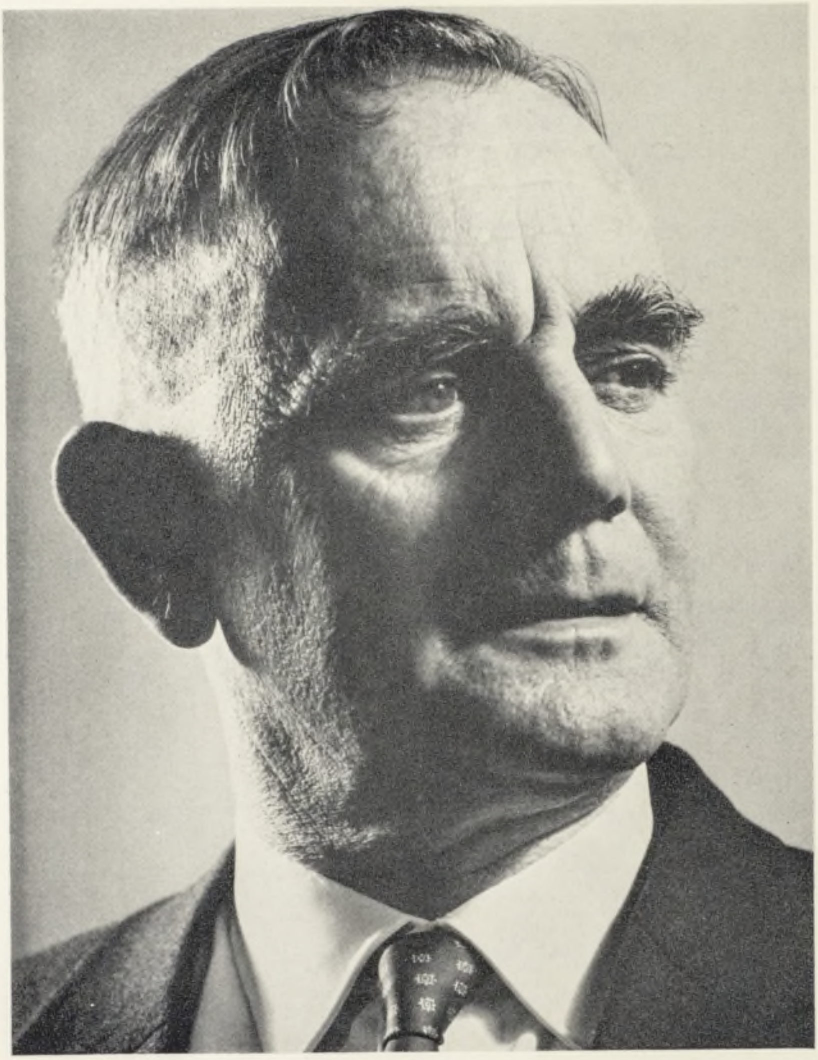

C. F. Powell.

CECIL FRANK POWELL

1903-1969

Elected F.R.S. 1949

Cecil Frank Powell died suddenly on 9 August 1969, on holiday with his wife in Italy, immediately after his retirement as Head of the Department of Physics at Bristol University. He had been elected to the Royal Society in 1949, receiving the Hughes Medal in the same year. He gave the Bakerian lecture in 1957, a Tercentenary lecture in 1960, and received the Royal Medal in 1961. He had received the Charles Vernon Boys Prize of the Physical Society of London in 1947. He received the Nobel Prize for Physics in 1950; and in 1967 the Academy of Sciences of the U.S.S.R. gave him its highest award, the Lomonosov Gold Medal, he being the first foreigner ever to receive it. He was elected a Foreign Member of the Academy of Sciences of the U.S.S.R. in 1958, and of the Yugoslav Academy of Sciences and Arts in 1966. He was an Honorary Member of the Royal Irish Academy, and of the Leopoldina Academy, Halle; an Honorary Doctor of Science of the Universities of Dublin, Bordeaux, Warsaw, Berlin, Padua and Moscow; and an Honorary Fellow of Sidney Sussex College, Cambridge, and of the Institute of Physics and the Physical Society. He had been Chairman of the Science Policy Committee for CERN 1961-1963; Chairman of the Nuclear Physics Board of the Science Research Council 1965-1968. He was also Chairman of the Cosmic Rays Commission of IUPAP. He was President of the Association of Scientific Workers from 1952 to 1954, President of the World Federation of Scientific Workers from 1956 till his death, and was a founder member of the Pugwash Movement from its inceptions between 1954 and 1957, presiding at the plenary meetings of the First Pugwash Conference and becoming Chairman of its executive, the Pugwash Continuing Committee, in 1967.

