Ten Years of Computer Development

The Rt. Hon. The Earl of Halsbury

This paper gives a historical account of how computers have developed in the United Kingdom. For the benefit of overseas readers it is recorded that Lord Halsbury was appointed temporary adviser to the Board of Trade in May 1949 and, when the National Research Development Corporation was established, he became its first Managing Director at the end of June 1949. He has announced his intention of leaving the Corporation at the end of March 1959: this paper is, therefore, part of the permanent historical records of British computer development, by one who has, for ten years, been closely associated with this work among a number of manufacturing companies.

1. Introduction

In June this year you listened to an interesting review by our President dealing with the second decade of computer development.[3] It might, therefore, seem a strange inversion of the historical order if I now proceed to talk about the first decade. I offer no apology for this, however, because the subject is an interesting one and never more so than when treated from the historical angle.

The historical approach is ambiguous because it contains a subjective element. Two historians, given the same set of facts, may interpret them in very different ways, both of which may be quite legitimate when regarded from one or another point of view.

2. The beginning

Take, for example, the question of where one begins. I have always been interested in beginnings and origins and it has always seemed to me that one appears to be between the horns of a dilemma in locating them. On one view of the matter there is nothing new under the sun; one always seems able to trace beginnings further and further back until they seem to get lost in the mists of time. On another view of the matter everything new is in some respects unlike everything that preceded it, and we find a new beginning moment by moment throughout the history of any development.

Personally I have always found it helpful to trace a development backwards until one reaches a discontinuity forming the most recent of many beginnings. We can locate such a discontinuity if everything subsequent to it can be regarded as directly descended from it and everything precedent to it has a thematic structure; that is to say we find, prior to the discontinuity, themes in isolation which, subsequent to the discontinuity, we find in combination. If we regard a modern computer as involving: (1) all-electronic data processing, (2) stored program, (3) automatic peripheral equipment, then the ancestor of all such equipment was born in Dr Wilkes' laboratory in Cambridge in May 1949. If you accept this criterion as satisfactory, then at the date of this lecture – in the autumn of 1958 – nearly one decade has elapsed. If, on the other hand, you regard the third feature of the specification as inessential and regard fully automatic peripheral equipment as an adventitious facility but no more, then you would locate the beginning of things in Professor Williams' laboratory in Manchester in the spring of 1948. The prototype of that computer could only subtract and had only manual input; there were, therefore, practical limits to the amount of computation it could effect and the quantity of data it would handle.* It has for me more than the importance of a date to which a conventional beginning can be assigned. The patent situation on Professor Williams' highly ingenious method of cathode-ray tube storage was one of the first things I had to deal with on becoming associated with N.R.D.C. If it was not the beginning of computers, it was the beginning of computers for me! Whether you regard the one event or the other as making a beginning depends on your point of view. If you are an engineer you may incline to think the establishment of a principle the more significant of the two events. If you are a user you may assigned priority to the achievement of a practice.

3. The background

On either view, however, one has to concede that every beginning has itself a history, and behind the combination of the first two criteria in the above specification there were computer-like machines which illustrated the one principle or the other. The ENIAC was undoubtedly an all-electronic machine. Its program, however, was of the plug-board variety, and its storage capacity so limited as to have disqualified it from attempting many types of computation that we should now regard as typical. The sequence-controlled calculators of contemporary date were able to work with a stored program of adequate computational power, but such programs were not stored electronically and the speed of the machines was reduced to that of electromechanical rather than electronic devices.

The card-sequenced calculator at I.B.M.'s world headquarters was a most impressive installation by any standard. Neon lights flashed across the most spacious of display panels, while loops of punched cards travelled about the creature's interior on their lawful occasions. As an exercise in dark marble, chromium plate, and a dim religious atmosphere, one had to vote it a complete success. The fact that, when I saw it, it was working on astronomical data seemed to add, if that were possible, to its cosmical significance. Alas that its like will not be seen again! Behind these various calculating machines of intermediate type there lay another discontinuity, and behind that again more history of the thematic type. The work of the late Dr Comrie in applying punched-card machines to mathematical computation provided one such theme. Wynn Williams' counting circuits and their development into coincidence and anti-coincidence devices for the purpose of cosmic-ray research represented another. And so we can retrace the course of history backwards, through Hollerith and Powers to Jacquard, on the one hand, or, by following a different theme, on the other hand, we can return to Babbage and thence travel in thought back to Leibnitz, Pascal, and whoever invented the abacus. What we never seem to find is a true beginning. The nearest we get to it is some sort of a discontinuity separating continuous evolution on the near side from thematic structure on the far side. Of these discontinuities in computer evolution, perhaps the most important will one day be acknowledged to be not a machine, not a storage device, not a circuit, not a logical design, but rather a meeting of two minds which cross-fertilised one another at a critical epoch in the technological development which they exploited. I refer of course to the meeting of the late Doctors Turing and Von Neumann during the war, and all that came thereof.[1,2] In a sense, computers are the peacetime legacy of wartime radar, for it was the pulse techniques developed in the one that were applied so readily in the other. I often ask myself whether the astronautics of the cold war will give us a peacetime legacy half as interesting in the future as computers are in the present. On the whole I feel inclined to doubt it.

