Five years ago in this University Sir James Chadwick gave the second Rutherford Memorial Lecture. I have read it many times and find it an admirable account of Rutherford’s scientific career and a wise and understanding description of his outstanding genius as an experimental physicist. Chadwick gave special emphasis to Rutherford’s remarkable achievements during his tenure of the MacDonald Chair of Physics here at McGill and to the great debt that the world of science owes to this University in giving such excellent facilities to such a young man. Together with the four other Memorial Lectures given under the auspices of the Royal Society, a very complete account has been given of Rutherford’s life and achievements. I will not attempt to recapitulate the story, but will content myself with a commentary on certain aspects of Rutherford’s attitude to the art of the experimenter which seem to be of special interest today.

Here in McGill the exponential decay with time of radioactive substances was first observed and interpreted as being due to spontaneous disintegration of an atom. A quarter of a century elapsed before this brilliant phenomenological interpretation by Rutherford and Soddy was to receive a theoretical explanation in terms of wave mechanics. Today, with the surprising developments of the last decade and the discovery of whole families of unstable elementary particles, we now see that the spontaneous decay of one subatomic bit of matter into lighter pieces is one of the most significant facts of the subatomic world. In point of fact, these families of elementary particles are superficially not unlike the families of radioactive elements which Rutherford and his co-workers did so much to disentangle. An outstanding difference is, of course, that no one has found a theoretical explanation of the spontaneous decay of elementary particles and most theorists believe that none is to be sought, but that such behaviour is just a fundamental property of microscopic matter.

My own personal contact with Sir Ernest Rutherford, as he was then, began in 1921 when I graduated in physics at Cambridge. Rutherford had then held the Cavendish Chair for two years and was still in the process of building up the laboratory and collecting some of his old colleagues of the Manchester days after the dispersal resulting from the first World War. In the first year after the War a young Japanese physicist, Shimizu, had come to work in Cambridge, and Rutherford had started him on the problem of using C. T. R. Wilson’s beautiful cloud-chamber method to study the process of disintegration of nitrogen nuclei, which he had discovered in Manchester during the last years of the War, by means of the scintillation technique. Rutherford himself, together with Chadwick, was actively pursuing these experiments and succeeded in showing that most of the
light elements could be disintegrated by fast $\alpha$-particles. Rutherford realized that the scintillation method, which was well suited to the detection of the fast protons emitted from the struck nuclei, was not capable of revealing the details of the collision process.

By 1911 Wilson, after fifteen years of careful and laborious experimentation, had perfected his cloud-chamber method of revealing the tracks of individual atomic particles. Some of his earliest photographs remain today the most beautiful, technically, ever taken. Rutherford saw that the cloud-chamber technique was excellently suited to the elucidation of the finer details of atomic disintegration processes. The only difficulty was the number of photographs needed.

Now, the scintillation experiments had shown clearly that the fraction of $\alpha$-particles which would disintegrate a nitrogen nucleus in a cloud chamber was exceedingly small. One could not reckon on more than ten nitrogen disintegrations for every million $\alpha$-particle tracks photographed. Rutherford boldly set Shimizu the task of making a cloud chamber with which to take a very large number of photographs. Shimizu tackled this problem by replacing the individual sharp expansion of the now conventional cloud chamber with a piston worked off a crank and moving continuously in a simple harmonic manner, so giving an expansion several times a second. His chamber was very small, only 6 cm in diameter and 1 cm deep. He also designed an elegant little camera by means of which two images of the chamber were projected on to a single cinematograph film. With this apparatus he photographed some 3000 $\alpha$-ray tracks and observed a number of forked tracks due to the collisions of $\alpha$-particles with nitrogen nuclei. Unfortunately, Shimizu had to return rather suddenly to Japan so that the work came to an end. It was then that Rutherford asked me if I would continue the work, which I, of course, was most excited to do.