He was born on 5 November 1903 at Tonbridge, Kent, the son of Frank Powell, a gunsmith, whose family had long practised that trade in the town. His mother, Elizabeth Caroline (*née* Bisacre), came from a Huguenot family settled in the Cotswolds, and her father, George Bisacre, had established a private school at Southborough, near Tonbridge. The family had a high regard both for learning and the practical arts. His father's brother Edwin had constructed the first successful motor-car in the district, and another brother, Horace, was a practical engineer of great ingenuity and resource, but without the advantages of professional academic training. One of his mother's brothers, however, had succeeded in spite of difficulties in reaching Trinity College, Cambridge, becoming a successful engineer and, later, a publisher. Powell's mother determined to try and secure similar educational opportunities for her son, and it was to her persistence that many of his later opportunities were due.

Frank Powell had been rendered bankrupt by a lawsuit consequent upon a shooting accident—a case which, as 'Powell's case', had some considerable fame among lawyers, so C.F.P. learnt many years later. The effect of this on the family circumstances, and C.F.P.'s first introduction to science are both graphically revealed in an account told in his own words: 'By accident I was very early made aware of the important distinction between book learning and practical experience; of the importance of "a commerce of the mind with things".

'My maternal grandfather was a school teacher, and when I was a boy of about twelve, living in Tonbridge, he used to visit us from time to time bearing gifts in the form of text-books on the sciences. He lived in Southborough, only two miles away, which had been the home of a number of cramming establishments. There were several booksellers there from whom, for ½*d* or 1*d*, he was able to buy second-hand text-books on subjects which he regarded with awe, but which he thought ought to be within the compass of a young mind. I remember the binding, but nothing else, of books on such subjects as "Advanced Trigonometry", "Solid Geometry" and "The Calculus of Variations". Most of this was completely beyond me, but among these riches there was a book on "Chemistry" by Perkin and Kipping which captured my imagination. I think I was first attracted to it because it had a pleasant binding and was on good paper, generous in the size of page and print, and well set out. But when I came to read it, I found it full of romance. It was all about things with names which excited the imagination like "spirits of nitre", "sugar of lead", "corrosive sublimate", "fuming sulphuric acid", "yellow phosphorus" and "spirits of salt", and it described such fascinating exploits as producing insoluble lead iodide by the process of double decomposition. When you mixed two colourless solutions, one of sugar of lead and one of potassium iodide, there was an immediate precipitate of tiny, brilliant yellow crystals; or so the book asserted. I wanted to do some of these things for myself.

'My mother was deeply concerned for my education and because my spelling was poor, she used daily to give me dictation, reading out the leading article from the *Daily Mail*. We were very poor at the time—my father used to get 25*s* a week—but I managed to persuade her to acquiesce in my proposal to 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 Christmas, 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 nitric acids. How much I owe him; 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 the 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 window, but there was a shelf six inches wide running across one end, and it could be illuminated by the light of a candle.

'With the 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 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, glassware, 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.'

This account has introduced Cecil Powell's Uncle Horace, the utterly self-reliant Jack-of-all-trades, who was the subject of many of the stories with which he liked, on social occasions, to exercise his skill as raconteur. They were intended as humorous stories, but showed real admiration. In many ways Powell endeavoured to emulate his uncle, always liking to do what he could with his own hands, including making his own furniture; and when he came to be involved in planning the large collaborative team efforts of 'big science', it was his constant endeavour to secure that every member of a team had some part in the project which was his personal responsibility, if possible involving physical as well as intellectual activity.