4. The scientific administrator

During the greater part of the decade which I am attempting to review tonight it has been my privilege to have a ringside seat as the more interesting developments took place. My position has throughout been that of an administrator and I speak as such tonight. I have no pretensions to being a computer engineer, nor am I a programmer, save out of curiosity to see how a code works out in practice. This being so, may I say a few words about the administrator's point of view as I conceive it in the context of scientific administration.

First of all, a scientific administrator will be a scientist. 'Will have been' might, perhaps, be more appropriate, for it only takes a year or two at the administrator's desk to remind him that any experimental skill he may have had once has been rendered obsolete by new developments. Though his will accordingly have qualified professionally in some discipline or other at some time in the past, this does not entitle him to pose as a professional in every field under the sun. It is a temptation to exercise his function by imposing his own thoughts on how the course of future developments should proceed, forgetting that a little knowledge is a dangerous thing. This temptation must be resisted. A scientific administrator's function is to provide the facilities, particularly the money, that experimentalists or design engineers require for the development of their own ideas, not his. The money is usually somebody else's and he should expect no gratitude for doling it out to one or another of his protégées. He is not their master but someone else's servant, employed to provide them with what they need.

The discharge of this function in accordance with the foregoing ideal is by no means easy. There is rarely enough money for all purposes and the administrator finds himself making selections willy-nilly. The final effect of this can be just as definite as imposing his will by fiat. Even if he resolves to act on advice, he still has to select his advisers, and, if their advice is not unanimous, to choose between them. However impersonal he tries to be, thus finds himself swept up into policy making, thereby affecting the course of events to an extent which may keep him awake at nights wondering just what sort of effect his decisions may be having for good or ill. Like the constitutional head of a state he is entitled to be told about everything that is going forward, and like such a constitutional head he may encourage or warn. Woe betide him, however,

* Photographs of EDSAC 1 and the Manchester University Mark I Computer appeared on p. 102 of Vol. 1.

if he tries to interfere with details or express a preference for one technical means rather than another. He can never be an expert in the professional sense so long as he sits at that desk of his, and his encouragements and warnings should be of a general character: not to try and do too much with insufficient means; not to take an eye off the ball; not to forget the lessons learnable from the mistakes of others, and so on. In this way he can play a creative part.

In dealing with computers I had to work some such *modus vivendi* out as I went along. Fate was kind by dealing me a hand representing a quite general situation at the outset.

5. Early forecasts of importance

Apart from those actually engaged in making experimental computers, no one had the slightest idea that something technologically important had happened. Until the general importance of computer technology was recognised it was idle to start influencing the way that developments should go.

There are a number of cases in the history of technological developments where the pioneer has quite failed to appreciate the revolution he has brought about. (It is more common for him to exaggerate the importance of what he has achieved.) Kipping's discovery of the silicones was such a case. To the end of his days he took pride in his discovery as an interesting contribution to pure chemistry; he never foresaw its field of application.

In the field of computation, and thus of mechanical aids thereto, the late Professor Hartree enjoyed a quite unique reputation as a pioneer and any opinions he chose to utter commanded instant respect. Quite apart from his work on the differential analyser and analogue methods of computation, he was a staunch friend of the early computer designers and one to whom they all owed much for his constant sympathy and encouragement in what they are trying to do. It was extraordinary therefore that he, of all people, completely underestimated the extent to which digital computers would prove to be *pervasive*. He at first foresaw them only as magnificent tools in the hands of a chosen few skilled in numerical analysis, and considered that in the full flower of their development one or two per nation would suffice for all imaginable needs. In later years he himself used to comment with some amusement on this spectacular misjudgement.

In the very early days, when the possible emergence of a new industry was being canvassed, the views of industry itself were sought, and the Federation of British Industries was asked to provide an independent opinion. Sir Norman Kipping, the able and energetic director of that august body, accordingly set about procuring one, and the results of his inquiries fell precisely into line with the opinion of Professor Hartree.

What was destined to be a computer industry accordingly came into being contrary to the prophecies of the best-informed opinions procurable at the time when the first computers were demonstrating their capacity.