Thus it came about that my first research problem was to carry on the work begun by Shimizu. I made use of many parts of his original apparatus and the camera design, but altered the expansion cycle from a continuous reciprocating motion to a series of fast expansions every fifteen seconds. The apparatus was made completely automatic and, when things went well, it was possible to take 1000 photographs a day giving tracks of some 20000 $\alpha$-particles. I first studied in detail the frequent forked tracks formed when an $\alpha$-particle collides with the nucleus of an atom of the gas, and showed by accurate measurements of the angles of the fork that the collisions were elastic within the experimental error of the measurements. However, among some 400000 tracks photographed during 1924 six forks were found which clearly were not elastic collisions. After careful measurement these were shown to represent the disintegration of a nitrogen nucleus by the impact of a fast $\alpha$-particle, with the ejection of a proton and the capture of the $\alpha$-particle. The resulting nucleus formed in the process clearly was that of a then unknown isotope of oxygen of mass 17. Thus the problem set four years before by Rutherford of what happens to the $\alpha$-particle on disruption of a nitrogen nucleus was solved.

Much of my subsequent research work was profoundly influenced by this first problem set me by Rutherford. I realized that I could not by myself, as a newly graduated physics student, have possibly selected my own problem with a fraction
of the success that attended Rutherford's selection of it for me. So I learnt early the vital importance of the role of the director of research in selecting promising problems for his research students. Then I learnt the importance of attaining a high level of technique in the taking of cloud-chamber photographs. In this, my aim was always not to fall far short of the standard set by C. T. R. Wilson in his earliest work: a far from easy task when photographs had to be taken by the tens of thousands or when, later on, cloud chambers grew greatly in size and complexity. Unless a high level of technique is maintained, then, when the rare exciting photograph comes along, the photograph may not be good enough to make out exactly what has happened. Related to this is the importance of studying carefully and quantitatively all the normal subatomic happenings occurring in the cloud chamber, so that when something strange does occur, it will immediately be recognized as such. For seventeen years after Rutherford set me to work with cloud chambers I continued, with short intervals, to work personally with them. After that my personal work declined but work with the cloud chamber still remained a major activity of my department.

Till the late thirties the cloud chamber was the only visual method of studying atomic particles. Then came the photographic emulsion, which has yielded in recent years such a rich harvest of important new discovery. Finally, in the last few years the advent of the bubble chamber, invented by Glaser, has provided a new tool of immense power. The essential characteristic of these three distinct, but analogous methods (the cloud chamber, the bubble chamber and the photographic emulsion) is, of course, that the fine details of individual subatomic processes are directly observed. Thus a single technically good picture may serve to test the validity of the conservation laws of momentum, energy, mass and charge, or to reveal a new phenomenon, or to discover a new particle. However, to obtain any information about the forces characterizing the interactions between any two particles, statistical information about the frequency of occurrence of different types of events is required. We see, therefore, that the development of these methods has proceeded along two lines: an increase in the precision of the measurements of individual events and the increase in the number of events studied, with, of course, the hope that both objectives can be achieved at the same time.

Looking back over the forty years of Rutherford's scientific career with its astonishing record of discovery, it is of great interest to attempt to elucidate some of the secrets of his success. Many of his contemporaries have touched on this and all seem agreed on many of the important factors. Rutherford's extreme power of concentration on a particular problem until he could see it from all sides and from all angles; his vivid pictorial imagination in space and time of the events of the world of subatomic particles, which was his favourite field of investigation; his gift for designing simple apparatus perfectly suited to the job in hand; his flair for spotting and following up rewarding lines of research; his eye for the unexpected; his sparing use of mathematics but his great success with it when he did; and lastly his boundless enthusiasm for finding out more about the physical world. The well-known story of Rutherford's discovery, here at McGill, of thorium emanation, as a result of following up the chance observation by Owen that the ionization due to
a certain thorium preparation was markedly affected by draughts, has an obvious moral for all experimenters. May every young scientist remember this story and not fail to keep his eyes open and his wits alive for the possibility that an irritating failure of an apparatus to give consistent results may once or twice in a lifetime conceal an important discovery!

