
James Lovelock


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ARCHER JOHN PORTER MARTIN CBE

1 March 1910 — 28 July 2002
We judge the worth of a scientist by the benefits he or she brings to science and society; by this measure Archer Martin was outstanding, and rightfully his contribution was recognized with a Nobel Prize. Scientific instruments and instrumental methods now come almost entirely from commercial sources and we take them for granted and often have little idea how they work. Archer Martin was of a different time when scientists would often devise their own new instruments, which usually they fully understood, and then they would use them to explore the world. The chromatographic methods and instruments Martin devised were at least as crucial in the genesis and development of molecular biology as were those from X-ray crystallography. Liquid partition chromatography, especially in its two-dimensional paper form, revealed the amino acid composition of proteins and the nucleic acid composition of DNA and RNA with a rapid and elegant facility. Gas chromatography (GC) enabled the accurate and rapid analysis of lipids, which previously had been painfully slow and little more than a greasy sticky confusion of beaker chemistry. Martin’s instruments enabled progress in the sciences ranging from geophysics to biology, and without him we might have waited decades before another equivalent genius appeared. More than this, the environmental awareness that Rachel Carson gave us would never have solidified as it did without the evidence of global change measured by GC. This instrumental method provided accurate evidence about the ubiquity of pesticides and pollutants and later made us aware of the growing accumulation in the atmosphere of chlorinated fluorocarbons, nitrous oxide and other ozone-depleting chemicals.

If all this were not enough to glorify Martin’s partition chromatography, there is the undoubted fact that its simplicity, economy and exquisite resolving power transformed the chemical industry and made possible so many of the conveniences we now take for granted.
FAMILY LIFE AND EDUCATION

Archer was born on 1 March 1910 in North London, one of the three daughters and two sons of Dr W. A. P. Martin, a general practitioner, and Mrs L. K. Martin, a nurse. The family came from Northern Ireland.

Archer’s childhood was spent in North London; he did not read until the age of eight and he attended Oaklands School in Crouch End between the ages of nine and ten. The family moved to Bedford in 1920 and Archer went to Bedford School from 1921 to 1929. When still a schoolboy he was exceedingly interested in chemistry and read his elder sister’s university text books; by contrast, he did not remember learning any chemistry taught at Bedford School because, he said, he was so far ahead of what he was supposed to be learning in physics and chemistry. He took great interest in distillation—distillation columns particularly impressed him—and while still in his teens he built a five-foot-high distillation column made of coffee tins and filled with uniform-sized lumps of coke. He won an exhibition to Peterhouse, Cambridge, in 1929 and his intention was to become a chemical engineer. He had by that time already found several books describing the chemical engineering side of distillation columns, and noted that industrial research on the preparation of good columns was much in advance of laboratory research.

The distinguished geneticist J. B. S. Haldane (FRS 1932), who was then a reader in bio-chemistry at Cambridge, touched Martin’s life, as he did the lives of other famous scientists; and he persuaded Martin as a student, to move from chemical engineering to biochemistry. After graduating with a lower second degree he worked for a year in the physical chemistry laboratory, with F. P. Bowden (FRS 1948) and C. P. Snow (now better remembered as a novelist). In 1933, with Haldane’s backing, he took a research post in the university’s Dunn Nutritional Laboratory. Here, under the supervision of L. J. Harris and Sir Charles Martin, he constructed a countercurrent distribution apparatus with which he separated vitamin E into three distinct fractions. He then started work for Sir Charles Martin, who was not related, on the anti-pellagra factor in pigs and for three years was responsible for the welfare of 30 pigs; he had to feed them and keep them clean. Other graduate students have known worse, and in 1936 he received his doctorate. Archer met Judith Bagenal when he was working at the Wool Industries Research Association in Leeds, and they were married in 1943. They had five children: two boys and three girls.

