CECIL POWELL

Fragments of Autobiography

The University of Bristol decided to republish this little fragment of autobiography as a memoir of Cecil Powell. Powell's own account of his life covers only the early period of his career: his boyhood, his time at Cambridge and the early years at Bristol. As an indication of his later work, interspersed throughout the text are photographs* which will serve as a reminder of his later achievements, which are set in their context in the introduction by Peter Fowler.

*In this CD-Rom version, the photographs are gathered together at the end of the text.

Introduction

The discovery of the pi-meson in 1946 was a milestone for British physics and the University. At that time, electrons, positrons, protons and neutrons were well known, and cosmic ray research using cloud chambers had revealed the existence of a particle, now known as the muon, of mass intermediate between that of the electron and the proton. A particle of just such a mass had been predicted by Yukawa in 1935 from considerations of the short range nature of nuclear forces. However, further work had revealed that, apart from its mass, the properties of the muon meant that it could not be the particle Yukawa had predicted.

The Bristol experiments on cosmic rays involved exposure of photographic emulsions for several months on mountain tops, where the cosmic rays are more intense. Upon their return they were developed and then scanned under the microscope, and tracks of charged particles were seen and analysed. The work was led by Professor C. F. Powell with his chief collaborator, G. P. S. Occhialini. Many tracks of mesons were seen and then in 1946 several examples of 'double mesons' were found, in which one meson came to rest and then gave rise to a second meson which always had practically the same range, around 600 microns, in the emulsion. This implied that the second meson was receiving a constant amount of energy as a result of two-body decay of the first. The secondary meson was shown to be the particle previously known from the cloud chamber pictures, but its parent was a new and somewhat heavier object, christened the pi-meson.

It was quickly established at Bristol that the pi-mesons were created in nuclear interactions of the cosmic rays. They exhibited two types of behaviour at the end of their range, either decaying to form a muon or producing a 'star' of several particles of different masses. The Bristol group speculated that these different outcomes were due to positively and negatively charged pi-mesons respectively. Negatively charged pi-mesons were attracted into the positive nucleus of an atom causing it to disintegrate, whereas the positive pi-meson, being repelled from the nucleus, decayed at rest producing a muon. We would now write:

$$\pi^{+} \rightarrow \mu^{+} + \nu_{\mu} + 34 \text{ MeV},$$

the second product particle being an unseen neutrino.

In 1948 pi-mesons of positive and negative charge were separately produced in an accelerator at Berkeley by Gardner and Lattes, and were shown to behave in just the way described by the Bristol group. Their properties matched exactly those predicted by Yukawa, and the pi-meson was recognised as the 'carrier' of the nuclear force.

In 1950 Powell was awarded the Nobel Prize in Physics 'for his development of the photographic method of studying nuclear processes and his discoveries regarding mesons made with this method'. In the previous year Yukawa had received the award for his work on the meson theory. At Bristol, the University and the Physics Department celebrated in due style, and the cosmic ray group embarked upon a major expansion of its research effort, involving many visiting scientists and the employment of a large group of 'scanners' to search for and to assist in the analysis of the tens of thousands of individual tracks selected for measurement. The discovery of the pi-meson ushered in an extremely exciting period which, in a few years, included the discovery of the k-meson at Bristol and revolutionised understanding of sub-nuclear physics.

Peter Fowler

CHAPTER ONE

I was born at Tonbridge in Kent, in the South of England, on the 5th December 1903. My father, Frank Powell, came of a family which had lived in the neighbourhood for some generations and my grandfather, Peter Powell, had established a gunsmith's business in the town which my father inherited. My mother, Elizabeth Caroline, née Bisacre, was the daughter of a schoolmaster, a Gloucestershire man of Huguenot descent, who had migrated from Wotton-under Edge to the small town of Southborough, two miles from Tonbridge, where he had set up a private school. I was the elder child of our family, my only sister, Phyllis, being three years the younger.

Both families were in poor circumstances when I was born. Peter Powell's fortunes had been seriously undermined by a strange misfortune. He was out shooting one day with a party when, on discharging his gun, a pellet ricocheted from a tree and hit a one-eyed beater in his remaining good eye, permanently blinding him. A series of lawsuits ensued bearing on the very difficult problem in law of the degree of responsibility which ought to be attributed to Peter Powell; and although he won his case, the costs of the actions, which he did not recover, were ruinous for him. I remember the matter being talked about in the family when I was quite young and I was strangely reminded of it many years later on a flight from London to Australia in 1959.

We had taken off from Singapore to Darwin when, about half an hour out, the pilot informed the passengers that one of the four engines was performing badly and that he proposed to return to Singapore, since the facilities at Djakarta were inadequate. For this purpose, the pilot explained, it was necessary to fly on a little further and return on our tracks before discharging the 2,000 gallons of petrol which had to be dumped before landing; otherwise there was a danger that we might fly through the resulting cloud of vapour. The pilot explained how this was to be done. A trailing hose was to be lowered from the aircraft through which the petrol was to be discharged, an operation in which there was no attendant danger because, even if the petrol caught fire, the speed of the flame was substantially less than that of the aircraft so that it wouldn't catch up with us.

We landed safely in Singapore; but the candour of the pilot, and the release in tension on landing, made everybody unusually communicative. As we walked back over the tarmac to the airport buildings in Singapore, under a brilliant moon at two o'clock in the morning, my neighbour and I exchanged names. He was a high court judge from Sarawak and he remarked that Powell was a name familiar to him in law as there was a famous case involving a shooting party which had established an important precedent. Much to his surprise, I told him it was my grandfather, and that he had recalled a matter which hadn't crossed my mind since I was a child. When we again took off from Singapore, the pilot told us that the trouble had been caused by a large bird which had been sucked into the air-intake of one of the engines, substantially reducing its power.

But in addition to the difficulties arising from this lawsuit, the handicraft production of guns and cartridges was becoming increasingly uneconomic with the growing industrialisation of production, and when I was three years old, my father was bankrupted. Afterwards he made a living as a clerk in his brother Edwin's electrical business in the neighbouring town of Tunbridge Wells. My mother gave support to the family in these difficult times by taking in lodgers, and my father helped eke out slender resources by bringing home rabbits and partridges which he shot on the land of friendly farmers in the neighbouring village of

Haysden where the Powell family had long had an old house with orchards and gardens, and where my father's brother, Horace, still lived.

My father's guns and fishing rods were amongst his great treasures. There was the ritual of cleaning the guns, the pull-through, the tow and oil for cleaning the barrel, the marvellous finish on the walnut butt, the delicate tracery on the burnished metal work around the breech, and the sweet precision of its mechanism. He kept all his gear in a great black box under his bed; boxes of cartridges, fishing reels and silken lines, delicate wooden mallets and tools for gun making, revolvers and 'knuckle dusters'.

This martial equipment was not at all in keeping with his nature. He was essentially a mild and temperate man, rather overshadowed by his brothers. He used to leave the house on week-days at about 8 am to ride, by bicycle, to Tunbridge Wells. In early summer he would be earlier, to leave time, on his way up the hill to Southborough, to visit the woods round Lambs Bank where the song of the nightingales, particularly brilliant in the early mornings, used to delight him.

My father had not much time to spare, but sometimes he took me fishing down the river where we would spend a quiet day in the high summer, quite content to watch a stationary float in a placid stream, undisturbed except by the occasional 'plomp' of a handful of ground-bait. We rarely caught anything, but that surely was not the real objective of the expedition. We once went even further afield, travelling by train to Paddock Wood and then by a branch line to Horsemonden, where he had been at school and which he remembered fishing as a boy in the Furness pond.

Although we were poor, our lives were made pleasant by our excursions to Haysden through a pretty countryside and all the adventures that are to be found in the valley of the river Medway; the spring flowers in the hedgerows, the scented white violets in the orchard, primroses and bluebells in the woods in early spring; wild roses and honeysuckle in the summer hedges, with buttercups and marguerites in the water meadows; blackberry- ing, and blue-white mists and bonfires in autumn evenings. In the early 1900s Tonbridgc was still a small town and the surrounding countryside unspoilt; a marvellous place of adventure for small boys, with its woods and ponds and streams.

My mother was a good deal younger than my father and it was she who kept things in order and steered us through the bad times. There is often great variety in the temperament and abilities of the children of a single marriage but it was particularly marked in the Bisacre family. Unlike her two brothers she had a strong constitution with a marked capacity for hard physical work and a passionate love of life. I remember her at this time in the kitchen on Monday mornings, engaged in the weekly wash with the room full of steam from the great boiler in the corner for which my father used to light the fire before starting for work.

She had been regarded, not without some affection, as the 'booby' of her family, as one of limited intellectual and academic attainments who was insufficiently sensitive to all the delicate and intricate nuances of human relations in the family and in the narrow South-borough circles in which she was brought up. She was too disposed to speak what she felt with a frankness and insight which showed too little respect for the prejudices and taboos which confined their lives, so that they were embarrassed.

She got a good deal of relief from the narrow round of her life when we were young by occasional visits to a London theatre in the company of one of her few friends, whom we called Auntie Ethel but who was not related to us. They would go off by an early train, returning in the late evening with a coloured magazine about the 'stars' in the evening performance. What was important for her, I believe, was the opportunity to laugh frankly and without restraint in the company of people who were similarly enjoying themselves. Many years later in 1943, only a few months before she died, she was employed, as part of the war effort, in a small factory filling chemicals into glass containers and she spoke of her happiness in the work and the kindness shown to her by her companions on the job. She felt for the first time the satisfaction of being part of a larger society devoted to something worth doing, which her life had hitherto denied.