6. U.K. and U.S. industry

Let me now say a word about the industrial situation in England and America at the time of which I am speaking – remembering that it is only ten short years ago.

Of the two punched-card systems, Hollerith's and Powers', the former had become the basis of a gigantic U.S. industry with world-wide ramifications: I.B.M. The latter was exploited by Remington Rand on a substantially smaller scale as part of a much larger business in typewriters and other types of business machines. The growth of I.B.M. under the energetic leadership of the Watsons, father and son, was characterized by a single-minded purpose, to expand endlessly in a specialised and delimited field. As a result of this they were early comers to the field of electronic computational circuitry and played a leading part in the design of many early card-sequenced devices of a partially electronic character. They were thus superbly placed to exploit the computer field when it was opened up.

Their principal competitors, Remington Rand, were not so placed, but reacted to the I.B.M. lead by buying up two electronic firms, Eckert and Mauchly, and E.R.A., which put them in a position to compete.

In England the industrial situation fell into quite a different pattern from the outset. The nation was not punched-card-minded so far as office administration was concerned, the consumption of punched cards per £1 of gross national product being noticeably low even by the standard of some European countries. Two firms of roughly comparable size competed with one another in terms of the Hollerith and Powers systems, and neither of them could be regarded as a giant comparable with I.B.M. Each had had a tough history of bare survival. Powers was the protégée and subsidiary first of its largest customer, the Prudential Assurance Company, then of Vickers-Armstrongs; the British Tabulating Machine Company was the licensee of I.B.M. itself, a position from which it gradually withdrew. The immediate post-war period found both companies in the typical post-war situation of a booming demand for run-of-the-mill products, a demand which had to be met despite all the post-war insufficiencies of materials, tools and trained personnel. Computers had in these circumstances to take second place to more important considerations, and had to be developed at a slower tempo than in the U.S.A.

7. The influence of defence expenditure

In addition to the contrasts that one could observe between the initial positions of the U.S. and U.K. industries, further differences developed as a result of the defence problems which the two countries had to tackle in the course of the cold war.

In speaking of defence I speak as a layman. I can disclose no secrets because I known none. One has only to look at the map, however, to perceive the implications of certain differences in geography. Britain is a small island. The U.S.A. is part of a great continent. The type of linked defence network of radar stations which is possible for the U.S. would be inappropriate for the U.K. to establish. In so far as it was a possibility, a radar defence network became, for the U.S., an urgency and, inasmuch as computers seemed to be ideal tools of interpreting and collating radar signals received, the development of defence computers became for the U.S. a matter of high priority. The result issued in a theoretical study *Project Lincoln* which evolved into a system *Project Sage*. The first result was an enormous expenditure of U.S. Government money on direct computer development, the computers themselves providing such value as the U.S. Government got for its money. No one knows how much was expended on this sort of development. A figure of 250 million dollars was commonly quoted some years ago, but on what authority I cannot say. No corresponding expenditure was incurred by the British Government, for the simple reason that no corresponding need existed.

It is a commonplace of aeronautics that military expenditure alone permits of the existence of civilian aviation. If civilian airliners had to carry the capital cost of all the know-how embodied in them, their capital price to the airline operations would put the latter out of business in competition with the railways and shipping lines. In a sense, therefore, one may say that civil aviation is in receipt of a large concealed subsidy – free use of the know-how established for military purposes. The subsidy is of course a virtual one; it is not a sum paid in cash, nor could its cash value ever be assessed. It is nevertheless real. In exactly the same sense we may say that a U.S. computer industry has received a large, virtual subsidy – free use of the know-how established in building defence computers. No such subsidy has ever been available to the computer industry in the U.K., and the contrast represents a very real advantage to the U.S. computer industry regarded as a competitive one. In terms of these considerations, the wonder is not that the U.S. industry is in some ways ahead of our own, but that our own industry can exist at all in competition with it. These facts should always be remembered in assessing the way in which our industry has grown. To emphasise them I will summarise them here as follows.

(1) *The initial position* found the British punched-card industry: (a) technically unprepared, (b) financially burdened with post-war reconstruction, (c) domestically preoccupied with activities other than computer development.

(2) *The subsequent position* found America's punched-card industry in receipt of a virtual subsidy for computer development.

This being the situation it was natural that the computer industry in the U.K. should originate as a branch of the general electronics industry. It would have been natural, that is, if one were to have regarded the manufacture of computers as an end in itself. In fact manufacture is merely a means to the end of effecting a sale whereby a profit is made, and the birth of the computer era found the general electronics industry just as unprepared to market computers as the punched-card manufacturers were to manufacture them.