The case of the large-angle scattering of \(\alpha\)-particles, on the other hand, was not so much a chance discovery as an unexpected one: for Rutherford specifically looked for the effect but was exceedingly surprised when he found it. In a sense Rutherford was looking for the unexpected. For he understood perfectly that the huge electric fields required to explain large-angle scattering must imply the revolutionary idea of a very highly concentrated electric charge within the atom. Within a few months he had worked out mathematically the probability of scattering through a given angle on the assumption that all the positive charge in the atom was concentrated in a very small central nucleus so that the mutual force was that of the inverse square; this prediction was then tested experimentally in the laboratory and found correct. Thus was the nuclear theory of the atom born: this was the greatest of all Rutherford's great discoveries.

It seems to me that it is of great interest to trace this interplay between experiment and theory not only throughout Rutherford's career but to subsequent developments in similar fields. Pre-eminent among these, and one which surely would have been very much to Rutherford's liking, is the field of the unstable elementary particles of nature. In the last decades this has indeed proved to be one of those fields which he liked so well—one of his 'Tom Tiddler's grounds where anything might turn up'. It is also, I think, of practical value to study this interplay, since those who are concerned with the practical tasks of educating young research students, and with the tactics and strategy of research, must often have to ponder on these matters. Of special importance is the relation on the one hand, between planned programmes of research and chance discovery and on the other between experiments designed to test some specific theoretical prediction and those made to explore a likely field with an eye to anything which may turn up.

There are, of course, many tales of Rutherford's jocular ragging of theoretical physicists: 'They play games with their symbols, but we in the Cavendish turn out the real facts of nature.' He himself made continuous and most effective use of simple physical concepts such as the conservation of energy and momentum and of orbital theory in elementary dynamics, and of the concepts of probability theory. He would hardly, I think, have held that these concepts belonged to theory at all. Rutherford meant by theory the more mathematical developments such as quantum mechanics. In spite of his personal friendship and admiration for the work of Niels Bohr, he clearly did not find it easy to assimilate quantum theory to his own way of thought.

Though Rutherford's joke was only to a slight degree serious, it serves us well by encouraging us to study carefully what have actually been the roles of theoretical prediction and chance experimental discovery in the history of the physics of the elementary particles of nature during the last forty years, that is, since Rutherford left Manchester for Cambridge. Let us begin with the picture of the
structure of matter as it was when Rutherford took up the Cavendish Chair in 1919. Then only two elementary particles were known, the proton and the electron. The best model of the outer part of the atom, and so of its spectroscopic and chemical properties, was still the Bohr orbital model. A few years had still to elapse before the work of de Broglie and Schrödinger was to show that atomic electrons were better represented by a system of stationary waves around the nucleus. The nucleus itself was then thought of as being composed of protons and electrons, though the difficulty of fitting a large number of electrons into a nucleus was well recognized.

Since 1919 a veritable revolution has taken place in our knowledge of the elementary particles of nature. This has been due partly to a revolution in theory and partly to a stream of more or less unexpected experimental discoveries. The high points of the theoretical advance were the emergence of wave and quantum mechanics from the study of the properties of the outside of the atom, the relativistic theory of the electron by Dirac with its prediction of anti-matter, Pauli's prediction of the neutrino and Yukawa's theory of nuclear forces and prediction of the meson. On the experimental side we note the discovery of the neutron, the positive electron, the $\mu$- and $\pi$-mesons and the whole family of strange particles, that is, heavy mesons and hyperons. The total number of particles and anti-particles which can be reasonably held to be in some sense elementary is now thirty.