One of the heroes of my youth was my father's brother, my Uncle Horace. He was a man of infinite resource. His dominant features were his eyes, under prominent brows, his rather large nose, and his untidy moustache. He was entirely indifferent to his appearance and wore an old bowler hat, green with age, on the back of his head; a high celluloid collar secured with a brass stud very prominent in the absence of a tie; and clothes made of a fabric called 'iron-cloth', which seemed to wear for ever. The colour used to fade completely long before the garment was worn out; and on one or two occasions I suffered by inheriting a coat which was, for me, grossly ill-fitting and, even worse, indestructible.

He lived in the house at Haysden, an old Elizabethan structure, very dilapidated, which he had substantially rebuilt by lifting the old roof from off the walls by jacks, and after rebuilding some of the walls, lowering it back. 'And all the time I was doing the job', he would say, 'I was sleeping under that roof'. He had a few good pictures and I remember in particular a Dutch painting of an East Indiaman, with its great carved stern and furled canvas sunlit in an eastern harbour, which evoked all the romance of the sea and of adventure.

We used to visit him on Sunday mornings in the spring and summer, with me seated on a cushion tied to the cross-bar of my father's bicycle. For two miles it was just tolerable, and on arrival at Haysden, we were given biscuits of a special kind, which my uncle kept in an enormous tin, with perhaps a glass of sherry for my father. If the house was ramshackle and ill-kept, the garden and orchards were a kind of paradise with all the fruits in season, strawberries, cherries, raspberries, apples; with great red peonies near the well, the marvellous smell of box hedges in the sun, and all the scents and sounds of summer's honeyed breath.

This fertility, according to the family legend, was due to the establishment of the Tonbridge Urban District Council in 1895. Before that date the orchards had been barren, and many of the apple trees had so rarely borne fruit that their varieties were unknown. When the Council was first established, its responsibilities were very limited, its principal task being to ensure that the streets were kept clean at a time when most of the traffic was horse-drawn. After two years, an immense heap of manure had been accumulated in the Castle grounds and its unsightliness and objectionable nature had resulted in widespread public protest, embarrassing to the local authority.

After consulting his sons, Peter Powell approached the Council asking on what terms he could obtain the manure. The response was immediate. He was informed that if he would remove it, he could have it for nothing. So two men were employed, with two horses and carts, to shift the heap to Haysden and they became a familiar sight moving up and down Tonbridge High Street.

The heap seemed to be inexhaustible. As the months passed the orchard grew deeper and deeper in muck which reached up towards the lower branches, and in the bar of the Royal Oak, the ale-house at Haysden, an animated debate developed as to the effect of the whole enterprise. One faction was firm in the opinion that the trees, so long unaccustomed to nourishment, would never survive such an inordinately rich diet. Another that the effect would be wholly beneficial. 'But', said my uncle, 'there came a lovely wet autumn, and that manure went in a treat'. In the spring the first thing that happened was that the grass reappeared and grew so tall that it reached the lower branches. It was a mild and moist May without frost, with a lovely show of blossom and lots of bees, and it soon became clear that there was a tremendous set of fruit. As the summer went on, dozens of hop-poles with forked ends had to be found to support the boughs and prevent them from breaking under the great load of fruit; and in the autumn there was a tremendous crop. The old centre tree turned out to be a Blenheim Orange and we took 40 bushels off it. And, he used to end, 'that's twenty years ago now, and the orchard has felt that manure from that day to this'.

With no one to look after him, Horace's domestic arrangements were exceedingly primitive. He used to store the apples, for example, on straw under the bed, and this gave a powerful, not unpleasant reek to the bedrooms; but it was a practice marvellously effective in attracting the rats. When they were too many and too bold, the moment came for drastic action. In the late evening, my uncle would go to his bedroom with a small rifle, and a lantern, which he hung on a nail driven into a beam just inside the door. Then quietly crossing the room he would stuff a wisp of straw into a hole in the ceiling, near the top of a vertical beam, through which the rats came and went. Returning to the corner by the door, he would roll an apple under the bed. A rat, disturbed, would run out, climb the beam and, finding the way blocked, would turn to retreat. Bang! went the gun, and the rat fell. The noise of a gun, even a small rifle, discharged within a house is indescribable, deeply impressive, but my uncle never missed.

In contrast with the interest and variety which was always to be found at Haysden, and the freedom from niggling conventions, our occasional visits to my mother's parents in Southborough on Sunday afternoons and evenings were a little dull. There was an all-pervading atmosphere of seedy, genteel respectability. We used to leave in the early afternoon to walk the two miles up the hill to Southborough with my sister in the push-chair and I walking by Mother's side. My grandfather had run a small boarding school for many years with about a dozen pupils, most of whose parents were abroad; but it had never been a great success and now that he had retired he had few resources and they were dependent in large measure on their son Frederick who had done well as an engineer, and their daughter Edith who lived with them and kept house and who made a little money by giving private lessons to backward children.

At this time they lived at 'Fern Terrace' at the bottom of Southborough Common near the road to Tonbridge. This house was full of second-hand Victorian furniture, plush-covered armchairs, an ancient tuneless piano and a general air of rather seedy and faded gentility. My grandmother was a prim, small and fragile woman for whom the strain of bearing and bringing up five children and acting as housekeeper and foster mother to the boarders had been too great. She used to stay in bed until noon, sitting up against the pillows crocheting elaborate table-cloths and knick-knacks of her own design, and receiving her visitors as in a miniature salon.

But for young children there was little to do; to my grandparents their liveliness and noise was difficult to bear and the tuneless piano was taboo. I used to walk on the common and in the adjoining pine-woods looking for agreeable companions whom I never found; and in the evenings I was sometimes induced to accompany my grandfather to church of which my only memory is an all-pervading impression of the vanity of human life and the mournfulness of the human condition.

I suppose that the general air of faded and genteel respectability of the Southborough of those days was characteristic of a small town which had become the retreat of moderately well-to-do retired people. It had an elaborate social hierarchy in which my mother's family had an indeterminate and very isolated place; they were 'neither fish, flesh, fowl nor good red herring', as my mother was used to remark.

But there were a few bright spots in this dismal picture, such as the white flannels and coloured blazers at the cricket on the small ground on the common near the 'Hand and Sceptre' Hotel, where the cricketers took their lunch and their beer and where the local team under Captain Harris entertained the visitors.

So at Southborough I preferred to be out of the house rather than within it. On the common, even on Sundays when cricket was taboo, at any rate there were people and movement even if I was lonely; but within there were disagreeable tensions which I became quick to distinguish, inescapable in a family group among whom there were such deep frustrations, such gnawing insecurity, and no escape from an all-pervading boredom and lack of purpose.

Although my parents were not what, in those times, would have been called religious people, they also would occasionally take me to church. My father's family was inclined towards low-church doctrine and his eldest brother Edwin was a member of a highly bigoted sect, his only subject of conversation seeming to be the crucial importance of being saved. At Haysden, wandering round the garden one would sometimes see him closely engaged with Horace who appeared sheepish and ill at ease, furtively looking for an excuse to follow some urgent practical task that would allow a decent escape.

But it was by accident rather than by design that at our local church, St Stephen's, the vicar was what my father called 'a very powerful preacher'. He was a man of grave and impressive bearing with iron grey hair, who used to thunder out vivid descriptions of the torments of hell. As a child of about nine, I used to sit terrified throughout his sermons waiting for the inevitable ferocious declamations. As a result, when on Sunday afternoons, I was sent to rest, rather as a relief for my parents than for my own physical needs, my blood would run cold as the trumpets and the trombones of the Salvation Army suddenly rang out in a nearby street at the beginning of their afternoon meeting. I would stay still and sweating for a half a minute or so until later noises reassured me that what I had heard was not the last trumpet on the Day of Judgement.

For a time our lodgers were birds of passage who stayed for only a week or so, but when I was eight or nine we secured a permanent guest, W. B. Gray, who stayed with us for many years until his death in the early 20s and who became one of the family. He ate with us and had a small sitting room in the front of the house. He was a bachelor who had been a surveyor and had taken part as a young man in the preparation of the first editions of the Ordnance Survey of some of the Southern counties. At the age of about 60 he had retired and led a quiet and comfortable life. There was a mutual affection between us. His Christian names were Walter Beveland and I used to address him as Sir Beveland. There is something engaging in being given titles to which one has no claim but which are not entirely incongruous and I remember many years later my own gratification on being addressed as 'Colonel' by an American from the Southern States. I am sure the unusual title of Sir Beveland, which was very appropriate to his dignified bearing and appearance, gave him a certain satisfaction.

At that time there was a summer festival, the 'Tonbridge Cricket Week', when the county team had two three-day fixtures, with marquees and enclosures for the well-to-do, and large crowds sitting all day watching the game in the summer sun on hard benches arranged around the ground. There were always large numbers of small boys who came to watch their heroes, Hutchings and Woolley, Blythe and Fielder and great and exciting contests against Yorkshire and Lancashire and the other champions of those times.