8. Early impediments

Such was the situation as it gradually became clear to me during the early part of the decade. I express myself in the first person because I am drawing on my own thoughts and experiences, but you will appreciate that whatever view I came to was the result of thrashing it out with a growing band of colleagues – D. Hennessey, H. J. Crawley, C. Strachey and others who joined us from time to time as part of the computer team at the National Research Development Corporation.

It is interesting to read old Board papers and reports of this period. I find, for instance, that starting from June 1949, it took me six months to get the punched-card representatives round the table with the principal electronic manufacturers, for a discussion on the possibilities of some kind of syndicated operation of the kind that has subsequently proved so successful in the design and erection of nuclear power plant. Nothing came of it and, in retrospect, I think I can see why. The analogy with the nuclear power industry is to some extent a false one. A boiler maker is ready to admit that he is not a turbine designer, and a turbine designer that he is not a civil engineer. Such admissions are readily made in heavy industry, where the cobbler tends to stick rigidly to his last. They are less readily made in light industry. Any firm of electronic engineers would be reluctant to admit that it could not make a mechanical card reader, one reason for the reluctance being that the proposition would not be true; by engaging some assistance it probably could make a card reader, though it might find that it did not in fact want to do so when the time came. What no one was prepared to do in the early stages of the game was to legislate themselves out of any opportunities latent in a novel situation that was not really understood.

The possibilities of syndication appearing unachievable by the end of 1949, I find that I spent the first six months or so of 1950 trying to persuade various firms interested to undertake a serious enterprise on the commercial scale. In the autumn I crossed the Atlantic to see what was going on in the U.S.A. I found to my considerable surprise that the American user in the business field was immensely intrigued by the possibilities of utilizing computers, and that, as no computers were available commercially from U.S. manufacturers, U.S. users were quite open-minded about buying British if any supplies could be quoted. This was at a time of acute dollar shortage and it seemed to me that a snap market, suitably responded to, might result in permanent business. The EDSAC was rising two years old, and an engineered version of Professor Williams' machine in Manchester, commissioned by the Ministry of Supply with Ferranti Ltd, appeared to have been on the stocks for about as long. The operation therefore seemed 'on', though I was warned that U.S. reluctance to use British components might prove a severe obstacle. I find from the records of this visit that I was trying hard to persuade anyone in England to manufacture a small computer, to a relaxed specification, for sale in the U.S. market. Nothing came of these efforts. Policy makers still had to be convinced that a new industry was opening up, and responsible engineers still had to be convinced that making a computer to a commercial specification was other than an act of lunacy. This state of affairs lasted until about April 1951, by which time the first phase of my own activities – exhortation and encouragement to manufacturers – came to an end. Thereafter it became clear that conviction had been carried on the main issue, namely that there was going to be an industry and that a number of manufacturers were prepared to draw on the resources of the National Research Development Corporation to augment the investment they were prepared to make in it. I think the inauguration of the Ferranti Mark I computer at Manchester University, which took place about this time, played a significant part in this change of front. Meanwhile, as it transpired, the period from 1948 onwards had not been misspent, for a substantial amount of research and development had been proceeding on public contracts of one kind or another.

9. Magnetic tape

The next year or so found N.R.D.C. busy on the contracts it was placing with manufacturers. In the interim the first Univac came into operation in the U.S.A. and enlarged our horizons considerably from the standpoint of what could be done with magnetic tape. It soon became clear that we should have to sponsor some development work in this field, but it proved extremely difficult to get potential users to write a specification in respect of their requirements. I well remember a meeting convened in June 1953 to discuss this need. The late Dr Colebrook of the National Physical Laboratory expressed the sentiments of all present by commenting, 'Give us something to play with while we're thinking.'

We, accordingly, sponsored a contract to produce six tape-recorders similar to a promising design which Dr Wilkes have been working on in Cambridge. Alas for the plans of mice and men. Half-way through the contract it became apparent that the design was just what was wanted for data recording at Woomera, and an appeal, that we could not well resist, was made to allow diversion of supplies thereto. It was our original intention that one machine should be issued on loan to each of the main centres of computer development, but this plan was thrown into disarray, and by the time the machines were delivered their design was lagging on contemporary developments and they arrived too late to influence thought. The general failure of the industry to produce a satisfactory tape-deck, so far, has had regrettable consequences and I think the reason is worth analysing.