AFTER CAMBRIDGE

Sir Charles Martin introduced him to his next collaborator, Richard Synge (FRS 1950), and together they built a 40-stage countercurrent apparatus for the separation of amino acid derivatives. They took this apparatus with them to the Wool Industries Research Station, at Headingley, near Leeds, where together they did the work that led to their seminal paper on partition chromatography. Between 1946 and 1948 he took the post of director of biochemical research at the pharmaceutical company, Boots Pure Drug Company, in Nottingham, and from here he went in 1948 to the Medical Research Council (MRC) laboratory at the Lister Institute in London. Here he had the good fortune to have Tony James (FRS 1983) as his colleague. Tony took on Dick Synge’s role and provided the help that enabled Archer to reduce GC to practice. Tony James continued to provide this essential role of caring colleague when they
both moved from the Lister Institute to the National Institute for Medical Research at Mill Hill in London. The years at the Mill Hill Institute were among the most productive and satisfying for Archer and for all of us there who met him and benefited from his wisdom. Martin flourished when there was someone, usually a colleague, to help him cope with the minor but, for him, onerous details of the work environment. He was glad to share the task of writing papers with his collaborators. His friends remember Archer for his aphorisms, one of which was ‘Nothing is too much trouble, provided always the trouble is taken by someone else.’ Two things I shared with Archer were dyslexia and Tony James as a colleague. Dyslexics find the linear world of sequential expression, so necessary for written examinations or scientific papers, difficult, even baffling. We both benefited from having this fluent and understanding colleague as a friend.

THE GENESIS OF PARTITION CHROMATOGRAPHY

The Russian botanist Mikhail Tswett discovered the technique of chromatography in 1903 when he separated the pigments from plants by washing them down a column of powdered chalk with an organic solvent. Chemists were aware of the process but it was not general or reliable enough to win their confidence as a way of separating natural or synthetic mixtures. Martin’s first reported thoughts on chromatography were as a graduate student at Cambridge. His colleagues were interested in the carotenes, and in 1933 Dr A. Winterstein from E. Kuhn’s laboratory in Heidelberg visited Cambridge and demonstrated a chromatogram of a crude carotene solution on a chalk column; the carotene separated appropriately into bands of various colours. Martin said that he was fascinated to see the relationship between the chromatogram and distillation columns and to realize that the processes involved in the separation of the carotenes and of volatile substances by distillation column were similar; there was relative movement of the two phases and their interaction at many points gave rise to good separations.

Martin delivered the opening address at a CIBA Foundation meeting in London in 1969 on the medical applications of chromatography. His talk was moving and personal and it fluently recorded the genesis of partition chromatography in its several forms. I will quote passages from this vivid account of his research, but it is available in full in the book Gas chromatography in biology and medicine, published by Novartis. Here is how he first approached the problem of separating vitamin E from vegetable oil:

I had always been interested in engineering processes and so I started to devise machines to do the counter-current extractions. The first machine was designed for the first stage in the separation of Vitamin E; vegetable oils were saponified and the soaps extracted with ether. This was a tedious, smelly job. So I put one twenty-litre aspirator bottle on the floor outside the laboratory and another on the flat roof (the laboratory was a single-storey building). I filled the top bottle with soaps and the bottom bottle with ether and joined the bottom of the top bottle to the top of the bottom bottle with half-inch bore tubing. By this means the ether and soaps changed places over a period of hours (over night, in fact). I found that ten feet of tubing gave about eight theoretical plates and I obtained very efficient extraction in this way. This method was satisfactory for extracting a particular substance from one liquid to another but much more was needed to separate two or more substances of closely similar partition coefficients. It was by no means obvious how one could duplicate the performance of a batch distillation column. Devising a still to evaporate the liquid leaving the column, and continuously dissolving the residue in the other liquid phase was not easy. I can still remember the delight of realizing (while walking home to lunch) that all that was necessary was to inject the substance to be separated...
into the centre of the column and fix the ratio of the flow rates of the liquids so that they equaled the reciprocal of the partition coefficient. The liquids flowing in at the ends of the column then carried the wanted substance back to the centre of the column and allowed it to escape only very slowly. But other substances, with higher or lower partition coefficients, left more or less rapidly at one end or the other.

Later in his lecture he recalls the steps that led to partition chromatography:

I continued designing new machines that I hoped would be more satisfactory, but although I worked out some dozens of ideas none of them produced a machine that was sufficiently cheap and easy to seem worth making. In 1940 it occurred to me that the crux of the problem was that we were trying to move two liquids in opposite directions simultaneously. Equilibrium had to be established rapidly or the experiment took far too long, but this meant converting the liquids to very fine droplets and if the droplets were too small they would not settle out or move in the required direction within any reasonable period of time. This meant that the machine was bound to be a compromise unless I could either introduce centrifugal force to speed up the movement of the droplets or think of a completely different system. Then I suddenly realized that it was not necessary to move both the liquids; if I just moved one of them the required conditions were fulfilled. I was able to devise a suitable apparatus the very next day, and a modification of this eventually became the partition chromatograph with which we are now familiar. Synge and I took silica gel intended as a drying agent from a balance case, ground it up, sieved it and added water to it. We found that we could add almost its own weight of water to the gel before it became noticeably wet. We put this mixture of silica gel and water into a column, put the acetylamino acids on to the top and poured chloroform down the column. We wondered how we should know where the amino acids were in the column and when to expect them to emerge at the bottom of the tube. By the end of the first day there was no sign of them. To find out what was happening in the column we added methyl orange to the liquid on the silica gel and thus were able to see the acetylamino acids passing down the column as a red band. One foot of tubing in this apparatus could do substantially better separations than all the machinery we had constructed until then.