Sir Beveland as a member of Tonbridge Cricket Club had a number of free tickets, and I used to be wafted through the turnstiles and into the exciting world within by the kind of magic they provided, and sometimes even into one of the stands. I used to wander round the ground, occasionally finding a 'score of the match' card somebody had dropped; and watching the traffic at the stalls where people seemed to have unlimited money for strawberries and cherries, and for exciting menus in the refreshment tents. This gala was always ended by a Venetian fête on the river which was watched from the castle grounds under the great ramparts of the Norman Castle. If the weather was fine, as it always seemed to be in the middle of June, this was a splendid occasion with the boats slowly moving by with their tableaux, illuminated by Chinese lanterns and the light of innumerable candles. If it was not as good as Venice it nevertheless had an air, and the spirit of holiday was abroad among the people.

During this week, we used to take part in an established ritual. Sir Beveland would appear one day in mid-morning with a substantial salmon. 'There, missus', he would say to my mother, 'look what I have caught with a silver hook', and my mother would set about preparing lunch with salmon, green peas and new potatoes and perhaps gooseberry tart or cherry pie.

By this time my self-confidence and complacency were growing. On one occasion, Auntie Hartnup, who was related to my father's mother, came to visit us and there was raspberry jam for tea. I explained to the assembled company, echoing my father's distrust of all modern innovations, that in these degenerate times raspberry jam was no longer what it used to be, and that it was now made largely of apple pulp with some added essence, even the small pips being artificial and made of tiny chips of wood added to complete the illusion. My mother who had recently made the jam with raspberries collected from Haysden was stung into a sharp but amused protest, and I learned the dangers of an uncritical acceptance of unverified sources.

CHAPTER TWO

My parents' life was hard and my mother was very anxious that her children should escape from the drudgery which had been her lot. Her brother Frederick Bisacre, after becoming a teacher, had succeeded in getting to Trinity College, Cambridge, and had become an electrical engineer. My mother determined that I should secure a similar emancipation.

I was sent to a local elementary school at the age of five and very early on had the opportunity of doing things with my hands. With narrow strips of wood, small brads and a hammer, we were encouraged to make little wooden boxes. I remember my first small box and the look and texture of the wood and the comment of my uncle Horace, that it was built upon the wrong principles, the base being enclosed by the sides, instead of the sides being mounted upon the base. This gave me a taste for working in wood; for the immense variety of the different timbers; of oak, yew, beech, teak, black walnut, sycamore, mahogany, rose-wood; and their varied scents and textures, under the chisel, the plane and the scraper.

This woodwork must have been a new feature of the school's curriculum, an addition to the usual formal instruction which was largely involved with reading, writing and arithmetic, but I enjoyed it all and got great satisfaction from what we called 'pot-hooks'. You were provided with a lined page at the top of which were printed examples of the most important strokes which make up a good calligraphy; lovely round o's, i's, n's and p's. I am sure such a practice is now much frowned upon, but I enjoyed it.

At the age of 12, I won a scholarship to Judd School, Tonbridge, after an interview, a sort of viva-voce examination, under the inspection of the grave and benign face of John Evans who was to become my new headmaster. I told them I liked the countryside and I think my knowledge of the flowers and when they came into bloom gained me a place. Soon after this I got a lot of help from my grandfather George Bisacre, who used to visit us from Southborough, bearing gifts in the form of text-books. Southborough was the home of a number of cramming establishments and there were several book-sellers whose shelves were packed with out-of-date textbooks which could be bought for $^1/2$d or 1d each. I recall the names and the binding, but little else, of the treatises on such subjects as The Calculus of Variations, Solid Geometry, Advanced Trigonometry, which at the age of 12 were completely beyond me, but which my grandfather, although also mystified himself, regarded as being within the compass of a young mind.

Among these treasures there was one book, which captured my imagination, on Elementary Chemistry by Perkin and Kipping which is still widely used. It was full of romance. It was peopled by things with such resounding names as 'fuming sulphuric acid', 'spirits of salt', 'sugar of lead', 'yellow phosphorus', and a hundred others, and it described fascinating exploits like making 'lead iodide' by the process of double decomposition. When you mix two perfectly transparent and colourless solutions, you get an immediate precipitate of brilliant yellow crystals of lead iodide, or so the book asserted. I determined to try some of these operations for myself and thus, without knowing it, became a disciple of Francis Bacon and Leonardo and an opponent of the tradition of scholasticism which advocates reliance on authority.

Because my spelling was poor, my mother used daily to give me dictation, reading out the leading article from the Daily Mail, and I managed to persuade her to agree that I should save up for enough apparatus to generate the gas hydrogen by the action of dilute sulphuric acid on granulated zinc. By accumulating the financial proceeds of two birthdays and one Xmas, this I managed to do.

My source of supplies was a chemist's shop near the railway station run by a man called Upton. He was a kind and amiable man, rather short-sighted with pale blue eyes, and I remember vividly the quizzical expression on his face as I peered up and over the counter and asked for such items as two ounces of cyanide of potassium and a bottle each of sulphuric and citric acids. How much I owe him that he never refused me anything. The proceedings must have been highly irregular even in those days; he used gravely to warn me of the nature of the substances he was delivering and how they should be handled; and treating me with gravity and seriousness of an adult, he placed his reputation and his future into my hands.

Eventually the day arrived when I had assembled all the gear; flasks and thistle funnels, rubber bungs and connecting tubing, granulated zinc and sulphuric acid. The apparatus was assembled in an outside shed with a corrugated iron roof which my uncle Horace had built for us to store coal. It had no windows, but there was a shelf six inches wide running across one end, and it could be illuminated by the light of a candle.

With zinc in the flask, I inserted the bung carrying the thistle funnel and poured in the diluted acid. There was an immediate reaction. The mixture in the flask seethed and bubbled like the witches' cauldron in Macbeth; great iridescent bubbles, covered with a most sinister looking black scum, appeared and broke; and I waited anxiously for the generated gas to bubble out from the end of the connecting tube and to rise up into the inverted flask filled with water which was my only available means to collect it. But nothing happened. I concluded there must be a leak.

Now according to Perkin and Kipping, hydrogen burns in air with a lambent blue flame. It seemed to me that, with such a manifestly large volume of gas being generated, it must be escaping somewhere in the form of a jet, and that I should be able to ignite it if I passed the flame from the candle over the places where a leak might occur. So cautiously taking the candle I began the experiment.

There was an explosion which in that confined space, and with that corrugated iron roof, seemed absolutely tremendous. The candle was blown out, and I was left in the dark, dazed and deeply impressed, but otherwise unhurt. Of all the apparatus, glass-ware, acid, granulated zinc, I never discovered the slightest trace except the candle and the rubber bung; and none of it, neither glass splinters nor acid, was buried in my face or clothing. After a few minutes I collected myself sufficiently to open the door and shout out: 'Mum, did you hear that?' and my mother, who had been frozen in her armchair where she had been reading the newspaper, doubtless choosing a suitable passage for the evening dictation, breathed again.

I was much helped in my studies by being allowed to sit with Sir Beveland in the evenings in his sitting room, under the condition that I should keep quiet. He used to sit in his chair before the fire reading a library book, smoking Players cigarettes, and drinking occasionally from his glass. Before the war he used to take whisky, but afterwards, as his resources declined, it was replaced by Rose's lime-juice cordial from Montserrat. There was little spoken communication between us, but I think he found my company not uncongenial, a certain compensation from his loneliness.

I used to watch him inhaling deeply from his cigarette and was once bold enough to ask him 'Do you really like smoking cigarettes?' He must have felt some elements of a rebuke in my question for he replied rather brusquely 'Do you think I would do it if I didn't like it?' He once remarked to my mother that he had only known one other boy who applied himself so closely to his books and that had been one of his school-fellows, generally detested by his fellows, who had eventually become a bishop.

I think this remark encouraged my mother's views of my capabilities and her ambition for me to succeed. She was inordinately proud of my sister and me, and one day, expatiating on our virtues, her own mother exclaimed in irritation: 'Don't be ridiculous, Bessie! All mothers think their children are the best!' 'Yes', said my mother in high indignation, 'but they are not!'

A slightly sinister figure looms up in my memories of this period, a boy named Clinton, who was nick-named 'Ghosty', some years older than my immediate cronies. He used to make expeditions to the extreme limits of our usual hunting grounds, and was most marvellously equipped with great knives, axes and shining fire-arms. For the most part he would remain aloof, only allowing one or two favoured boys to accompany him in making camp, and his appearance and bearing could be so intimidating that we dare not go near. 'Aha! Powell', he would say in a menacing voice, 'have you yet been made acquainted with the water torture, or the ordeal by slow-roasting and piecemeal slicing? Come and visit me some time, and I shall be delighted to give you a demonstration'. And these sentiments were accompanied by sinister leers and a kind of intense inward delight in the prospects he was envisaging. We kept away. The last I heard of him was a brief press cutting on the school notice board reporting his part in an expedition, I think to Brazil, to the Matto Grosso.

At about this time I was involved in another incident which also shows the care which fortune sometimes shows for the young and inexperienced. A friend, Swale, and I used to tour the countryside together by bicycle. One day we were returning from the village of Ightham, wearing straw hats, which were de rigueur for school-boys in those days, and descending in a manner we had developed after long practice. I had no back brakes; and the connection between the lever on the handle-bars and the fork which applied the brake-shoes to the front wheel was missing. When alone, I used to stop by leaning over the handle-bars and pulling on the brake-fork. This was generally effective but uncomfortable and slightly nerve-wracking. With two of us, it was more convenient to put my arm around Swale's shoulders, so that his brakes would serve us both.