It is worth analysing because I do not see why our belief in the law of cause and effect should be abandoned when sitting down to an administrator's desk. I remain an optimist with respect to the principle that if people can be brought to understand things they will end up by controlling them. Briefly, then, an electrical engineer is one sort of person, and a mechanical engineer is another. Each tends to be somewhat amateurish in the field of the other's expertise, and this amateurishness shows up very clear when an assessment is called for as to whether a job is easy or difficult. Such an assessment only too often takes the form of supposing that anything which looks easy is easy, a piece of self-deception which is all the more dangerous because it is quite often true. In the odd case where it is not, the amateur is then committed to endless frustration, the cause of which is elusive because the solution always appears to be round the corner. In fact, the construction of a good tape transport mechanism is a difficult and tricky piece of not only mechanical but aerodynamic engineering, and the main reason for their failure to produce a viable solution of the problems involved is that electrical engineers have refused to recognise that fact and tackled their problems in a spirit of facile optimism. As a result we have not a single tape-deck in the U.K. able to compete with U.S. equipment, and, when I enquire the price of the best competitor to it, I am shocked to be quoted for the equivalent of one and a half *Continental* Rolls-Bentleys with Hooper coachwork and purchase tax! If the chief designer of the *Viscount*, *Comet* or *Britannia* could have been inspanned from the beginning, at any price he chose to name, it would have been an economy in the long run and tape-decks would be half the price they are today!

10. Catching-up

In 1954 I spent a month in the U.S.A., bringing my knowledge up to date and checking up on the alarming reports I was getting with respect to the progress that was being made there. I saw the first of the I.B.M. 700 series as pioneers of the second generation of machines, and returned full of determination to try and restore the balance if possible.

It seemed to me that direct competition with such machines would be a tactical blunder, and that we ought to try to skip a machine generation by going straight forward to the transistorised produced of the future. In this mood I composed the following specification, copied from a record in one of my files.

(1) A data-processing *system*, taking full advantage of the amount of thought that has gone into this matter in the United States.

(2) Specifically, a system working in alpha-numeric code based upon the use of cores and transistors throughout.

(3) A rapid-access store, based on cores, of approximately 10,000 words.

(4) Backing store on drums and/or tapes and/or discs. The logic of the mechanism to be such that the units in the backing store can be added to, Meccano-wise.

(5) The construction to be unitised by being broken down into functional blocks as far as possible.

(6) Component functional units to include every means of conversion between computer input/output and the output/input of commercial devices such as Hollerith machines, Powers machines, five- and seven-hole teleprinter tape, together with commercial printing devices such as Creed teleprinters, Olivetti typewriters, Powers and Hollerith tabulators, and Bull or Shepard line printers.

(7) Analogue/digital and digital/analogue converters to be developed in association with the other input/output devices, together with graph trackers, plotters, etc.

This specification, reproduced here as I originally wrote it, came remarkably close to a design later sponsored by N.R.D.C. and embodied in a machine recently announced as the EMIDEC 2400.

11. Advance in logical design

I must return, however, to the period of 1953/54 during which C. Strachey of N.R.D.C. was labouring on the design of a new logic for a drum-type machine, later to become well known as Pegasus.

Pegasus was the first machine to incorporate this and to be dominated by the logical designer with experience of what the user needed. Previous designs and endeavoured to express that it was believed the user wanted, but as user experience was still embryonic there was but a slender link between what he wanted in his mind and what he needed in his practice. In these circumstances logical designs had tended to be dominated by electrical engineers who selected what was convenient to build rather than what it was necessary to incorporate, a natural state of affairs in the early days when the functioning of any sort of a computer whatever was something of an engineering miracle. From the programmer's point of view Pegasus proved a popular favourite, and a number of Pegasus-like features appear in the logical design of the EMIDEC 2400. This latter goes a long way to meet my 1954 plan for skipping a machine generation on the commercial side by passing directly over to fast transistorised circuits. Whether it is regarded as the last machine of the second generation or the first of the third depends upon the point of view. It appears to me to possess a definite capacity for time sharing and parallel programming; in terms of Dr Wilkes' Presidential Address it would, therefore, appear to have a claim to third-generation status.

Arrangements for the EMIDEC 2400, and the repercussions of marketing the Pegasus, preoccupied me during 1954/55.

12. Very fast computers

By 1956 I had a new problem to cope with. A number of super-fast computer projects began to be undertaken in the U.S.A. – LARC, Stretch and so on. Were we strong enough to compete? Ought we to try? Could we afford not to? Could any such proposal be established on a commercial basis? During the last two years I have unsuccessfully wrestled with divided counsels on all these issues. I would dearly like to see the effort made but, so long as I can only act by persuasion, and so long as every potential collaborator remains unpersuaded, I cannot begin. N.R.D.C. can endow contractors and collaborators with money. It cannot endow them with unanimity!