When Rutherford in 1920 made his famous speculations about the possible existence of the neutron, he naturally envisaged it as an intimate combination of an electron and a proton, in a way as a kind of lowest state of Bohr's model of the hydrogen atom. Now, of course, it is usually rightly treated as an elementary particle in its own right. However, it is interesting to note that if Rutherford had lived to learn of the experimental proof that the neutron spontaneously decays into a proton and an electron, he would certainly have held that his original picture of the nucleus was not so far from the truth. In fact, however, it does not seem generally useful today to suppose that when one particle transforms itself into others, the latter are in any real sense existing in the parent particle beforehand. During the nineteen-twenties Rutherford made various experiments to attempt to detect the predicted neutron. None of them worked and now we see that none of them were on the right lines. When the discovery of the neutron was made by Chadwick in 1932, it arose through a complicated succession of unexpected experimental discoveries made in different countries. The beginning was the discovery by Bothe, in Germany, that when $\alpha$-particles from polonium fall on beryllium, hard $\gamma$-rays estimated to have 5 MV energy are produced. Following this up, Joliot, in France, studied these rays with a cloud chamber using a paraffin target, and was greatly surprised to find, along with the fast electrons, a number of fast protons. Joliot tried to attribute these to the Compton recoil of exceedingly energetic $\gamma$-rays—some 50 MeV would have been required. Then Chadwick, reading of this work of Joliot, and with the background of Rutherford's speculation about the possible properties of the neutron, saw at once that here it was. The proton tracks of Joliot were not recoil tracks from photons but the recoil tracks from the long-sought neutrons. After a few weeks' brilliant experimentation and reasoning
Chadwick published his classic paper in which the discovery of the neutron was announced and its properties described. In his Rutherford Memorial Lecture here in 1953 Chadwick modestly said 'that the discovery of the neutron came naturally in the general line of advance marked out by Rutherford years before'. The actual pathway to the goal was, as we have seen, a very devious one and included two chance experimental discoveries in two countries.

In 1928 Dirac produced his famous theory of the electron. By finding a quantum-mechanical equation for the electron consistent with the restricted principle of relativity, he not only explained in a natural way the recently discovered spin and magnetic moment of the electron, but showed that the equation implied the existence of particles identical with negative electrons but with a positive charge. He worked out some of their properties, such as how they could be produced by energetic $\gamma$-rays and how they could disappear by uniting with an electron to give photons. At the time these conclusions seemed so unlikely that Dirac himself tried at one period to avoid them by twisting his theory to make his positive particles into protons. This attempt failed: the theory could not be altered so as not to predict the positive electron. However, the general climate of physical opinion did not seem to take the prediction very seriously. When Carl Anderson in 1932 discovered the experimental particle amongst the cosmic rays, it came to him and to nearly everyone else as a totally unexpected and chance discovery. Anderson does not mention Dirac's theory and very likely had never heard of the prediction. When a few months later Occhialini and I, equally unexpectedly, found a large number of tracks of positive electrons in cosmic-ray showers, we knew of Dirac's work, as he was in Cambridge and in close touch with our experiments. Thus we were able to discuss our experimental results in the light of his theory and to argue that the positrons must, in fact, have been created by the process of pair production. This was quickly verified with $\gamma$-rays by Chadwick, Occhialini and myself. Moreover, it was possible to identify the radiation observed by Gray & Tarrant during the absorption of hard $\gamma$-rays as the annihilation photons of Dirac's theory.

The beautiful generality of Dirac's theory led to the belief that it should apply also to protons. This started the long search for the negative or anti-proton, which has only found success in the last few years.

In 1935 Yukawa attempted to explain the strong forces between nucleons by supposing, following Heisenberg, that these forces were of the exchange type, as were familiar in homopolar binding of atoms in many molecules. He pointed out that this would imply the existence of a new particle of mass about 300 electron masses and, moreover, that this particle should be unstable and decay into an electron with a lifetime of about $10^{-8}$ s. In 1938 the chance discovery by Anderson and by Street of the tracks of a particle of about this mass amongst the cosmic rays seemed to provide confirmation of Yukawa's theory. It turned out, however, that this was not the Yukawa particle which had been discovered, but a quite unexpected and still strange particle now called the $\mu$-meson or muon. This particle has only a very small interaction with nucleons, in contradiction to Yukawa's particle, whose very essence was that it was to be highly interactive. Verification of Yukawa's
prediction had to await the chance discovery of the $\pi$-meson or pion in 1947 by Powell & Occhialini and colleagues, in Bristol, by the photographic-plate technique during a survey of cosmic-ray tracks. However, Yukawa’s prediction markedly aided the experimenter in discovering the muon, since it led them to look for particles of intermediate mass. Moreover, it introduced for the first time the concept of an elementary particle which was itself unstable, and so greatly helped the experimenters to interpret certain cosmic-ray anomalies by means of this property. The theoretical difficulty of explaining the prevalence of muons, which indicated a strong interaction at production, with their very small interaction with matter, led also in 1947 to the suggestion by Marshak & Bethe that perhaps another heavier particle was first produced, which subsequently decayed into a muon.