But on this occasion, as we descended, briskly but well under control, my straw hat blew off in the breeze. Releasing my companion I asked him to go back for it, with the intention of using my front brake. But by the time I began to pull on the brake fork I was already going fast; and pulling harder, the whole mechanism suddenly came away in my hand. Although I only vaguely realised it at the time, my situation was like this: I was descending a hill with a gradient of one-in-eight at about 30 miles an hour, too fast on that rather rough road to permit using the old dodge of half-dismounting and dragging a flat foot along the road surface as a friction block. So I stayed on, in free flight down the hill.

About 50 yards below me there was a sharp bend in the road where it turned acutely, and at that point the road was edged by a grassy bank, about two feet high, on which was mounted an iron fence made of horizontal strips, with pointed vertical members about four feet high; and behind the fence there was a dense wood. The most sanguine observer could have anticipated nothing but a disastrous outcome. The nightmare possibility crosses my mind that the iron fence might have done for me what a multiple-wire-slicer does to a hard-boiled egg, so that I should have emerged piece-meal on the far side. But the outcome was completely different.

Later my tracks showed that, in a vain attempt to steer round the bend, I had scarcely departed from a straight line. The front wheel hit the mound of earth below the fence and buckled completely on impact. The front fork, anchored in the bank, then became a kind of axis of rotation and I was shot rotating into the air over the top of the fence into the wood. I was conscious of being upside-down and spinning in the air and I landed in a bush a dozen yards within the wood. I found myself, as in an armchair, relaxed and comfortable, a foot off the ground, and without a single scratch or piece of torn clothing. I spent a little time feeling myself and enjoying my relief and situation, until Swale, coming down the hill with the hat, was mystified to find a wrecked bicycle and no trace of the rider or of where he could have gone. We recovered my bicycle by wheeling it the seven miles back to Tonbridge on the back wheel using the shattered front wheel as a kind of handle-bar for steering; but it was too damaged ever to be repaired.

When I was older, in my 'teens', I was sometimes allowed to take part in my uncle's enterprises. Just after the first world war, for example, he bought a navy long boat in Chatham, as part of surplus naval material, which was sold for a song. His intention was to install a motor and convert it into a motor-boat, but the first job was to get it up the river to

Tonbridge. We went down to Chatham and towed it along the bank. For the most part, my uncle steered and I pulled, but from time to time he took my place at the end of the tow rope; but the tedium and the labour were relieved by the passage through the 15 lock-gates, most of which we had to operate ourselves.

With the boat installed in his boat-house, just below the big bridge in Tonbridge, my uncle set about installing an old Leyland petrol engine, recovered from a wrecked lorry, together with shaft and propeller, and eventually the family were taken for expeditions up the Medway on fine summer afternoons for picnics, always accompanied by the dense smoke from the old engine.

About 300 yards from Horace's house at Haysden was another which he had built for his brother Henry. At one time they had established a brick works at Haysden and the relics of ancient heavy machinery mouldered in the meadow round Henry's house. The clay-pit adjoining the house was now filled with water which had been stocked with bream, roach and tench and one could take hundreds of small fry by float and line on summer evenings. When the business had failed large numbers of bricks had remained unsold and Horace built the house rather than allow them to go to waste.

When I reflect on these and numerous other enterprises, and of the unbounded optimism with which each venture was succeeded by a new one, the continuing, undiminished, sanguine estimates of new possibilities, it seems to me that my uncle and his brothers were fighting a losing battle for a way of life which history had left behind. They had known better times, they had unlimited confidence in their own skill and resource from the exercise of which they got great satisfaction, and they tried desperately to avoid adapting themselves to the radically new social forms which were in the ascendant and which could offer them nothing which recompensed them for the joys of a lost paradise from which they were being ejected.

One of my last memories of my uncle is of meeting when he was 75, in Tonbridge High Street, during the second world war. When I had last seen him he had been suffering from neuritis and I enquired after his health, and what he was doing. 'Working for the Admiralty', he said, 'making steel covers for some of the auxiliary gear of motor torpedo boats. We are doing it in my boathouse. We cut up 12 inch diameter steel tubes into short lengths and bore and tap holes in it.' 'But how about the neuritis?' I enquired. 'I had all kinds of treatment for it,' he replied, 'heat treatment and massage, but nothing did any good. But these steel tubes are pretty heavy and we have to use chain-tackle to handle them. At first I couldn't raise my right hand above my shoulder, but in using the tackle I found I could go higher and higher and now I can do what I like with it', and he demonstrated by waving his arm in the air. 'I have to get the finished stuff up to Woolwich and we take it by lorry. I've been through Tonbridge High Street at times during the night that I've never been in it before.' When I told my mother of this incident she remarked that though I might find it entertaining she hoped I would not make light of the fortitude which it implied.

I was fortunate that at Judd School I came under the influence of a physics master who was devoted to his profession and an able teacher. He had built circuits, with crystal rectifiers and a large aerial, by means of which we could receive the wireless signals in Morse code transmitted from the Eiffel Tower; and the experiments in his class were well designed and gave scope for originality and skill. I used to be allowed to spend a lot of time there on my own. And he was a man of some taste and originality. He was a deep admirer of Van Gogh's paintings and he had some good reproductions of some of his paintings, including some of the sunflowers, prominently displayed in the laboratory.

CHAPTER THREE

I did well in the school and was eventually entered for an open scholarship at Sidney Sussex College, Cambridge, where in 1921, I was successful at my second attempt. C. T. R. Wilson, the inventor of the Wilson Chamber, was a fellow of Sidney Sussex at that time. Although, through my mother's family, I was not altogether unacquainted with the traditions of learning, the change from a small country town to a great university was not an easy transition, involving coming to terms with new values and with new people relatively rich in material terms, in opportunity, and in experience. I managed to grow into my new situation and eventually gained a first-class honours degree. I was then faced with the problem of what I was to do.

Up to that time I had always had a clear objective. There had always been the next examination in which I wanted to do well; but now it was as if I had poked my head through the clouds. There was also the need to earn some money so that I could give support to my family. After much reflection I eventually decided to follow my grandfather and to become a teacher and I applied for a post at Uppingham School. I visited the school and was interviewed and entertained by the headmaster, Fisher, who later became Archbishop of Canterbury. After getting the job I became more and more uneasy about my suitability for a position of that kind and eventually I asked Rutherford, the Cavendish Professor of Physics, if I could be accepted as a research student. He agreed and arranged that C. T. R. Wilson should be my supervisor.

It is difficult for a generation which has seen such an immense change in the scale and sophistication of science to recapture the atmosphere of the Cavendish in the '20s. When I arrived in 1922, Rutherford had already, in 1919, demonstrated the artificial disintegration of the light elements by bombarding them with fast alpha-particles from radio-active sources. We used to see him disappearing from time to time into a partitioned corner in his laboratory, with his assistant Crowe, to count the scintillations which recorded the ejected protons. This involved long periods in darkness viewing a zinc sulphide screen under a low-powered microscope; and they used to emerge after an hour or so, blinking in the sudden light, like miners coming out of the pit.

But in 1925 when I began research, and in the next three or four years, there was a certain pause in the tempo; the main directions of further advance were not yet clear. In fact it was a period when the technical resources for the next brilliant period were being developed and mustered. The electrical counting of particles in place of the old scintillation method was elaborated by Wynn-Williams; Blackett was building equipment for the automatic operation of a Wilson Chamber to make it feasible to photograph the tracks of the alpha-particles and observe those rare chance occasions when one of them made a close collision with a struck nucleus and disintegrated it; Cockcroft and Walton were developing methods for generating steady potentials for accelerating protons to high energies of about a million electron-volts so that they could be employed as bombarding particles for collisions with nuclei.

Wilson was an exceedingly retiring man. He also had become a schoolmaster as a young man and he had fled after a short time to do research in the Cavendish Laboratory. He had achieved great fame for his invention of the cloud chamber, which allowed the tracks of ionising particles passing through a gas to be made manifest: a cylindrical, glass-walled chamber is provided with a piston whose sudden motion causes the enclosed gas to expand. The gas, often ordinary air, is initially moist with water vapour, and the sudden expansion causes a sharp drop in its temperature so that the air can no longer hold the original amount of vapour in it. We say it becomes 'supersaturated' and then some of it must condense. If at the moment of expansion, charged particles, such as alpha-particles from radium, are admitted to the chamber, the excess water condenses upon the charges or ions along the track. The trajectory of the particles then shows up as lines of droplets which form on the ions liberated by the passage of the fast particle through the atoms of the air; rather as the vapour trails of aircraft are shown up through condensation in some conditions at great altitudes. The tracks of the particles are recorded by taking flash photographs immediately following the expansion.

The Wilson cloud method allowed atomic phenomena to be demonstrated in a most vivid way and showed the essential correctness of the picture of nuclear processes which had been built up on the basis of a mass of indirect evidence. It also provided the first of a number of devices in nuclear physics which allowed far-reaching conclusions to be reached, with some confidence, on the basis of a single observation.