Synge and I, in our first paper on partition chromatography (Martin and Synge, 1941) had evolved a theory relating the speed of the zones to the partition coefficient. Further, by introducing the concept of the theoretical plate for chromatograms, a prediction could be made about the shape of the zones and their rate of broadening. Later, after work with peptide paper chromatograms, I found it possible, by assuming that the free energy of transfer of a compound from one phase to another was an additive function of the free energies of individual atoms or groups of atoms, to forecast with reasonable accuracy the partition coefficient and chromatographic behaviour of peptides and many other substances.

PAPER CHROMATOGRAPHY

In spite of all their efforts with the silica gel columns they could separate only the amino, monocarboxylic acids. The separation of basic and acidic amino acids was intractable. They looked for materials other than silica to hold the water, and their first choice was paper. Paper chromatograms of dyes were familiar, and paper certainly absorbs water, so it was an obvious choice. Dr A. H. Gordon, who was now working with them at Leeds, suggested that they use the colour reaction of amino acids with ninhydrin to reveal by a blue colour the positions of the separated amino acids. It was not long before they had developed paper strip chromatography on filter paper. The analysis was made by placing a drop of mixed amino acids onto one end of the paper strip and then allowing a solvent, usually butanol or a butanol–water mixture, to flow by capillary action along the strip. The paper strips were in boxes in which the air was kept saturated with water—with troughs containing the mobile solvent into which the tops of the strips could dip. Several boxes were needed because it was characteristic of the method that, although it was not particularly quick, very little work was needed to run many strips
simultaneously. After the chromatogram had run, the paper was dried and the separated amino acids were revealed as blue patches after spraying with ninhydrin solution. An important next step was to run the chromatogram in two dimensions. The first solvent spread the amino acids in a line near one end of the paper from a spot near the corner; then, after drying, they turned the paper through a right angle and spread the line of spots into a two-dimensional pattern by using a different solvent. With this simple and inexpensive technique, biochemists could analyse the complete amino acid composition of a protein or a peptide. It opened for them a vast new world of research; it was the technique that enabled Frederick Sanger (FRS 1954) to unravel the amino acid composition of insulin, for which he received his first Nobel Prize in 1958.

**GAS CHROMATOGRAPHY**

Martin and Synge discussed the possibility of GC in their second joint paper in 1941, but it was not until Martin moved to the National Institute for Medical Research (NIMR) at Mill Hill in North London that he and Tony James developed, and reduced to practice, the use of a gaseous mobile phase in chromatography. In their 1941 paper, Martin and Synge predicted that, if the stationary phase in GC were a liquid, very refined separations of various kinds of compounds would be possible. Although this paper was widely read by chemists in the petroleum industry, no one thought this prediction worth testing experimentally until nine years later, in 1950, Martin and James started to work on gas–liquid chromatography. Again we have Martin’s words delivered at the CIBA Foundation meeting in 1969:

James and I tried to separate our materials using crystallization on a column—what is now known as zone-refining. This project looked hopeless for a few months, and James became more and more discouraged; we could do much better with a couple of beakers than with all the complicated apparatus we had constructed. So (to improve James’ s morale) I suggested that we study gas chromatography; I was sure this would work. Professor J. Popjak had asked me for a more refined method than paper chromatography for separating fatty acids and I thought that gas chromatography might be able to do this. So we spent our first week waiting for the bands to come out of a gas chromatograph: in fact, they had all come out in the first few seconds. We used quarter-inch-bore glass tubing, about 15 inches long, packed with Celite (which had been found to be the most convenient material to use with liquid–liquid columns). We passed nitrogen in at one end of the column, the other end of which was provided with a capillary that dipped into a test-tube containing indicator solution. A small conical flask, instead of a burette, held the titrant. The flask had a doubly bored stopper, one hole carrying a tube that passed from the bottom of the flask to a jet just above the level of the liquid in the test-tube, while the other hole had a piece of valve rubber attached that could be milked between finger and thumb to express a drop of titrant from the jet. James sat with a stop-watch and a piece of graph paper and timed and plotted the drops while I watched the test-tube and put in a drop of titrant whenever the colour of the indicator changed. Plotting the number of the drops against time yielded a series of steps. The height of the steps denoted the quantity of acid emerging, and their position on the time axis showed the retention time. We first separated the methylamines, since they would run at room temperature. Later, using a steam jacket for the column, the other end of which was provided with a capillary that dipped into a test-tube containing indicator solution. A small conical flask, instead of a burette, held the titrant. The flask had a doubly bored stopper, one hole carrying a tube that passed from the bottom of the flask to a jet just above the level of the liquid in the test-tube, while the other hole had a piece of valve rubber attached that could be milked between finger and thumb to express a drop of titrant from the jet. James sat with a stop-watch and a piece of graph paper and timed and plotted the drops while I watched the test-tube and put in a drop of titrant whenever the colour of the indicator changed. Plotting the number of the drops against time yielded a series of steps. The height of the steps denoted the quantity of acid emerging, and their position on the time axis showed the retention time. We first separated the methylamines, since they would run at room temperature. Later, using a steam jacket for the column, we separated the first members of the fatty acids series. Initially we used a fatty, oily material, but this gave very distorted bands. I had enough experience with chromatography by this time to realize that non-linear absorption creates tailing. But on these plots we had the reverse of tailing—a long front and a sharp tail—which we eventually realized was due to the association of the fatty acid to dimers; in other words, dimerization was a considerable problem to us for six months or more. By adding a soluble acid (such as stearic acid) in excess to the stationary-phase liquid, we were able to sort out this difficulty and obtain reasonably shaped bands. This technique worked very well for fats and oils and equally well for amines. We obtained our first useful results six weeks after starting the experiment. This was the beginning of gas chromatography for us.
The details still had to be worked out, but it is really astonishing how closely similar this first column was to many columns still in use today. We wanted to illustrate the technique by using it to separate some natural mixtures, so we tried to identify the amine responsible for the fishy smell of stinking goose-foot (*Chenopodium vulvarium*). We found trimethylamine in this plant and were able to separate the three methylamines and ammonia quite readily from it.

**MILL HILL**

It was not until Martin joined the staff of the NIMR that the scientific community began to appreciate the magnitude of his contribution. The Royal Society elected him to the Fellowship in 1951, in what some of us thought was only just in time, before he and Richard Synge received the Nobel Prize in recognition of their researches in partition chromatography. Sir John Cornforth FRS, who was present at the party given in Martin’s honour at Mill Hill, recalls him saying that it was much like winning the football pools. The Secretary of the MRC, Sir Harold Himsworth (FRS 1955), praised Martin in Latin, saying ‘*ex Martino semper novi aliquid*’ (‘Martin can always get new things from a liquid’) — a neat corruption and mistranslation of the old Latin tag *ex Africa semper novi aliquid* (‘from Africa always something new’).

I had known this truly modest man, Martin, as a scientist at the NIMR and we had conversed briefly on general topics at tea or lunch in the canteen but it was not until 1956 that I had my first encounter with him as a potential colleague.

I had the need to analyse tiny quantities of fatty-acid methyl esters from cell membrane lipids. I knew of the superb resolving power of Martin’s gas chromatograph and wondered if he would be interested in analysing my mixture. I went to see him at about coffeetime one morning. He was in his laboratory on the third floor of the institute at the left-hand end facing Mill Hill Broadway. It was a rectangular room with a large island bench in the centre, festooned with equipment. He was surrounded by a group of Institute staffers including Tony James, Rosalind Pitt-Rivers (FRS 1954), George Popjak (FRS 1961) and Joan Webb, and they were all busily completing the *Times* crossword — what might now seem to be a waste of work time and at tax payer’s expense. The louche appearance of the laboratory was deceptive; Mill Hill was a most disciplined institute and firmly controlled by its director, Sir Charles Harington FRS. We were elite and we had to produce, but how we did it was our own affair. I saw them as being like Olympic athletes warming up before a contest. They welcomed me and immediately pressed me into finding the answer to an obscure crossword clue ‘Who is in the cock Edward?’ Nine letters long and it turned out typically, of a cryptic crossword puzzle, to be the archaic and arcane word ‘cowhocked’. Imperceptibly the conversation turned to science and soon I was asking for Archer’s help with my problem.