13. Conclusion

Such is the tale of my own comings and goings during the greater part of the first decade. I have already asked you to excuse my use of the first person in recounting it. In an hour's lecture it is not possible to do more than highlight the main events of ten years. For this reason I have omitted everything with which I was not directly concerned, such as the introduction of core storage and the magnificent pioneering of the LEO team. I have also omitted an account of the N.R.D.C. project at Siemens Bros Ltd, where an Elliott 405 has been installed for production scheduling and stock control, the initiative in this case having been taken by Hennessey and Crawley, rather than by myself. In passing it all under review I cannot refrain from asking myself what lessons there are to be learned. It is often said that Britain is first class at making inventions and much less good at industrialising them. The industrialisation of computers, regarded as an invention, is an interesting test case. My concern has been to take what mathematicians teased engineers into doing for fun, and convert it into the basis of an industry by using money, either in the form of grants made or grants withheld, as a stimulant rather than a narcotic. I think the first lesson is that to dismiss Britain as good at inventions but bad at industrialising is an over-simplification. An invention is born perforce into some pre-existing industrial situation which may or may not favour its spontaneous development. I see little to cavil at in the way we have industrialised the jet engine, radio-navigational aids, or nuclear power. The computer was born to both Britain and the U.S.A. in approximately the same hour. The industrial situation in the U.S.A. was initially and subsequently far more favourable than it was in the U.K. The situation in the U.K., however, had nothing intrinsically wrong with it save in the context of computer development. If computers had not been born, no particularly unfavourable comparison would have been made now as to the industrial structure in the U.K. and U.S. It is easy to be wise after the event. Where I feel more disposed to be critical is in the field of use. The American user has supported the American computer manufacturer consistently and enthusiastically from first to last, by queueing up with orders for supplies. In Britain he has hung back waiting to see a new idea tried out on the dog. How far this is a matter of national temperament, and how much it depends on the amount of money available for speculative investment, I cannot say, but I suspect that national temperament does play a part, and a big one at that. Even manufacturers of computers have been slow to apply, for internal use, the machines they were endeavouring to sell potential customers for their own. And then there is a third factor. The background patter comes into it yet again in the field of customers' attitudes. They expected to have their O and M service along with supplies of hardware, as they had been accustomed to. Delay in doing this contributed to the 'hang-back' atmosphere.

Indecision of this kind, a lack of zeal for novelty, and a reluctance to pioneer, are demoralising for the manufacturer. They leave him with time on his hands in which to fight frog-mouse battles about alternative design criteria, instead of getting on with one or the other, so that finally he is left with, for example, a computer but no viable tape-deck. If lack of consumer zeal is the real handicap what can a body like N.R.D.C. do? It can stimulate but cannot revolutionise, for it is not itself a consumer. The Atomic Energy Commission in America commissioned Stretch for its mysterious purposes, whatever they may have been; in doing so it acted as a pioneer consumer. If the Atomic Energy Authority (or any public body for that matter) declines to show the same initiative in Britain as its counterpart in the United States, to whom else are manufacturers to look for an order? If, in due time, the Atomic Energy Authority, say, decides that a Stretch-like machine is essential for its purposes after all, will it then be able to afford the delay in commissioning one from a British manufacturer? If not, it will presumably acquire a copy of Stretch from America and that will be that. Users will criticise British manufacturers for backwardness. They will not criticise themselves and one another for failing to catalyse forwardness.

I do not wish to end on any such note of gloom, however. In comparing the British and American computer efforts we must remember that we are comparing developments in two economies which differ by a scale factor of 1:10. In the field of the larger machines, such as the Ferranti Mark I* and the DEUCE, our production does not seem to have been out of line with American production by much more than the scale factor, and in view of the difficulties entailed by the original difference in industrial patterns, I find that an encouraging thought; for it means that in the first decade a small industry has come into being and has more or less held its own. The real struggle seems booked for the second decade.

Chairman's remarks

The following points was made by the Chairman Dr. F. Yates (Rothamsted Experimental Station) in opening the meeting and moving the vote of thanks to the author when the paper was presented to the British Computer Society in London on 16 October 1958.

The National Research Development Corporation have done much to advance the development of electronic computer in this country. They have also taken an active part in the setting up of the British Computer Society, and they have done a very great deal to assist us by the provision of office space and encouragement of the work on our publications. My own Department owes a special debt to the Corporation in that they were instrumental in securing us our machine, the Elliott 401, which was built under an N.R.D.C. development contract. The Corporation has also recently installed a Pegasus in this College (the Northampton College of Advanced Technology).