Wilson had few research students. The first was Mahalanobis who worked with him before the first world war; Wormell and I were next; Dee and J. G. Wilson followed. Wormell was started on some experiments on thunderstorms in which Wilson had been interested for many years. Wilson thought that they might be responsible for the origin of cosmic rays, for he computed that there must be hundreds of thunderstorms occurring all over the globe at any one time. The very large potential differences generated in them, as manifested by lightning flashes, could in principle accelerate particles to much greater energies than anything of which we had then any experience. In fact, we now know that they are quite inadequate to produce the cosmic ray particles which were later proved to have much greater energies than were visualised in the 1920s.

I was given the job of making an all-glass expansion chamber so that I could study the nature of the condensation produced by different degrees of expansion of dust-free air. With the technical resources then available, it was a difficult task. With modern glasses and grinding methods it would be relatively easy to make; but with soda-glass and emery powder it was difficult and when after a month's work, a complete instrument was eventually made, it sometimes shattered at the first expansion. I always found it remarkable that Wilson should ever have attempted to make such a device but he had succeeded 20 years before: Rutherford used to tell a story that on one occasion just before he went on a long vacation, he had left Wilson in the attempt, sitting glass-blowing at the blow-pipe and pedalling the foot-bellows. When he returned Wilson was in the same position and seemed to be engaged in exactly the same task.

But eventually I made a chamber which worked and with it carried out a series of observations at different temperatures showing how the degree of supersaturation in dust-free air required to produce a cloud of tiny droplets through gas varies with temperature. The original idea had been to see if there was any advantage in running a Wilson Chamber at temperatures different from ordinary room temperature, but by a piece of good fortune I discovered that my results had a bearing on the discharge of steam through nozzles.

It had long been known that if steam at a certain temperature and pressure is passed from a boiler and allowed to escape through a nozzle, as it does in a steam turbine for example, more steam is discharged than is theoretically possible, even if there is assumed to be no viscous resistance to the flow, no fluid friction. The theoretical discharge was computed under the assumption that, in expanding, the steam condenses immediately it cools to the saturation point. But it doesn't. The steam only begins to condense when it becomes supersaturated to the point where cloud-like condensation occurs. I found that when one allows for this, then the observed rates of discharge agree with the theoretical predictions and so my results had a modest bearing on the design of steam turbines.

Before this work was completed I was interviewed in Cambridge by A. M. Tyndall, the Director of the new H. H. Wills Physics Laboratory, who was looking for young men to assist in building up its staff. H. W. B. Skinner, whom I had known in the Cavendish, was already there and in the spring of 1928 I moved to Bristol as Tyndall's research assistant, to work on the mobility of ions in gases. When a gas is ionised, by an electric discharge or the passage of fast particles, the positive and negative ions thus produced drift in any electric field which is established in the gas. Their 'mobility' is defined as the speed of drift in centimetres per sec in a field of unit strength, 1 volt/cm, at standard pressure and temperature. Ions of different kinds move with different mobility. The subject had been studied for many years, since the discovery of radio-activity, and had attracted many distinguished physicists including Rutherford and Langevin, but the results of different observers had been widely discordant.

Tyndall and I realised that the variability in the results was due to the changes in the nature of the ions. In these early days the apparatus was often constructed of ebonite and brass, and made gas-tight by means of sealing wax. The result was that any gas admitted, however pure initially, was immediately contaminated with small quantities of water vapour, carbon dioxide and organic vapours. Such molecules attach themselves to the positive ions and electrons which are the immediate result of the ionisation of the gas, so that they are very quickly clothed in a layer of heavy molecules, and their nature and mobility changed, and the nature of the final ion, and thus its mobility, varies with the nature and concentration of the impurities.

In this situation it was essential to use an all-glass apparatus which could be heated in vacuo and de-gassed by methods which were then becoming available through the developments in vacuum technology, so that when a purified gas was admitted, it remained uncontaminated. By these means, and introducing valve methods for the generation of alternating potentials over a wide range of frequencies, we and a number of colleagues succeeded in determining precise values for the mobility of a large number of ions in a variety of gases.

In 1935, through a lucky accident, I became a member of an expedition to Montserrat in the West Indies in connection with the seismic activity which had for some years been occurring in the island. In the previous three or four years there had been many hundreds of small earthquakes with a steady increase in their frequency and there was anxiety lest one of the volcanoes in the island should erupt like Mt. Pelée in Martinique in the Antilles in 1903.

The eruption of Mt. Pelée had been the first example in recorded history of a type of eruption known as the nuée ardente. On a morning in May, whilst most of the inhabitants of the main town in the island, St Pierre, were still asleep in their beds, the top had blown off the volcano and a great jet of steam and debris had been ejected to an altitude of several kilometres. This was followed by a great flow of material at a high temperature which poured through the rift in the crater and down the mountain side, towards the town. This ejected material contained great boulders weighing many tens of tons assisted in its descent by the fact that it contained superheated steam and other vapours which, escaping from the interior in jets, turned the boulders into a kind of hovercraft. Much of this mass, accelerating under gravity down the mountain, swept through the town at a speed of some hundreds of miles an hour, completely destroying it. All the inhabitants were killed except for one man—a condemned criminal who was in the town gaol and who, protected by its massive walls, survived the immediate onslaught only to succumb a few days later.

There were 19 ships in the harbour at the time and they were all destroyed except the Roddam, which happened to have steam up at the time and managed to get away with half its crew burnt to death on its decks. Ships over 40 miles away were covered in the falling ash from the eruption to a depth of several inches so that tons of material had to be cleared from their decks.

The eruption was followed by a very bizarre phenomenon. The internal pressures still maintained within the main edifice of the volcano thrust up a great spine of heated rock, the plug which had stopped the throat of the volcano, to a height of about half a kilometre where it remained a prominent sight for many months until rapid weathering under the action of torrential rain from tropical storms rapidly weathered the hard rock so that it rapidly disintegrated.

CHAPTER FOUR

But just at this time, the Cavendish was the scene of a number of decisive discoveries which greatly excited physicists all over the world: the discovery of the pair-production of positive and negative electrons by gamma-rays in the cosmic radiation, by Blackett and Occhialini; of the neutron by Chadwick; and the disintegration of lithium by artificially accelerated protons by Cockcroft and Walton. I became very interested in nuclear physics and I determined in 1935 to build a Cockcroft and Walton generator of 750 kV in order to work in this field. We were fortunate in having a large room 20 feet high at the top of the laboratory which was not occupied and which was large enough for such a generator. I had the advantage of the help of a gifted young physicist, Fertel, who was later electrocuted in an accident on the Birmingham Cyclotron, and after working for about two years, making almost all the components in the laboratory workshop, we began to generate high-energy beams of protons.

The original intention was to study the scattering of fast neutrons by protons using a Wilson Chamber filled with hydrogen, the neutrons being generated in the disintegration of light elements, such as lithium, beryllium and boron by the fast deuterons from the generator. But about this time W. Heitler, who had been in Bristol for some years, pointed out that Blau and Wambacher had successfully used 'halftone' photographic emulsion to detect particles in the cosmic radiation and, since the method had the advantage of extreme simplicity, he thought we might begin by sending similar plates on to a mountain to see if we could simulate the Viennese results.

A photographic emulsion consists of myriads of small crystalline grains of silver bromide, with a small admixture of iodide, suspended in gelatine. The emulsion is commonly coated as a thin layer on to glass or celluloid. In normal photography, when light falls on the emulsion it produces minute changes in some of the grains so that when the plate or film is immersed in a chemically reducing solution, called the developer, some of the grains affected by light are turned into black grains of silver whilst the others are unchanged. If subsequently the plate is immersed in a 'fixing' solution, the unaffected grains are dissolved out of the gelatine whilst the silver grains remain. The plate is then washed in water to remove the chemicals introduced into the gelatine by the fixing and other solutions; and the plate is subsequently dried to produce the finished photographic negative. The place where no light fell is now transparent, and blackest where the highlights fell. The early workers in photography were much struck by the fact that 'light produced darkness' in such processes.

The action of fast charged particles is similar to that of light. The moving particle produces changes in some of the grains through which it passes so that, after processing, the track shows up as a line of developed grains, like beads on an invisible string, which can be seen and recognised when the plate is examined under the microscope at high magnification. So in the experiments on the cosmic radiation it was only necessary to place a number of small plates coated with a suitable emulsion at high altitudes on a mountain where radiation is more intense than at sea level. To keep out light, the plates are wrapped in black paper, which the cosmic rays readily penetrate, and they are recovered after a few weeks, brought home to the laboratory, and processed.

We were very encouraged by the early results. We found 'stars', points from which the tracks of several charged particles diverged, which certainly represented disintegrations of nuclei from which several, sometimes as many as ten, charged particles, protons and alpha-particles had been ejected. They were certainly due to the collision with one of the silver or bromine nuclei in the emulsion of a fast proton or neutron, part of the cosmic radiation, which had an energy many times greater than those which were available with the most powerful accelerators in operation at that time.

These modest successes encouraged us to consider the possibility of using photographic emulsions in our experiments with the 750 kV accelerator, to detect the neutrons and their scattering in place of the hydrogen-filled Wilson Chamber, as first proposed. Neutrons carry no charge and produce no direct ionisation in a gas. But they sometimes make close collisions with the nuclei of the atoms in any material medium through which they pass. If the medium contains hydrogen they sometimes make 'head-on' collisions with the nuclei of these hydrogen atoms, the protons; the neutron is then brought to rest and all its original energy of motion is imparted to the proton which is projected forward in the original line of motion of the neutron. The protons do ionise and produce tracks, and the greater their energy the further they go before being brought to rest by the loss of energy they impart to the atoms through which they pass. The length of the track of the proton, its range, therefore gives a measure of its energy. By measuring the ranges of these recoiling protons, their distribution in energy and thence that of the neutrons from which they have recoiled can be inferred.