Working hours at the institute were flexible, but most worked longer than was required by their contracts. Absent were the planning meetings and bureaucratic requirements of today’s science; there were no brainstorming sessions and we were, whether alone or in collaboration, expected to find our own solutions to our self-generated problems. Many of these productive collaborations and exchanges of ideas began in an informal talk over a cup of tea or coffee. The NIMR was a place of great but firmly constrained freedom. I am grateful to Sir John Cornforth for reminding me in an e-mail message how unusual the NIMR was. He writes:

> But what a place the NIMR was to bring on partnerships crossing old disciplinary lines. We give Sir Charles Harington all the credit for this; he centralized the power in himself and resisted any attempts to create indi-
individual fiefdoms, but he used his power not to impress his own preferences but to encourage the widest collaboration. Within a year of joining the NIMR I found myself with at least three projects where chemistry was not an end in itself but an essential part of a wider and more exciting whole. You could always find someone with the expertise that was wanted and people were always finding you with problems that you could help to solve. Archer’s lab was a powerhouse, extending so much the power of so many people. George Popjak, who was working on fatty acid biosynthesis, used Archer’s chromatography for separating homologous acids, and when we were doing the degradation of radioactive cholesterol the method was invaluable. I remember when Joan Webb complained of the tedium of collecting small liquid fractions from the column effluents; Archer designed an automatic fraction collector that enabled her to put her feet up. This collector was made in the Institute’s workshops, with minimal delay and on the spot consultation about the details of the design. It is no wonder that so many discoveries and so many techniques flowed out in those years; you knew that you were free to think and to collaborate with people whom you could convince, or who could convince you, that the work was worth doing. You did not have to pretend that you knew what the result would be like, or predict what methods of resources you would use, before you were given the resources to begin.

The NIMR was a tribal group with Sir Charles as its unquestioned leader. I well remember, as a relatively junior scientist, envying Martin’s ability to come to work in the summer dressed in an open-necked shirt and shorts; excellence brought privilege but there were no intermediary baronies between staff scientists and Sir Charles.

In such an environment Martin and James flourished and, for their first practical gas chromatograph, soon settled on a four-foot-long 1\(\frac{1}{4}\)-inch diameter glass column, held vertically and filled with 200-mesh granules of Celite, a diatomaceous earth. The grains of Celite were coated with a thin layer of non-volatile hydrocarbon, which was the stationary phase. These early experiments established GC as a potential analytical method and reduced to practice the essentials of a working gas chromatograph, which are the sample introduction port, the column and the detector. Martin realized that few would wish to do laborious hand titrations of the column effluent and that there was an urgent need for an automatic detection method. He could have used a thermal conductivity detector, a device that measures vapour concentration in terms of the rate of heat loss by conduction and by convection from a heated wire held in a gas stream. Commercial detectors working on this principle were available but Martin thought that they were too insensitive, unpredictable in response and prone to drift.

He therefore proceeded to invent the gas density balance (GDB) for use as a good detector for the gas chromatograph. Nothing else Martin made so well illustrates his outstanding ability as a craftsman and his deep understanding of kinetic theory as does this most elegant detector. In essence it consists of a rectangular block of copper in which there is a three-dimensional network of interconnecting small pipes. Imagine a three-dimensional map of one of London’s major tube stations where several lines meet with its escalators, passageways and train tunnels and you will then have some idea of the intricacy of the GDB. In operation it was the gas flow equivalent of a Wheatstone Bridge, a device in which the imbalance between two flows of electrons is observed; in the GDB the imbalance between two flows of gas was used as the measure of the density difference between them. The two gas flows were one from the analytical column and one from an identical reference column. The flows were adjusted so that inside the detector there was no flow along a channel connecting them. When vapour was present in the carrier gas the bridge was unbalanced and gas flowed along this channel. The rate of flow was linearly related to the density difference between the two gas streams and the rate of flow was detected by a simple anemometer that consisted of a tiny pair of thermoelectric junctions placed in the gas stream just above a heated wire. When pure carrier gas flowed along both arms of the detector there was no flow in the measurement channel, the plume of
hot gas from the wire then heated both junctions equally, and there was no signal. A small
vapour concentration in one channel caused a flow that displaced the plume, heated the junc-
tions unevenly, and so provided the output signal of the detector. Martin’s GDB is the queen
of gas detection devices. Its response is reliable, linear and simply proportional to gas density.
It is in principle that holy grail of analysts, an absolute device.