When we consider the first three of the strange particles proper—the charged and neutral $V$-particles discovered by Rochester & Butler in Manchester and the $\tau$-meson by Powell and his colleagues in Bristol—we find that no hint of their possible existence preceded their accidental discovery in 1947. In the next few years the $\Lambda$-hyperon, the cascade hyperon, the $\Sigma$-hyperon, the $\Theta^-$-particle and the other charged $K$-mesons (to use their old phenomenological names) were all discovered experimentally by the study of the interactions of energetic cosmic-ray particles by means of photographic emulsions or cloud chambers. The experimenters were looking for anything that might turn up. Before long, so much became known about so many new particles that it became possible to find order in the complex phenomena, and so be in a position to formulate phenomenological theories from which to make predictions about the detailed behaviour of some of the particles or, in some cases, to predict the existence of new ones. This was the case with the long-lived $\Theta^-$-particle and with the neutral $\Sigma$- and $\Xi$-hyperons.

The first important theoretical argument about the behaviour of the strange particles was put forward by Pais in 1952. Experimentally, the strange particles are copiously produced in energetic collisions, their number being a few per cent of the number of pions produced. This indicates that they must interact with nuclei rather strongly, comparably in fact with the strong interaction between pions and nucleons. On the other hand, all the strange particles have a lifetime before decay of between $10^{-10}$ and $10^{-8}$ s, which is very long compared with the characteristic time (of the order of $10^{-23}$ s) expected for decay processes associated directly with strong interactions. The suggestion was made by Pais that perhaps all the strange particles were produced in pairs. When two strange particles together interact with a nucleon, they do so strongly and thus are copiously produced. But when one alone is concerned, the interaction is weak, so giving a long life. For instance, in the decay of a $\Lambda$-hyperon into a proton and a pion, only one strange particle is present, so the interaction is weak and the rate of decay is slow.

This prediction was soon verified by experiment for the case of the collision of energetic negative pions from the cosmotron at Brookhaven with protons. Pairs of strange particles, in this case a $\Lambda$-hyperon and a $K^0$-meson, were found to be produced. Such associated production is now believed to be the general rule.

Subsequently, Gell-Mann and (independently) Nishijima formulated a rather simple empirical scheme to describe the observed behaviour at production and
To each particle is attributed a new quantum number \( S \), the so-called strangeness: the algebraic sum of the values of \( S \) is supposed to be conserved in production processes, which are all strong interactions, and to change by \( \pm 1 \) during weak decay processes. This scheme successfully described all the facts known at the time, and successfully predicted a few new facts. Though the strangeness is essentially an empirical quantum number, it is now known that it is related in a quite definite way with the isotopic spin and parity, which have proved such useful concepts in nuclear and pion physics.

Another purely empirical discovery is that of the conservation of baryons—a baryon being a particle of nucleon mass or greater—or more generally the conservation of the number of baryons minus the number of anti-baryons. We see this from the fact that when a hyperon decays it always gives one nucleon. The only occasion when a nucleon disappears is when it annihilates with its own anti-particle. This has been proved to occur with anti-protons and anti-neutrons. It is now believed that all particles have their own anti-particles, both fermions and bosons, though in fact Dirac's original theory only applies to the former. I am indebted to Professor Salam for the stop-press news of the anti-particles. Established particle—anti-particle pairs are:

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(\nu, \bar{\nu}), (e^+, e^-), (\mu^+, \mu^-), (\pi^+, \pi^-), (K^+, K^-), (\theta^0, \bar{\theta}^0), (p, \bar{p}), (n, \bar{n}), (\Lambda, \bar{\Lambda}),
\]

It is expected that \( \Sigma^+, \Sigma^-, \Sigma^0, \Xi^+, \Xi^- \) will each have an anti-particle with which it can annihilate into light particles.