At that time this method was the basis for determining the distribution of energy of the neutrons emitted in the disintegration of light elements, the neutrons being detected in a Wilson Chamber filled with hydrogen. But the method was arduous and in a characteristic experiment 30,000 photographs had to be taken to establish a particular neutron spectrum so that the whole experiment might take six months. When using our Cockcroft generator, we placed a few square centimetres of a half-tone emulsion near a 'target' of beryllium bombarded with 700 kV deuterons, we recorded thousands of recoiling protons and were able to determine the neutron 'spectrum' with a higher precision than had been obtained with the Wilson Chamber, the measurements being completed in one or two days only.

When I reflect about this period it seems to me that in several respects we were remarkably fortunate in arriving at this point in the development of the method. In the first place, we were ignorant of work which had been done in other laboratories which indicated that the tracks of protons in available emulsions were too tenuous, the grains too few, to allow the observed length between the first and last grains in a track to provide a reliable measure of the true range and therefore of the energy of the particles. Secondly, the commercially available emulsions of the time, designed for a quite different purpose in ordinary photographic practice, were erratic in their performance in recording tracks, and the first we employed were amongst the best of the type we encountered. And thirdly, we approached these problems just at the time when artificial sources of protons were becoming available which allowed a clear demonstration of the precision which could be achieved. Previously the protons had commonly only been available in the disintegrations of light elements by alpha-particles from radioactive elements. The sources were then very weak and there were no groups really homogeneous in direction and energy so that a convincing comparison with the results given by other methods was difficult.

So we were much encouraged. But some of our colleagues were not yet convinced, for we might have been prejudiced in our results by knowing the main features of the neutron spectra we examined which had previously been determined with the Wilson Chamber. In 1939 we therefore exposed plates to the neutrons produced by bombarding fluorine with the higher energy deuterons produced with the Cambridge accelerator and determined the resulting neutron spectra. Nothing had been published on the 'spectrum', but it was being examined with the Wilson Chamber in Cambridge. But again, in a few days, we obtained results which were closely similar to those obtained in many months by the old method.

We now turned our attention to the possibility of using plates to examine in detail the disintegrations produced by the beams of protons and deuterons which were now being generated with the Liverpool cyclotron. For this work the plate had three important advantages over other methods: firstly, it is continuously sensitive; secondly, it can record with precision the point of entry of a particle and its direction of motion; and thirdly, the nature of the particle and its range and energy can be determined by observations on the character of the tracks. A single large photographic plate was arranged with its plane parallel to, and about 2 cm below, the axis of a narrow pencil beam of particles from the cyclotron, homogeneous in energy and direction, in an air-tight 'camera' containing a gas at a low pressure. The beam passed down the axis of a cylindrical tube provided with a short aperture through which any particles resulting from the impact of the particles in the primary beam with the nuclei of the gas atoms could pass to enter the emulsion. It was thus possible to record simultaneously particles of all types and ranges, emitted over a wide range of angles in a single exposure.

In the first experiments of this type Chadwick, Pickavance, May and I studied the collisions of 4 MeV protons with neon nuclei and the angular distribution of the elastically scattered protons. We also established the existence of an inelastic process in which the struck neon nucleus is raised to an excited state, the proton emerging with reduced energy. Later, in collaboration with J. Rotblat, we applied similar methods to a wide range of experiments using the 6—7 MeV deuteron beam, which the Liverpool cyclotron could also produce, and studied a wide range of interactions. In the 20 years which followed this method was widely applied in experiments of this type.

But in addition, during the war years, F. C. Champion and I found time from other duties to pursue the question of the scattering of homogeneous groups of neutrons by protons. Contrary to other observers at that time we were able to show that the scattering is isotropic in the centre-of-mass system of the collision up to energies of almost 10 MeV. And during this time I was also still looking at plates exposed on the Jungfraujoch immediately before the war and the possibilities of the cosmic-ray work. What we now call the mu-mesons or muons had been discovered by Anderson and Neddermeyer in 1936 and I have a note in an old note-book for 1938 to the effect that it is easy to show, from the observed characteristics of the tracks of protons in the 'half-tone' emulsions we were then using, that the tracts of 'mesotrons' or 'baryons', as they were then called, should also be recognisable at the end of their range.

CHAPTER FIVE

In 1945 we were joined in Bristol by G. P. S. Occhialini who had returned to England from Brazil before the end of the war. He was enthusiastic about the potentialities of the photographic method and made approaches to Ilford Ltd, who produced our plates, with a view to improving their recording properties. Although we had already shown that reliable range measurements could be secured, tracks were only distinguishable if the number of developed grains along the track was sufficiently numerous. This depends upon the number of ions knocked out of atoms by a particle per unit length of its path, a number which gets lower the higher the velocity of the particle. In effect, as matters then stood, we could detect particles only when they were of relatively low speed, such as we encountered in the experiments on the cyclotron, but most of the particles with speeds approaching that of light, which are the most significant in cosmic radiation, eluded us.

There were a number of ways in which it seemed possible that the recording properties of emulsions could be improved; by increasing the size and sensitivity of individual grains, for example, or by increasing the number of grains in unit volume of emulsion. C. Waller was the research chemist at Ilford's at that time and Ilford's methods of manufacture were such that he found it possible to make emulsions with a very substantial increase in the concentration of silver bromide. When the new emulsions were exposed and developed it was clear that a very remarkable improvement in recording properties had been achieved.

Occhialini immediately took a few small plates coated with the new emulsions—about 2 dozen each 2 cm x 1 cm in area, with emulsions about 50 microns thick—and exposed them at the French observatory in the Pyrenees at the Pic du Midi at an altitude of 3,000 m. When they were recovered and developed in Bristol it was immediately apparent that a whole new world had been revealed. The track of a slow proton was so packed with developed grains that it appeared almost like a solid rod of silver, and the tiny volume of emulsion appeared under the microscope to be crowded with disintegrations produced by fast cosmic ray particles with much greater energies than any which could be generated artificially at the time. It was as if, suddenly, we had broken into a walled orchard, where protected trees had flourished and all kinds of exotic fruits had ripened in great profusion.

Among the earliest disintegrations which excited us was one from which a particle was ejected which, at the end of its short range, seemed to be crossed by a short straight track. This raised one of the technical difficulties inherent in the method. It is a great advantage of the photographic method, and also one of its limitations, that it is continuously sensitive, from the time the emulsion is dried during manufacture, up to the moment that the plate is developed. Whereas the tracks recorded in the expansion of a Wilson Chamber, or of a bubble chamber, are certainly contemporaneous within a period of the order of a few milliseconds at the instant of the expansion, all that one can assert of an emulsion is that the tracks were recorded at some time between manufacture and development.

The problem presented by the 'hammer tracks' was therefore whether the 'handle' and the 'head' of the 'hammer' were due to related particles, or whether they represented an accidental coincidence of events quite unconnected with one another. This problem repeatedly occurred in the subsequent development of the method and was of crucial importance in a situation where far-reaching conclusions might be drawn from a single, or a very few, isolated observations. In fact it is easy to show that the probability of a chance, but precise, juxtaposition of tracks is extremely improbable with the density of tracks in emulsions which were commonly employed. The a priori probability, for example, that the track of one particle shall begin precisely at the point at which that of an unrelated particle ends, is of the order of 1 in 10^7 with typical conditions of exposure. Nevertheless before drawing important conclusions, it is always essential to observe great caution in scrutinising the tracks.

We were greatly puzzled by this hammer track until I recalled that it had recently been shown that the nuclear species, lithium eight, Li_3^8, which had been artificially created in nuclear disintegrations of moderate energy, has a mean lifetime of about 1 sec and transforms with the emission of an electron to produce beryllium eight in an excited state; and that this excited nucleus immediately disintegrates into two alpha-particles, each with an energy of about 4 MeV which recoil from one another with equal and opposite momenta, and which therefore have the same range. It seemed clear that such a process would account precisely for the characteristics of our 'hammer tracks'; that we were 'seeing' the emission of the Li^8 nucleus during the disintegration of one of the silver or bromine nuclei of the emulsion and its subsequent decay, about a second later, into two alpha-particles. Subsequently many other events of a similar nature were observed which gave us confidence in the correctness of this interpretation, but during this early period we were denied the confirmatory evidence which the track of the decay electron would have provided, for the new emulsions were not sensitive enough to give recognisable electron tracks.

These new observations produced an atmosphere of the liveliest enthusiasm and anticipation among us. We began an intense search of the small areas of the new emulsion which had been exposed and took steps to get more experimental material. We had by this time several microscopes which could be used for searching the plates and a number of girl observers and a feverish hunt began. Almost every day produced something new and exciting. At the beginning, the observers when they found any 'event' in their search, such as a disintegration, would call a physicist to scrutinise it to see if it showed any remarkable features. Almost immediately Peter Fowler, who was in his final year as an undergraduate at that time, was shown an event in which associated with a small disintegration, there appeared to be a particle, a meson of mass about 200 m_e, which had reached the end of its range at the point where a disintegration occurred. There were only two possible explanations of the observed tracks. Either the particle had come to rest at a point which, by chance, coincided with that of a completely independent disintegration; or the tracks were related in which case the sequence of events was unambiguous. The particle of relatively small mass, the meson, must have reached the end of its range and produced a nuclear disintegration, when it was at, or almost it, 'rest', with little or no energy of action.