C.F.P. went first to the local elementary school from which, at eleven, he won a scholarship to the Judd School at Tonbridge, where the highly reputed physics master F. Jarvis made his choice of subject unambiguous. He proceeded with a State Scholarship and a College Open Scholarship to Sidney Sussex College, Cambridge, and graduated in 1925, obtaining First Class Honours in Parts I and II of the Natural Sciences Tripos. He was placed second in his year for physics. He accepted a teaching post at Uppingham, but did not take it up, preferring the opportunity to do research in Rutherford's laboratory, under the direction of C. T. R. Wilson. In 1928 he moved to Bristol as Research Assistant to Professor A. M. Tyndall. There he became Lecturer in Physics, 1931, Reader in Physics, 1946, Melville Wills Professor of Physics, 1948, Henry Overton Wills Professor of Physics and Director of the H. H. Wills Physics Laboratory 1964-1969, and at the same time a Pro-Vice-Chancellor of the University, 1964-1967.

In 1932 he married Isobel Therese Artner, born in Hamburg, the daughter of an Austrian father and a Scottish mother. This proved a very happy union. She was to give close support to every aspect of his life and work from then on, and it would be difficult to overestimate her contribution. The first of their two daughters was born in 1933. The immediate consequence was that he found his income inadequate, and applied for a research post with B.T.H. at Rugby. He received an offer at more than double his University salary, and was prepared to accept it: but this was vetoed by his wife who was sure that he would not be as happy in a commercial setting as at the Royal Fort, and Tyndall was able to arrange an increase in his salary, averting the crisis. There can be little doubt that the choice thus made for him was right.

It was not until 1938 that he was to find the scientific subject which he could make peculiarly his own. Max Delbrück, who shared rooms with him in Bristol in 1930 (and introduced him to the girl he married), says: 'I would like to characterize Cecil's attitude at that time as one of enjoyment in his technical competence without any deep interest in science.' Delbrück attributes to Powell's wife the truly deep interest in science, its purposes, and its political implications, which he developed in later years.

His first research work, at Cambridge under C. T. R. Wilson, on condensation phenomena, had the original intention of determining whether better track photographs could be obtained by operating a Wilson chamber at other than room temperature. Its most important outcome was one of significant consequence for engineering, the discovery that supersaturation in rapidly expanding steam accounted for an anomalously high rate of discharge of steam through nozzles.

His work during his first four years in Bristol, for the most part with Tyndall, was concerned with measuring the mobilities of ions in gases, eliminating uncontrolled sources of impurity which had previously made the nature of the ions variable and uncertain. This work established the nature of the ions in most of the common gases, and yielded accurate measurements of their mobility. In the last of this series of papers Tyndall's name is absent and Powell is assisted by L. Brata, a Siamese. Two papers follow, in 1933 and 1935, with L. Brata and R. L. Mercer respectively, on new sources of positive ions (of indium and thallium, generated thermionically on oxide surfaces).

In 1935, to quote from a memoir by Tyndall, 'discussions . . . raised the doubt whether in the future any Laboratory would acquire full international prominence unless some branch of nuclear physics was a subject of experimental investigation within it'. In consequence, Powell assumed the responsibility for constructing a 700 kV Cockcroft generator, and in due course a cloud chamber to register tracks from its highly focused 20 μ amp proton beam. This was his concern till 1939. In 1940, as the laboratory filled up with other occupants displaced by war, the high voltage generator was dismantled, never to be assembled again.

Nevertheless, at the outset of this project, Powell was diverted from it by being appointed seismologist to the expedition to Montserrat organized jointly by the Royal Society and the Colonial Office, to investigate the series of earth tremors there, which it was feared, might lead up to a volcanic eruption similar to that of Mont Pelée in Martinique in 1903. With rather primitive instruments the foci of the earthquake activity were located, and in due course A. C. MacGregor and Powell reported on their studies leading up to the correct conclusion that this outcome was not to be expected.