By 1951 Martin and James had reduced the gas chromatograph to that practical stage at
which it was immediately useful on a daily basis to their colleagues at the NIMR. These first
gas chromatographs consisted of a vertically supported jacket containing boiling ethylene gly-
col to hold the column and detector at 180°C. They gathered their samples for analysis in
small glass capillary pipettes holding one to several microlitres of liquid and then pipetted the
sample onto a pad of glass wool at the top of the four-foot glass column. They added their sam-
ple for analysis by first turning off the gas supply and then allowing about 30 seconds for the
pressure at the column head to drop to zero. They then detached the tubing from the column
and placed the sample on a glass wool plug at the top of the column.

The too sudden removal of the gas supply would cause voids in the column and a loss of
performance; I have even seen an impatient chromatographer cause the column to erupt spew-
ing powder like a miniature volcano.

The gas chromatograph was used mostly to analyse fatty acids and lipids; these were first
hydrolysed and their component fatty acids were then converted to methyl esters, which chro-
matographed well. Unfortunately, even at 180°C the methyl esters of the longer-chain fatty
acids stearic, oleic and erucic took hours for their separation on the columns then used. The
recorder pen followed a monotonous baseline during the long intervals between peaks. Impatient chemists would sneak into Martin and James’s laboratory and pipette samples of
their fast-moving mixtures onto the column. We always referred to these unexpected peaks as
Gilberts, in recognition of the first name of the chemist most likely to do it.

News of the successes with GC at Mill Hill soon penetrated the medical and industrial ana-
lytical fraternity. Previously the complete analysis of the fatty-acid composition of a vegetable
oil would have taken hundreds of grams of oil and months of work at the bench; now it took
only an hour or so and needed only a few milligrams. The same was true of the analysis of the
products of the petrochemical industry: what had previously been a long or impossible task
now took minutes. The complete and accurate analysis of the composition of petrol became
achievable. So liberating was the new technology that scientists from industry and academia,
eager and expecting to learn enough to apply this powerful technique in their own research,
visited the NIMR. GC had an appeal that was in some ways similar to that of the ‘hands on’
technologies of amateur radio, photography, and motor cycle engineering, which bring
together a global network of enthusiasts with associations that emerge and hold repeated meet-
ings for as long as enthusiasm is sustained. Chromatography, and especially GC, is a technol-
ogy in which most practical chemists can make their own instruments or at least essential parts
such as the separating columns or the detectors. Hardly anyone would attempt the difficult and
precise task of grinding the lens for a microscope or personally preparing an integrated circuit
on a silicon chip. But a two-dimensional paper chromatograph could be set up in a few hours.
To build a complete gas chromatograph needed little more than handyman skills. This com-
fortable familiarity made Archer’s gift to chemists so popular and was why it developed so
quickly.

As with any new invention, the early users continued the research and development of the
technique. Their lesser but significant inventions added value and turned what was a labora-
tory method into a practical instrument. Thus the simple but tricky method of sample introduction used by Martin and James soon became an entry port with a silicone rubber septum into which samples were injected with micro litre syringes. New column packings with different supports and stationary phases soon appeared; these were sometimes presented with the panache of television chef.

Then there were the new detectors. We all regarded the GDB with awe and said that it was close to an ideal detector, but hardly anyone had the craftsmanship to make it and it was just a little too insensitive to exploit the full resolving power of the gas chromatograph. The full potential of Martin and James’s gas chromatograph was not realized until gaseous ionization devices became available. The first of these that was sensitive, stable and reproducible enough for GC was the ‘argon’ detector. This device used the Penning effect, whereby the long-lived metastable triplet-state rare gas atoms transferred their internal energy on collision with vapour molecules. The energy level of argon metastable atoms is 11.6 eV, which is sufficient to ionize almost all organic vapours but not sufficient to ionize any of the common atmospheric gases. Instrument companies in the UK and the USA were soon making gas chromatographs with the argon detector and they were commercially successful. They saw wide use, especially in biochemical research involving lipids. The argon detector suffered two disadvantages: first, it required a radioactive source to free electrons from the carrier gas, and second, water vapour, although not detected, rendered it insensitive. In 1960 McWilliam and Dewar introduced the flame ionization detector that had all of the advantages of the argon detector but none of its drawbacks and it soon became the main detector used in practical GC.