No one can contemplate this list of thirty so-called elementary particles without speculating whether some of them might not be better treated as complex structures. Various attempts have been made to do this, for instance, by assuming that nucleons, \( K \)-mesons and pions are the really elementary particles and that hyperons are built up out of them. But no such theory has passed beyond the stage of being a hope: the five alternative decay modes of a \( K \)-meson highlights the difficulties of any such theory. For the present let us conclude, with Salam, that 'all the particles are elementary but some are more elementary than others'. Oppenheimer has likened the present stage of strange particle theory to the position of the theory of spectra around 1910, when the duplicity of atomic spectra had been recognized, but when we were still far from having discovered the electron spin and still farther from the stage of Dirac's theory of the electron.

One of the most curious and exciting events in the history of modern physics has been the discovery of the non-conservation of parity. My account of it will be as it appears to a not very abstract-minded experimental physicist, or (shall I say) as it might have appeared to Rutherford had he lived to hear the end of the story. The term parity was introduced to designate the spatial properties of atomic and nuclear wave functions. A wave function has even or odd parity according as it does not, or does, change sign when the sign of the co-ordinates is changed. The detailed experimental study of nuclear reactions has shown that the total parity of the wave function of a complex nucleus remains unchanged during all changes of state. This has been verified to a high order of accuracy in several different ways: by detailed studies of nuclear transitions, by polarization experiments with
scattered protons and by the study of the strong interaction processes occurring in the production of pions and strange particles. The conservation of parity is generally stated to imply that ‘all physical results should be independent of whether the observer uses a left- or a right-handed co-ordinate system’. It was deduced from this that if any physical reaction can take place, the corresponding reaction seen in a mirror is also a physically possible one. Thus, if right-polarized electrons exist, left-polarized electrons must also. It was further postulated, by analogy with strong interaction in nucleons, that in all physical phenomena one would always find an equal number of right- and left-handed particles. It was held that Nature’s hardware shop always stocked an equal number of right- and left-handed corkscrews. As an example of the rigidity of this belief, it was held that it was not worth the experimenter’s while to test whether, for instance, a β-ray emitted by a radioactive nucleus was polarized longitudinally with respect to its direction of emission (for this would indicate a preference by nature for a right- or left-handed helicity), or whether there might be an asymmetry in the number of particles emitted from a magnetically oriented nucleus.

Then in 1956 Lee & Yang drew attention to the fact that there is no experimental evidence for the conservation of parity in weak interactions such as β-decay, and they outlined the possibility of several experimental checks. It took Wu and colleagues only forty-eight hours to show experimentally that the β-particles from magnetically oriented 60Co nuclei were emitted asymmetrically with regard to the direction of the magnetic field. Within a few weeks other workers had shown that the β-particles from unoriented radioactive nuclei were longitudinally polarized: this experiment is so simple that it could well have a place in any practical class for the final year of an undergraduate physics course. Polarization effects were also demonstrated for the pion-muon-electron decay processes and more recently for the decay of some strange particles.

It is useful to be reminded, as Grodzins (1959) has done in a recent paper, that an experiment was carried out as long ago as 1928 giving strong evidence for the longitudinal polarization of β-particles emitted from natural radioactive substances. The story is an interesting one, and starts with the introduction of the spinning electron in 1925 by Uhlenbeck & Goudsmit to explain the multiple structure of spectral lines. In terms of the contemporary Bohr orbital model, the electron spin was supposed oriented at right angles to the electron’s orbit and with its sense either parallel or anti-parallel to the orbital angular momentum. The energy of interaction between the magnetic moment of the electron in its two positions and the magnetic moment of the orbit resulted, then, in the doublet separation of a p-state.

With this background of knowledge, Cox, McLwraith & Kurrelmeyer (1928) in New York University started experiments to find out if the electrons could be polarized in much the same kind of experiment as that used by Barkla when he discovered the polarization of X-rays, that is, by double scattering. Their paper was published with the title ‘Apparent evidence of polarization in a beam of beta rays’. The work was continued by Chase, a pupil of Cox, who published three papers on the same subject during 1929 and 1930. The general method was to let the β-rays from a beta-ray source be scattered twice through 90° by thick gold
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absorbers. The intensity after second scattering was measured as the angle between the plane of the first and second scattering was altered. As a result of many experiments, these workers concluded that the double-scattered intensity was 3% greater when the detector was at an angle of 90° to the right compared with 90° to the left. The authors pointed out that the difference between the two positions was just that between right- and left-handed rectangular axes. They concluded that in all probability 'we have here a true polarization due to the double scattering of asymmetrical electrons'.