Just at this time D. Perkins, at the Imperial College in London, who had independently been making similar experiments with the new emulsions, found an 'event' with closely similar characteristics. The observation of two 'events' of a similar nature seemed to exclude completely the possibility of a chance juxtaposition of unrelated tracks and it appeared certain that we were observing the consequences of the capture of a negative meson by a nucleus of an atom in the emulsion, and its resulting disintegration. Such a process had been visualised to account for observations on the decay of the positive and negative muons, present as the penetrating component of cosmic radiation, when they are arrested in different materials. Counter experiments had shown that the positive muons always decay into an electron, irrespective of the material by which they are slowed down and brought to rest. On the other hand, whilst negatively charged muons stopped in materials of low atomic weight like carbon are observed to decay with the emission of an electron, when arrested in heavy elements like lead, the decay electron is commonly not observed. It was therefore believed that the nuclei of heavier elements commonly capture a negative muon before it has an appreciable chance to decay and are thereby disintegrated. It was therefore reasonable, at that time, to attribute the disintegrations observed in emulsions by slow particles at the end of their range, to the capture of muons by silver or bromine nuclei, a conclusion which was later called into question.

The observers soon learned to recognise the tracks of mesons and we found many examples of similar disintegrations produced at the end of their range. Indeed the interest and liveliness of the observers was a crucial element in the progress of the work. They learned to distinguish by inspection the tracks of stopping mesons, of mass about two hundred times that of the electron, from those of protons and alpha-particles, for there are characteristic differences between them which an experienced observer soon recognises. And we took a good deal of trouble to help them to learn to interpret the events they found and to understand the significance of what they were doing.

Very soon after the observation of disintegrations produced by slow mesons, which gave a dramatic demonstration of the transformation of mass into energy since the whole mass of the meson disappears and no kinetic energy contributes to the disintegration, an observer directed my attention to an 'event' which puzzled her. It appeared that a meson had come to rest, and from the point where it had stopped, a second meson had emerged. This second meson had passed out of the emulsion before stopping, but it was only moving slowly when it escaped from observation, and its true range can have been only a little greater than the observed length of track.

This observation greatly excited us. Again there was the possibility of a chance juxta-position of unrelated tracks, but the small number of slow mesons recorded in our plates made the probability of two meson tracks being so precisely associated exceedingly remote. Further, we had not, at that time, found a single example of a meson whose track appeared to originate in the emulsion.

But this observation allowed us to discuss the kind of similar event for which the observers ought to keep a close look-out, and a day later a second example was found which seemed precisely similar in significance to the first, but with the advantage that the secondary meson, diverging from the end of the range of its parent, was this time arrested in the emulsion so that its precise range could be determined. We were immediately struck to find that the two secondary mesons appeared to be ejected with equal, or closely similar, velocities.

At this stage we had the advantage of having worked on homogeneous groups of particles from the artificial disintegration of the light elements and had thus established a range-energy relation for protons. The faster a particle, the further it goes before being arrested in a given medium. If for particles of a given mass and charge the relation between velocity and mean range is known, or that between energy and mean range, a similar relation for other particles of the same charge but of different mass may readily be computed. Assuming the secondary particles from these two events were of mass about 200 m_e, the energy with which they were ejected could therefore be estimated from their range. Their observed range of about 400 microns, 0.4 mm, then indicated that they were emitted with an energy of about 4 MeV.

This second event gave us great confidence that we were dealing with an important phenomenon and not a chance juxtaposition of unrelated tracks. If the chance of one such association was small, the chance of finding two representing closely similar phenomena was entirely remote. But what kind of process was involved? It seemed certain that a meson had reached the end of its range and that a second meson had been ejected from the point of arrest. But how long the first meson rested at the end of its range before the secondary was ejected was entirely uncertain, and although there were indications that the secondary emerged with a definite energy such a view at this stage could not be asserted.

The most obvious explanation of the process was that the primary meson was more massive than the secondary; that those we were observing were positively charged and that on reaching the end of their range they transformed spontaneously into secondaries of the same charge, the kinetic energy being provided by the equivalent mass which disappeared in the process. It then followed from the principles of conservation of momentum and energy, that if the secondary meson was indeed always being ejected with the same energy, only a single neutral particle was recoiling from it at the instant of decay of which the track would, of course, remain unobserved.

Under these assumptions, if the secondary were identified as the penetrating particles of the cosmic radiation, whose mass was known to be about 200 m_e, and if the neutral particle was a neutrino or a gamma-ray of zero-rest mass, then it was easy to show from the conservation laws that the mass of the primary must be about 265 m_e. These tentative conclusions were later substantiated and illustrate the power of method to permit far-reaching conclusions on the basis of a single, or of a very few observations. But at the time there were other possible explanations which had to be considered.

In particular, Charles Frank in Bristol examined the possibility that the phenomenon was produced in an entirely different way: that the primary meson was negatively charged; that when arrested it was captured by one of the nuclei in the emulsion with which it interacted. As a result the nuclear charge was reduced by two units, a positive meson with the same mass as the first subsequently emerging with 4 MeV of kinetic energy, which was provided by the change in the mass of the capturing nucleus associated with its transformation. Frank showed that no isotope of the common elements in the emulsion could provide a consistent explanation on the basis of such assumptions.

But it was clearly crucial to find more examples of the process, so that the constant velocity of ejection of the secondaries could be established and so that favourable examples of flat tracks could be found which would permit estimates of the masses of both the primary and the secondary mesons to be made by the rough methods which were then available. So the search for further examples was pressed with great vigour and enthusiasm.

With eight or ten observers searching the plates, it was six or eight weeks before we found any more; then, once again, two appeared in quick succession, and in a month or two more we increased the number to ten. The new events confirmed that the secondary mesons do indeed commonly appear with kinetic energies constant within a narrow range, and we were confident we were involved with two different particles with masses of the order of 200 and 260 electron-masses, respectively.

Our early reports on these results were received with a certain very proper reserve by other physicists. The early deficiencies were remembered and there was hesitation in giving credence to such important conclusions on the basis of a method with which most physicists had had no experience. Further, our findings were supported by mosaics of micro-photographs, for as a track wanders at changing depths in the emulsion it passes out of focus; a succession of overlapping photographs must be taken with appropriate settings in order to obtain enough to construct a mosaic which gives a picture of the whole event at high resolution. In this situation a lecture I gave in Copenhagen in the early spring of 1947 was of particular importance.

With ten or twelve secondary meson tracks measured, it was possible to begin to plot the distribution in the values of their range and the evidence that they represent a group homogeneous in velocity. A group of particles of precisely the same initial speed when allowed to enter a given medium do not all have precisely the same length of path before being brought to rest. There is a variation in their ranges owing to the fact that they lose energy in hundreds of thousands of encounters with individual atoms, sometimes losing more, sometimes less energy in the different collisions. The resultant final range is therefore a statistical process and the variation in range is referred to as the 'straggling' of the particles. This process had many years before been studied by Niels Bohr and he and O. Bohr had computed the expected 'straggling' as a general problem so that they were in a position to predict the spread in range to be expected by a homogeneous group of muons of energy 4 MeV.

There had been previous speculations by theoreticians, that there were two kinds of mesons in nature, notably by Sakata in 1944 and later by Marshak in 1947, but we had known nothing of them. These speculations were based upon what seemed an irresolvable contradiction in the behaviour of the cosmic ray mesons which we now call muons. The muons constitute the penetrating component of the cosmic radiation and have a mean lifetime of about 2 micro-seconds. They were believed to be created in the high atmosphere when the incoming protons which form the greater part of primary flux of the cosmic rays approaching the earth from outer space collide with the nuclei of the atoms of the air. The fact that the mesons are copiously produced in such interactions implied that they have a very strong interaction with nuclei, that they are 'strongly-interacting particles' to use the modern idiom. On the other hand, we have seen that previous experiments had shown that those negatively charged, when they are captured by light elements such as carbon, decay with mean lifetimes approximately equal to the value found when the particles are free. It was realised that this implied that they are captured into states of low energy round the capturing nuclei, where they spend an appreciable fraction of their time in the nucleus itself, but nevertheless commonly fail to interact with it. Such a behaviour is impossible for a strongly-interacting particle which should interact in a time of the order of the character nuclear time: viz: about 10^{-23} sec.

These difficulties were readily resolved by the discovery of the two types of mesons. The primaries, designated pi, or pions, are produced directly in nuclear reactions but they rapidly decay, when in flight in the atmosphere, to produce the secondary mu-mesons or muons, which proceed nearly in the line of motion of the parent pions and form the penetrating component of the cosmic rays.

With the realisation of this possibility, the nature of the disintegrations observed in emulsion by slow mesons at the end of their range was ambiguous. Some of them could be attributed to local disintegrations produced by the impact with nuclei of the stream of fast neutrons in the cosmic ray flux which produced relatively slow negative pions which were arrested before they had time to decay and which were captured by nuclei in the emulsion. But others had to be attributed to the negative muons which were known not to decay when captured by the heavier nuclei in emulsion and it was reasonable to assume that they too would produce disintegrations.