The advent of the ionization detectors made possible the exploitation of the superior resolving power of capillary columns. Marcel Golay introduced the idea of using open tubular columns coated internally with a thin layer of stationary phase. The modern gas chromatograph uses columns of this kind and either a flame ionization detector or a much improved thermal conductivity detector. Where great sensitivity is needed, as in environmental measurements, the detectors used are the mass spectrometer or the electron capture detector; these enable the measurement of a few femtograms, less than a million molecules of pesticides and PCBs.

Martin’s partition chromatography methods now grace well-endowed laboratories as components in an instrument ensemble comprising a gas or liquid chromatograph, a mass spectrometer or infrared spectrometer and a computer. Young graduates introduce their samples and soon the computer screen reveals not merely the quantities present but also their chemical identities. How many of them have any notion of the basic principles of their instruments or could repair them should anything go wrong? Martin’s invention has evolved to the point at which we expect it to work faultlessly and with the same familiarity with which a car, a television or a personal computer function.

Partition chromatography has been, and still is, enormously valuable to industries throughout the world. It is sad, even perhaps shameful, that the UK, the NIMR and Martin, Synge and James failed to benefit directly from the vast wealth their invention had generated.

After and during World War II, many scientists and especially those at Mill Hill were marinated in a simple idealism. Romantically, we imagined that our sole duty was the good of mankind and that it would be morally wrong to profit from our research. Few in the UK had come to terms with the fact that we were no longer the seat of a vast empire rich enough to be so generous but were now just another small nation in a rapaciously competitive world. Despite the experience of the scandalous loss of revenue from the invention of penicillin in
wartime and our failure to protest at its patenting by our American allies, we still had no idea about how to manage or market our inventions. In many ways we were as a nation like the clients of those early twentieth-century charities that provided sustenance for distressed gentlefolk.

**AFTER MILL HILL**

Martin left the Mill Hill Institute in 1957 to set up a business from his home making fraction collectors for chromatography. In 1960 he moved to Abbotsbury, a large Edwardian family house in spacious grounds, which he bought with his Nobel Prize. It was in Elstree, Hertfordshire, and not far from Mill Hill. Here he started a joint business venture with the instrument company Griffin and George and they made, among other things, GDBs. His daughter Vanessa Pawsey tells me that he enjoyed the more rural surroundings and he had as his colleague John Bayes, a talented instrument scientist; but he missed the constellation of scientific colleagues of many different disciplines that made up the Mill Hill Institute. He frequently made day visits to the NIMR to renew acquaintance with his friends there, and up until I left there in 1961, Archer would come to my laboratory to discuss our experiences with detectors and other problems of separation science. These were wide ranging and included thoughts on critical-phase chromatography and on capillary electrophoresis. It was during one of these meetings in my laboratory at Mill Hill in 1960 that Martin mentioned his interest in what now we call nanotechnology. His approach was sequential: first make the smallest possible machine tools using current technology and then use these to make the next stage smaller tools; use these for the next smaller stage and so on through the micro levels; and then use these tools to fabricate nanoscale mechanisms. This was a very different approach from the present-day production of nanoscale electronic devices by lithography, the photographic reduction of a human scale design directly to the nanoscale. Martin was convinced that an ultramicroscopic mechanical computer of the Babbage kind would be as fast and as efficient as an electronic one. We argued in the friendliest of fashions about the relative merits of mechanical and electronic instruments. Remember these were still the days of vacuum tube electronics, and when laboratory scientists had little understanding of the needs and benefits of control theory and feedback. Electronic devices of the 1950s were still temperamental, their responses prone to drift unpredictably or simply to break down. I was in love with the future as represented by electronics whatever its faults; Martin preferred the traditional mechanical approach. This was no stand-off from rigidly held positions, because we both respected each other’s preferences. I thought of the GDB as one of the great inventions of the century and Archer was kind about the sensitivity of the electron capture detector, although scathing about the anomalous behaviour and unpredictability of the early versions that I had just then made.