At the end of 1937 or beginning of 1938, W. Heitler (in Bristol from 1933 to 1941) showed Tyndall a publication by Blau and Wambacher, remarking that the method was so simple that 'even a theoretician might be able to do it'. Encouraged by Tyndall, Heitler exposed an ordinary photographic plate on the laboratory roof, and on examining it with a microscope saw some tracks and a 'star' (the latter due to some radioactive decay). Heitler suggested absorption experiments in air and lead, and Powell devised the box of photographic plates and lead sheets which Heitler carried to the Jungfraujoch at the end of July or beginning of August 1938, to be brought back 230 days later in March 1939. Powell also exposed a plate tangentially to his proton beam, was impressed by the results, and then thought of using the technique with 'knock-on' protons to determine the energy spectra of neutron sources. From this time on he set about the task of devising precise ways of measuring track characteristics, to develop the photographic emulsion method into a precision tool of particle physics. The letter to *Nature* by Powell and Fertel on 'Energy of high velocity neutrons by the photographic method' (15 July 1939), appeared just before the letter (by Heitler, Powell & Fertel, 12 August 1939) on 'Heavy cosmic ray particles at Jungfraujoch and sea level'. Miscellaneous diary notes of R. L. Mercer (his assistant then, and a friend from schoolboy days: his complete diaries for this period were unfortunately destroyed) show Powell gradually concentrating his attention on to the new method during 1938 and 1939. In 1938 the high voltage set and cloud chamber took most of his time (though on 17 March he was in London discussing another proposed seismological expedition to Dominica). The 20 μamp focused proton beam was obtained on 4 May. On 24 March 1939 Mercer joined the newly-formed track-measuring team for photographic plate work 'C.F.P., Fertel Stobbe and girl', or 'Cox, Fertel and the little girl' according to another note. He had to leave it because of conjunctivitis six months later. On 17 April a new Leitz binocular microscope arrived. On 28 May Powell states the need for 'three more microscopes and three girls'. (His future style of work-organization had already taken shape.) Fröhlich recalls that at the outbreak of war Tyndall spent all resources available to him in buying up German microscopes that were still available in this country. Mercer notes '19 September 1939, Phoned Dunscombe (the local optical dealer)—No news of the Leitz microscopes'. On 17 November he has 'Letter from Chadwick offering C.F.P. use of the 5Mvolt cyclotron for proton-proton scattering'. Powell was sure of the importance of the new method. On 20 November he told Mercer 'Between you and me, boy, we are at the centre of world physics' and 'After only a few weeks, Champion is as fast and as accurate as I'. From 1940 onwards Powell applied the photographic method to high energy neutron spectroscopy for the British Atomic Energy Project, but the main harvest of results and development of methods was to follow after the war. An appendectomy, with peritonitis, in 1941, diminished his activity for a while.

It is important to realize that in his successful effort to make the direct registration of tracks in the photographic emulsion a method of precision in nuclear physics, Powell was proceeding against all received opinion. This is made clear in A. M. Tyndall's memoir which is reproduced as an Appendix.

Powell's outstanding contribution to scientific knowledge was the discovery of the pion in 1947. This discovery finally resolved a great mystery and apparent contradiction surrounding the cosmic ray mesons and nuclear forces. Its impact on nuclear physics was so tremendous that it is worth recalling briefly some of the history preceding this discovery and the vital part Powell played in it.