Although, by present standards, the technique and experimental arrangements were not the best, it seems most likely that the experiments really did demonstrate the longitudinal polarization of natural β-particles and so, in modern jargon, demonstrated the non-conservation of parity. If these experiments had been followed up, for instance, by any of us younger colleagues of Rutherford, many of whom were at that time looking for new and simple experiments to do—for this period in the Cavendish was less than usually active—then the hypothesis of the universal conservation of parity would never have been put forward and the essential asymmetry of nature between right- and left-handed systems would have been established nearly thirty years earlier than it in fact was.

No doubt one of the minor reasons why these experiments were not followed up was the diversion of interest to the transverse polarization of electrons. This resulted from Mott's application in 1929 of wave mechanics to particle scattering by Coulomb fields. He predicted that relativistic electrons, scattered at large angles by nuclei of high atomic number, should be strongly polarized transversely, and that this should be experimentally detectable by the asymmetry of a second scattering. Experimenters soon verified this important prediction by Mott of transverse polarization but appear to have forgotten the experimental discoveries by Cox and his colleagues of a longitudinal polarization. The former, being an electromagnetic effect, does not conflict with parity conservation, the latter does. All the experimenters attempting to verify Mott's theory used artificially accelerated electrons in order to obtain better geometry and greater intensity. Since, of course, such a beam of electrons is unpolarized, the experimenters only found the expected transverse polarization after the first scattering. If the experiments had been carried out with a β-ray source, then both effects would have been found: the forward-backward asymmetry due to the transverse polarization introduced at the first scattering and the left-right asymmetry due to initial longitudinal polarization of the natural β-rays.

In fact, Cox's experiment could not be interpreted fully without Mott's theory. Today we would describe the physical process as follows. The electrons ejected from a radium nucleus have negative helicity, that is, their spin is anti-parallel to their linear momentum. On the first and mainly plural scattering through 90°, the direction of the spin does not change, so that the scattered beam is now transversely polarized and so is asymmetrically scattered in the left-right plane at the second target.

Looking back to those years I cannot remember ever having read or heard of Cox's work—and this in spite of being in close touch with Dymond's experiments.
to verify Mott's prediction of transverse polarization, and being engaged personally with the verification of another of Mott's predictions, that of the 'interference' scattering of identical particles (in this case α-particles scattered by helium nuclei).

There are also some curious features in the history of the study of the radiations emitted by radioactive nuclei, oriented magnetically at very low temperatures. For nearly a decade this had been an active field of research in several laboratories and many detailed studies had been made of the angular distribution of γ-rays with reference to the direction of the orienting magnetic field. A knowledge of these distributions, which are all symmetrical about the plane at right angles to the magnetic field, gives important information about the nature of the nuclear transition in which a particular γ-ray is produced. What is odd is that, though many workers must have thought of doing so, no one apparently went on to measure the angular distribution of the β-particles. If they had done so, the asymmetry predicted by Lee & Yang and observed by Wu and colleagues in 1956 could easily have been discovered five or even ten years earlier.

It is true that if one makes a classical Rutherfordian model of an aligned magnetic nucleus in the form, say, of a tiny magnetic solenoid, one would expect no asymmetry between the energy of the electrons shot out in the direction of, or reverse to, the magnetic field. On the other hand, it would not seem quite impossible to suppose that the structure of a heavy nucleus might not in fact be itself longitudinally symmetrical along its magnetic axis. If one postulates a pear-shaped structure (Wilets 1959), then one would expect an asymmetry in the β-emission. So it seems in retrospect that there were good Rutherfordian reasons for at least having a look at the angular distribution of the β-emission—especially as the experiment was not a difficult one. Very often in the history of science has a wrong theory led experimenters to make the right experiment.