The author has referred to the reluctance to utilise computers he has encountered in certain fields. I have found that this reluctance afflicts my own field of research statistics. To me it has always seemed that a general-purpose computer is the computing tool of which statisticians have been dreaming all the years they have been working desk calculators. When, however, we acquired a machine at Rothamsted, I found that my enthusiasm was regarded by my fellow statisticians with the greatest suspicion, and even horror. One very eminent colleague, who is indeed younger than myself, remarked 'I may be old-fashioned, but I do like to see the figures I am analysing'. This criticism of electronic computers would have more substance if in fact statisticians did themselves personally analyse large bodies of data, actually, of course, a great deal of the heavy computing work is done by assistants of various kinds. What is required is the development of new techniques of computing which will take advantage of electronic computers while still permitting adequate contact with the computation process by the statistician. It is significant that the late Chairman of the Agricultural Research Council, impressed by the large amounts of numerical material that are piled up in the course of research and never adequately analysed, has recently expressed the view that far more electronic computation was required in agricultural research.

I was interested in Lord Halsbury's view on the relative merits of small and large machines, particularly having regard for Dr Wilkes' comments in his Presidential Address. Dr Wilkes made the point that large machines are expected to be substantially more economic per unit of computation if devices such as parallel programming are made use of. In spite of this I have a feeling that there is likely to be a place for some time to come for small and moderate-sized machines, so that one can have units dispersed around the country that are freely available to small organisations.

The author (*in reply*): I think one can overdo the economic aspects of sharing large computers. I have in my desk a slide-rule which cost 25*s*. before the war and which spends most of its time idle; if somebody suggested I could get double value from it by sharing it with someone in the room next door, I would reply, 'I am prepared to pay a premium to have it when I want it'. For that reason I think the small machine will always have a certain use in companies of a certain size. The problems of sharing a large machine between several autonomous users are not just technical problems concerned with parallel programming and time-sharing; they involve problems of conviction and competence, and the secrecy of data supplied by different clients for the use of one machine.

Competence in the technical activity of putting several jobs on to the machine and sharing time by micro-seconds does not overcome sociological problems and business administration problems. These must be solved, however, before the type of commercial time-sharing which Dr Wilkes foresees can come about. Therefore, for a long time to come, I believe there will be a future for the small machine.

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Short Notes

Implementation of Karp–Luby Monte Carlo Method: An Exercise in Approximate Counting

Richard Karp and Michael Luby introduced a powerful framework for the construction of Monte Carlo algorithms to solve hard counting problems [cf. *Journal of Complexity* 1 (1), 45–64 (1985)]. They then applied it, as a special case, to the problem of counting the number of satisfying truth-value assignments for a Boolean formula in disjunctive normal form. In this paper, we describe an implementation of that algorithm. Our experiments show that it indeed works very well in practice.

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1. Introduction

Counting is difficult. To quote Valiant:[1]

'Numerous problems in the mathematical and physical sciences can be reduced to questions of counting solutions in combinatorial structures. Much effort has been put into developing analytic techniques for doing this effectively for the various problems that arise most frequently. A glance at the literature, however, suggests that the search for positive results has had only very limited success, and that for the majority of questions we still cannot count exactly in any effective sense'

Similarly, while estimating the number of simple (non-self-intersecting) paths between two corners of a grid graph, Knuth observes:[2]

'Of course, I have only generated an extremely small fraction of these paths, so I cannot really be sure; perhaps nobody will ever know the true answer.'

In complexity theory, the notion of # P-completeness formalises the difficulty of counting problems. Introduced by Valiant, this class typically contains problems polynomially equivalent to the counting problems associated with many NP-complete problems such as counting the number of Hamilton circuits in a graph. The reader is referred to Gary and Johnson,[3] and Valiant[4] for the fundamentals of NP- and # P-completeness. Angluin[5] and Stockmeyer[6] offer several theoretical results about # P. Problems which are # P-complete are at least as hard as NP-complete problems, making it unlikely that a polynomial algorithm exists to solve them.

Fortunately, the prospects are not as dim as they look, mainly due to tools like Karp and Luby's innovative Monte Carlo framework that offers an attractive alternative for several problems of this sort.[7] (Hammersley and Handscomb[8] give an overview of Monte Carlo techniques. Fishman[9] studies such techniques in reliability area.) Using randomisation, Karp and Luby propose fast algorithms which report an 'almost correct' answer 'almost surely', provided that one is willing to spend an extra effort to make the quoted notions more and more refined. Specifically, such an algorithm returns after a polynomial effort in the problem size, ε, and δ, an answer A^* for a counting problem whose exact answer is A such that

$$\text{Prob}\{A^{-1}|A^*-A| > \varepsilon\} < \delta$$

Here $|\,|$ denotes the absolute value.† In other words, the method computes the answer within *relative error* at most ε and attaches to it a confidence value of at least $1-\delta$. Statistically speaking, only 100δ percent of the time the returned result would not obey the relative error bound. Here. ε and δ are small positive constants specified by the user. Note however that such algorithms generally have to make $(p\varepsilon^{-1}\log\delta^{-1})^{O(1)}$ trials where p is a measure of the problem size. Thus, while one can theoretically choose ε and δ as close to zero as required, it is prohibitively expensive to use very small values.