These encounters resumed when I returned to England in 1964 but now by less frequent visits to my home laboratory in Wiltshire or to his at Abbotsbury. They confirmed for me that Martin had lost none of his capacity to think creatively; what sadly he had lost was the wonderful companionship of Mill Hill. There we both had been able to pursue our ideas, no matter how strange, without distraction by economic or bureaucratic pressures and in the supportive company of like-minded colleagues. Working as a scientist from home is possible and resembles the life of an artist or a novelist; I have found it rewarding and productive but it was not the life for Archer Martin. When he visited me at my laboratory in Wiltshire in 1972 he said, ‘The solitary life seems to suit you better than it does me.’
Martin’s wandering across the industrial landscape had a tragic quality. One after another commercial organization saw him as would a football manager an acknowledged star up for purchase. What they did not realize was that Archer flourished only in an elite academic environment and however excellent were these firms this was something that they could not provide. In this part of his life he associated successively with Griffin & George, Perkin Elmer and Philips, but none of them seemed able to provide the atmosphere in which he flourished. From 1964 until 1976 Martin was an Extraordinary Professor at the Technische Hogeschool in Eindhoven; this was in the nature of a visiting professorship.

From 1969 to 1975, David Long, then the Director of the Wellcome Foundation Institute at Beckenham in Kent, found a visiting position for Archer where he had a similar degree of freedom to that at the NIMR. In 1973 he found Archer a place at the University of Sussex but funding problems allowed its existence for no more than one year. David Long tells me that later when Archer was at Sussex University, he was in some way a personification of the absent-minded professor who would easily get lost when the intricacies of a new idea filled the spaces of his mind. During his time at Beckenham, Archer became interested in an anti-inflammatory substance present in whey and in egg white. Attempts to isolate and characterize this substance in his later years had symmetry with his early researches on vitamin E.

A more permanent offer came from the University of Houston which, in 1974, awarded him the prestigious Robert A. Welch Professorship in Chemistry. A. Zlatkis, Professor of Chemistry at Houston, was instrumental in making Martin’s move to Texas possible and, as I know from personal experience, he would have relieved him of the burden of administrative and teaching duties. Martin, brought up in the Cambridge and Mill Hill scientific tradition, would have found Houston, at that time, a more demanding but less forgiving research environment; this is no a criticism of the university, merely a recognition that it takes years to adapt to a new life. Archer’s family tells me that he could often be embarrassingly direct in speech; this tendency led, while at the University of Houston, to a misunderstanding of American mores. Archer spoke publicly in terms that were considered by his hosts to be so politically incorrect that in 1979 he was obliged to return to the UK.

All of those who worked with Archer acknowledged the period with him as a great experience of their lives and from which they gained more than they had given. In a personal letter to me, Tony James wrote, ‘Archer was the kindest person I have ever known.’ Whenever chromatographers gather at meetings and talk about this kind and modest man we marvel not just at his scientific creativity but at his personal contradictions. We remember his aphorisms almost as if they were physical laws. Here are just two more of them: ‘Never do the first experiments too carefully’ and ‘Never answer the first letter, if it is important they will write again’. These surely came from exasperation and not from selfishness. The last 20 years of Martin’s life, after a brief spell at the École Polytechnique, in Lausanne, were a gentle decline into the misty world of Alzheimer’s disease. He died in Herefordshire in 2002.

**Career**

1932–36 Dunn Nutritional Laboratory, Cambridge
1948–50 Lister Institute for Preventive Medicine of the MRC
1950–56 NIMR, Mill Hill.
Biographical Memoirs

1956–80 During this period he was an independent consultant to various industrial firms including Griffin & George, Philips and Perkin Elmer.

1969–75 Wellcome Foundation Research Laboratories at Beckenham in Kent

1964–76 Extraordinary Professor at Technische Hogeschool Eindhoven.

1973–74 Professorial Fellow at University of Sussex.

1974 Robert A. Welch Professor of Chemistry at the University of Houston.


HONOURS AND PRIZES

The Tswett Medal
The Kolthoff Medal
The Franklin Institute Medal

1950 Fellowship of The Royal Society

1951 The Berzelius Medal of the Swedish Medical Society

1952 Nobel Prize for Chemistry

1958 The John Scott Award

1959 The John Price Wetherill Medal

1963 The Leverhulme Medal

1968 Honorary Doctorate, Leeds University

1971 Honorary Doctorate, Glasgow University

1972 The Rising Sun Medal

1980 CBE

ACKNOWLEDGEMENT

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SIGNIFICANT PAPERS BY A. J. P. MARTIN

A full bibliography appears on the accompanying microfiche; a photocopy is available from The Royal Society’s Library at cost.