A not dissimilar situation existed with regard to the polarization of natural β-particles. For, on purely electromagnetic grounds, the electrons emitted from a magnetically oriented nucleus should be very slightly longitudinally polarized. This arises from the interaction of the electron spin with both the external magnetic field and that of the nucleus, and does not violate the conservation of parity. On the other hand, the observed large longitudinal polarization of natural β-particles, which does violate parity conservation, results from the effect of the spin and the linear momentum of the electron: as the velocity approaches that of light the polarization becomes complete. So if the experimenters had looked for the small classical effect, even though it is in fact too small to be detected, they would have found the actual big effect. This is just what Cox and colleagues did in 1928.

Moreover, there was a quite adequate reason by analogy for looking for a big effect—actually, the analogy is now known to be false, but as a reason for doing the experiment it was strong. Mott showed that very fast electrons become nearly completely transversely polarized by 90° Coulomb scattering at a heavy nucleus. This results from the interaction of the spin and the linear momentum: the electron moving very fast through the electric field of the nucleus 'sees' a very strong magnetic field and is polarized by it. A fast β-particle while being emitted from a
nucleus also moves in a strong electric and magnetic field: might not a big longitudinal polarization be a possibility? Of course, we know now that the big \textit{transverse} polarization of a fast scattered electron does not violate parity conservation, while the big \textit{longitudinal} polarization of a $\beta$-particle does.

The case of the pion-muon-electron decay provides perhaps the clearest case where simple or even naive physical reasoning might have led to the expectation of physical effects which conflicted with the prediction of the theory of the universal conservation of parity, that is, that no longitudinal polarization of a decay particle was to be expected. For any physical process more essentially asymmetric longitudinally than the decay of a pion into a muon and a neutrino can hardly be imagined. Since the muon has, like an electron, a spin of $\frac{1}{2}$, it is natural to assume that it can be polarized. What could be more natural than to assume that it might be polarized longitudinally after being born along with a neutrino in the asymmetrical process of muon decay? If so, then it would have been reasonable to expect that the polarization might be detected by observing an asymmetry in the direction of the subsequent emission of the electron into which the muon decays.

Several years ago, a few measurements had been made, with photographic emulsions, of the direction of emission of the electrons resulting from muon decay in relation to the direction of the muon track: a small asymmetry was found. It is understandable that this work was not energetically followed up, since, at that early stage of the emulsion technique, there were so many important investigations to be easily made, and which did in fact lead to many discoveries of great importance. Thus the simple but tedious measurements required to confirm the existence of such small asymmetries was left for the time being—until, in fact, Lee & Yang drew attention to their extreme theoretical importance. We know now that the electrons from a muon at rest are completely polarized with negative or left-handed helicity, that is, with their spins anti-parallel to their momenta. Since spin is also conserved during the process, the two neutrinos, which are emitted with their momenta opposite to that of the electron, must also have negative helicity.

An indication of the relative simplicity of the experiments required to verify Lee & Yang's predictions is found in the fact that in the ensuing two years hundreds of experiments have been made in many countries to investigate the three main effects: the polarization of natural $\beta$-rays, the asymmetrical $\beta$-emission from oriented nuclei and the polarization of muons from pion decay. As a result of all this work the field seems temporarily being worked out, except, of course, for the finer details. However, this only applies to $\beta$- and pion decay: there is still a vast amount to be done in the field of the strange particles.

It is of interest to remember that it was neither general physical considerations nor detailed studies of the processes of $\beta$- and muon decay which raised the first doubts about the universal validity of the conservation of parity: the initial stimulus was the experimental result that two of the newly discovered strange particles had different decay schemes into states of opposite parity, but had indistinguishable masses and lifetimes, and so most likely were one and the same particle.
The moral to experimentalists of this curious piece of scientific history is clear. Too many of them must have been deterred from making some simple but important experiments because of the predictions of a theory which they did not fully understand—for if they had, they would have realized that it was not soundly based.

I wonder what Rutherford would have thought of this exciting story with its complex interaction between theory and experiment, and of the failure of the experimenters to make the obvious experiments and so leaving it to the theorists—playing with their symbols—to get the right answers.

References

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