CHAPTER SIX

But we were still very much concerned at the wealth of phenomena which must still be occurring in our emulsions but not being recorded; the tracks of all singly charged particles moving at velocities near that of light failed to give recognisable tracks and we continued to urge our colleagues in the photographic industry the great advantages to be gained by a further increase in the sensitivity of the emulsions. This was achieved in the course of about 18 months, first by Kodak Ltd, whose research department near London produced the first electron-sensitive NT4 emulsions; and later by Ilford Ltd.

The electron-sensitive emulsions allowed tracks produced by particles with a specific ionisation four times less than the previous minimum value to be recognised. They were then able to record the tracks of all particles with the electronic charge even when moving at relativistic velocities. Their production was a technical achievement of great significance. D. M. Ritson took a few square centimetres of the first samples we received to the Jungfraujoch, where after two weeks' exposure he successfully developed them and brought them back to Bristol, and we immediately began to examine them.

We had been anxious that even a short exposure of the new emulsions at mountain altitudes would result in recording a large density of tracks, a confused 'jungle', which would make difficult the interpretation of individual events; but this was not so. Almost immediately we found an example of the track of a positive pion, which had first decayed with the emission of a muon, the muon subsequently transforming into an electron of which the track was clearly distinguished; other examples quickly followed.

But in addition we were again fortunate to find, only a day or so after beginning the systematic examination of the new plates, an event which greatly stimulated our interest and excitement. A particle, of which we estimated the mass to be about 1,000 m_e, appeared to come to rest and to transform into three secondary particles one of which was a negative pion which produced a disintegration at the end of its range. The directions of ejection of the three secondary particles were coplanar and two of the secondaries although their nature could not be established were certainly either pions or muons. Assuming them to be pions there appeared to be a momentum balance between the three secondaries so it was reasonable to assume them to be the product of the transformation of their parent particle, whose mass could then be computed from the law of conservation of energy. The result was about 960 m_e, consistent with the direct estimates based on measurements on its track.

These tentative conclusions were later confirmed and provided a second striking confirmation of the possibility of drawing far-reaching conclusions from a single observation; but we had to wait for more than 18 months before a second example was found by Hodgson. After two years we found other examples in Bristol and the existence of the K-mesons and this particular mode of decay was fully substantiated.

The electron-sensitive emulsions also allowed us to decide whether the neutral particle which recoils from the muon when the pion decays is a gamma-ray or some form of neutrino. If the momentum and energy balance is indeed provided by a gamma-ray, its energy must be of the order of 30 MeV. Such a gamma-ray in passing through the emulsion has a certain probability of materialising into a pair of electrons, both of which will proceed as a close pair only narrowly inclined to the direction of motion of the parent gamma-ray. O'Ceallaigh examined a large number of decaying positive pions and chose a sample in which the emitted gamma-ray, if it existed, must have moved in a direction such that the tracks of any electron pairs into which it materialised would have been readily detected. None were found and he was thus able to show that the neutral recoiling particle is rarely, if ever, a gamma-ray. It was therefore reasonable to attribute it to some form of neutrino, possibly, but not certainly, identical with the neutrons of beta-decay. It was more than 12 years before experiments with accelerators demonstrated that this particle, the muon neutrino, is indeed different from the electron-neutrino; and before beams of such neutrinos of high energy became available for decisive experiments on the fundamental features of the weak interactions and the riddle which the existence of the muons and different neutrinos poses.

In the course of these observations O'Ceallaigh discovered a second mode of decay of the K-mesons. He found a particle, again of mass about 1,000 m_e which transformed into a secondary mu-meson of range much greater than the typical values of $\pi \rightarrow \mu$ decay. Later similar examples were found in which the secondary mu-meson was found to be emitted with different values of energy, and it seemed clear that more than one neutral particle was being ejected in such a mode of decay, together with the muon. This is the mode now referred to as $K_{\mu 3}$; $K^+ \rightarrow \mu^+ + \pi^\circ + \nu$. A little later still he obtained tentative evidence for the mode $K_{\mu 2}$; $K^+ \rightarrow \pi^+ + \pi^\circ$. The methods of determining the mass of a particle by observation on the characteristics of its track were of very limited precision and it took a number of years before it was established that the heavy mesons were all of the same mass and that we were observing their decay in different modes.

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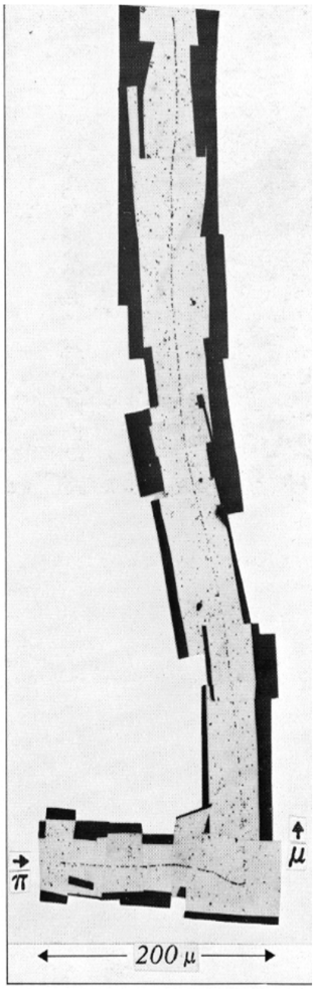
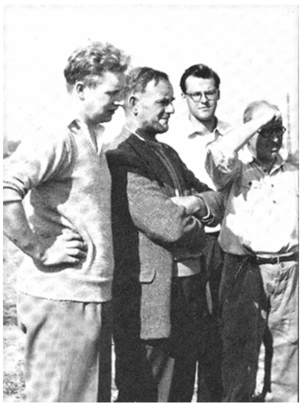
Cecil Powell's Autobiography: illustrations

Cecil Powell with his wife Isobel

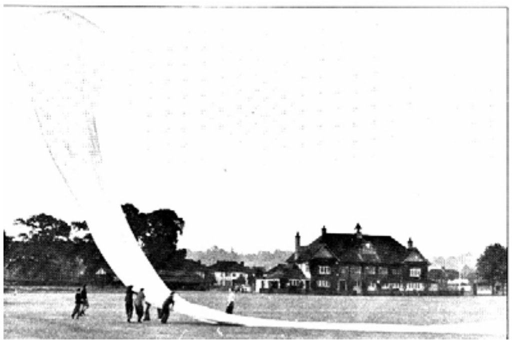


Primary meson track, π, with secondary light particle, μ

Cecil Powell with two school-friends



With John Davies & Hans Heitler



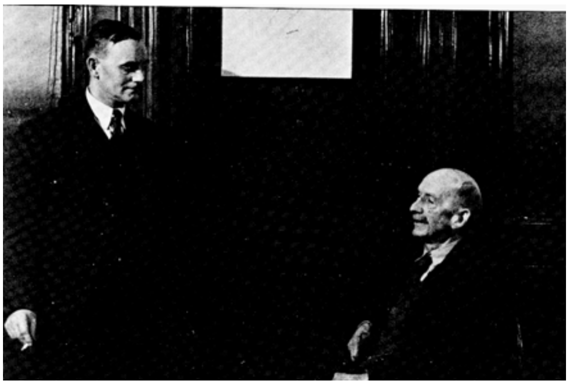
Inflating a balloon with hydrogen at Coombe Dingle

Watching a balloon launch

A launch at Coombe Dingle

With Occhialini and his family

With MGK Menon



With CTR Wilson

With Prince Philip, MHL Pryce et al

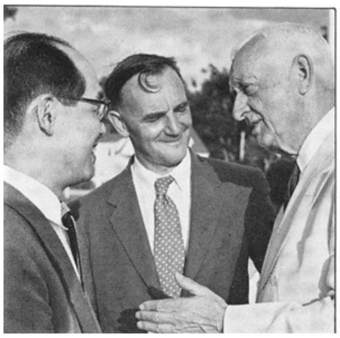
With Yukawa and Mrs Yukawa

Nobel prize:the audience

Nobel prize: the laureates

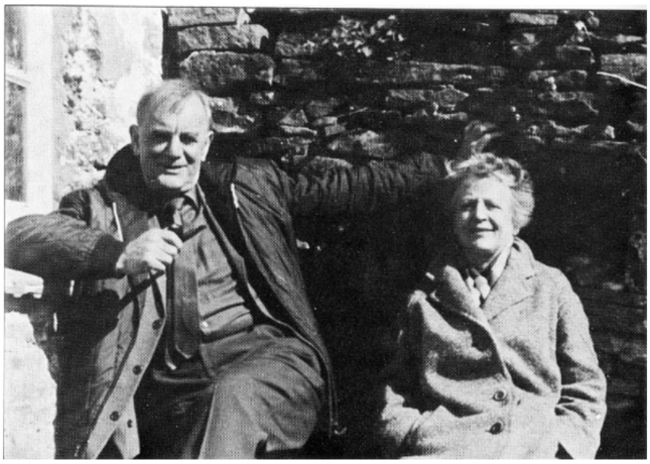
Giving the Nobel lecture

Chairing the 1957 Pugwash meeting



At the 1957 Pugwash meeting, with Yukawa & Eaton

At a Christmas party shortly before his retireement, telling Kevin Tindall the story of 'the bargee, 'is wife and 'is 'orse'



Cecil and Isobel, Easter 1967