In this paper, we demonstrate that this is not a serious issue since, using liberal values like $\varepsilon = 0.1$ and $\delta = 0.1$ will provide the almost right answer. Therefore, there is essentially no need to resort to conservative values like say, $\varepsilon = 0.001$ and $\delta = 0.001$. Specifically, we describe an implementation of a Monte Carlo algorithm by Karp and Luby for counting the number of satisfying truth-value assignments for a Boolean formula given in disjunctive normal form.[10] For brevity, we shall frequently cite results from Ref. 10 without elaboration. The reader is asked to consult that paper for a complete description.

2. Problem definition, notation and Karp–Luby Algorithm

We closely follow the notation of Ref. 10. Let $X = \{x_1, x_2, \ldots, x_n\}$ be a set of Boolean *variables*. Thus each x_i can be either 0 or 1. The members of $X \cup \bar{X}$ where $\bar{X} = \{\bar{x}_1, \bar{x}_2, \ldots, \bar{x}_n\}$ are called *literals*. Here $\bar{x}$ denotes the complement of x. A *clause* is a logical 'and' of a set of literals. A *disjunctive normal form (DNF) formula* is the logical 'or' of a set of clauses. A *truth-value assignment* is a function f from X to $\{0, 1\}$. A truth-value assignment f is a *satisfying truth-value assignment* for a given DNF formula F if F evaluates to 1 upon substitution of $f(x_i)$ for each variable x_i.

Let F be given as $\bigcup_1^m C_i$ where C_i's are the clauses. Let N_F denote the number of truth-value assignments satisfying F. In this paper we shall deal with the problem of computing N_F exactly or approximately. (In the latter case it will be denoted by N_F^*.) This problem will be called CNTSAT in the sequel. It is known that CNTSAT is # P-complete. Exact but inefficient ways of computing N_F will be postponed until Section 3. Now we shall briefly summarise the Karp–Luby algorithm to compute N_F^*.

† In the sequel, it will also be used to denote the number of elements in a set but no confusion will arise.

The algorithm of Karp and Luby to approximately solve CNTSAT is based upon the following crucial observation:

Let the universe S be the set of all tuples (i, x) such that x is a truth-value assignment yielding $C_i = 1$. Let R be the set of all those tuples (i, x) such that C_i is the lowest-numbered clause satisfied by x. Then $|R| = N_F$.

A *trial* of the algorithm consists of drawing a member of S randomly and testing whether it lies in R. Let us assume, without loss of generality, that the clauses contain no contradictory pair of literals or no repetitions of the same literal.

The algorithm starts initialising N_s to 0 and computing $|C_i|$'s and $|S|$. (By a slight abuse of notation, $|C|$ denotes the number of satisfying truth-value assignments for clause C.) Computing the former values is trivial, i.e. $|C_i| = 2^{n-k}$ where k is the number of literals occurring in C_i. (Remember our assumption in the preceding paragraph.) It is also noted that $|S| = \Sigma_1^m |C_i|$ and $|R| = |\bigcup_1^m C_i|$. Thus $|S|/|R|$ is at most equal to m. This bound is essential since using Bernstein's inequality Karp and Luby prove that a total of

$$N = \text{ceil}(|S||R|^{-1}\ln(2\delta^{-1})4.5\varepsilon^{-2})$$

Trials would be required for the Monte Carlo experiment. At each trial, the algorithm computes a tuple (i, x) randomly, as noted above. It then determines the lowest-numbered clause C_l satisfied by this x. If $l = i$ it increments the number of successful trials N_s by one. The final step of the algorithm is to report $N_s N^{-1}|S|$ as the answer N_F^*. By the nature of the method, this answer is guaranteed to be 'good' by the formula

$$\text{Prob}\{N_F^{-1}|N_F - N_F^*| > \varepsilon\} < \delta$$

Thus, with probability at least $1-\delta$, the value N_F^* reported by the algorithm is a fine guess for N_F, i.e. it is off at most with relative error ε.

As for the complexity of the algorithm, Karp and Luby proved that $O(m^2n)$ is the