .

Fred Hoyle (Maths) certainly had the personality, but seemed unaware that lectures needed advance preparation. (He preferred to talk about cricket!) Abram Besicovitch (student of Markov) was a most distinguished mathematician. I was loyal to his somewhat bumbling lectures when others deserted them. He became Rouse Ball Professor.

I think that some of my subjects were third-term options covering 'latest' developments. Martin Ryle covered Radio Astronomy with great enthusiasm, and Maurice Wilkes (then Director of the Mathematical Laboratory) was just as interesting to me, covering Computers and EDSAC. I doubt if he mentioned Turing. (Nor did an early computer course I attended elsewhere later in the 1950s)

Demonstrators

Several who lectured to us were actually Demonstrators at that time. Ken Budden was one, becoming a Lecturer in 1953. He was less colourful than, say, Hoyle and Ryle, but he (and Basil Briggs) was prominent in the theory of ionospheric radio-propagation - which became central to my own work at a later date.

Tommy Gold was another Demonstrator who lectured - on Dynamics. He was certainly a one-off, quite histrionic when he talked of 'a ball rolling on a horizontal table'. Clearly he was a very versatile man - but I certainly did not know then that he had a crucial role in EDSAC, providing Wilkes with a workable mercury delay-line at a time when other early computer-developers were floundering.

Others

About six post-graduates assisted in the practical classes. I do not really remember them, but they included Anthony Hewish, Kenneth F. Smith, George W. Hutchinson and Robert G. Chambers, who all achieved great distinction.

Fred Linsey was a familiar. white-coated figure I think he was a senior technician who ran the laboratory. He joined the Cavendish in the 1930s, but worked on Radar at Malvern during the war.

References

1. "Cambridge Computing - The First 75 Years" Haroon Ahmed. Published at https://www.cl.cam.ac.uk/downloads/books/CambridgeComputing_Ahmed.pdf

2. "An Introduction to Computational Physics" Tao Peng. C.U.P. 2006

3. "First Draft of a Report on EDVAC" John von Neuman. Republished at https://www.wiley.com/legacy/wileychi/wang_archi/supp/appendix_a.pdf

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