Many years before, in 1935, Yukawa had postulated that there should exist characteristic quanta associated with the nuclear field, analogous to gamma-ray quanta of the electromagnetic field. Yukawa predicted that these quanta should have a Compton wavelength, $h/2\pi mc$, equal to the range of nuclear forces ($\sim 10^{-13}$ cm) and thus a mass, m, of order 200 times the electron mass. These speculations were apparently confirmed with the discovery in cosmic rays of mesons, or particles of mass intermediate between that of the electron and proton, by Anderson & Neddermeyer, and by Street & Stevenson, between 1936 and 1938. These mesons appeared as penetrating particles in cloud chambers, and eventually one or two examples were found which came to rest in the gas and underwent radioactive decay into an electron and neutral particles. It was soon shown, by delayed coincidence experiments with counters, that the mean lifetime of such mesons was about 2×10^{-6} s, a figure which was also roughly in accord with the expectations of Yukawa.

The Yukawa quanta were however expected to undergo strong interaction with atomic nuclei, and a disquieting feature of the early observations was that, although numerous examples were known where mesons had traversed metal plates in cloud chambers, no example had ever been observed of their interaction 'in flight'. The situation was finally clinched by Conversi, Pancini & Piccioni, in some beautiful experiments in Rome just after the end of World War II. Using a system of counters and an electromagnet, they were able to observe the separate fates of negatively and positively charged mesons coming to rest in various materials. In particular, they observed that negative mesons stopping in carbon always, or nearly always, underwent decay. Several calculations had shown that slow negative mesons should be captured by an atom into Bohr-type orbits of high quantum number, and thence rapidly cascade down to the lowest level, in which they should spend an appreciable fraction of the time inside nuclear matter. For the case of carbon, such mesons would travel in nuclear matter, in a mean lifetime, a distance of some 10^{12} times the internucleon separation. So, these mesons must have only a weak interaction, and could not be the nuclear quanta of Yukawa.

In 1947, Marshak & Bethe had pointed to a way out of this difficulty by proposing that there might be other types of meson; a heavy meson, to be identified with the Yukawa quantum, produced copiously in energetic nuclear collisions, which decayed rapidly into a lighter meson which itself had no strong interaction. Shortly afterwards, Powell and his group obtained photographs of the decay of one type of meson, which they called the pion, into a lighter particle, called the muon. They showed that the pions were indeed produced copiously in nuclear interactions in cosmic rays, and that it was therefore the non-interacting muons which had been observed in the earlier experiments.

The impact of the discovery of the pion on the scientific world in general and on physics in particular was profound. It stimulated the building of a new generation of accelerators with which to probe the newly-discovered domain of sub-nuclear physics and to uncover the richness of completely new and unexpected phenomena. In a very real way, Powell can be said to have been the father of particle physics.

It is of interest to recall some of the detailed circumstances which led Powell to the discovery of the pion. This discovery followed from a truly remarkable combination of the right man pushing exactly the right experimental technique at the right time. As has already been stated, Powell had for long been the chief protagonist of the photographic method, during a period when it had fallen into some disrepute and was considered by many to be incapable of reliable and reproducible precision measurements. Powell was undeterred by the majority opinion on the matter and was convinced that, if one could achieve a modest increase in sensitivity and obtain more reproducible emulsions, one would have a technique which would almost certainly yield new and unexpected nuclear phenomena, since it allowed the investigation of processes taking place on a scale of distance and time not attainable by any other method then available. To this end, Powell played a prominent part in a small panel set up by the then Ministry of Supply just after the end of World War II. This panel, chaired by Professor J. S. Rotblat, had the task of advising the photographic firms of Ilford and Kodak Ltd, who were under contract to the Ministry of Supply to produce emulsions for nuclear research. Within a few months, the chemists at Ilford (C. Waller in particular) had produced a series of 'concentrated' emulsions, with a halide/gelatine volume ratio as high as 1:1, in 50 μm thickness. The most sensitive of these, the C2 and B2 types, were able to record particles of ionization down to about six times that of a relativistic particle of unit charge. Shortly thereafter (1947) it was shown simultaneously by Occhialini & Powell in Bristol and by Perkins in London that these emulsions could record the tracks of cosmic ray mesons near the end of their range. The discovery of the characteristic pi-mu decay followed soon afterwards. Eventually, in 1948, the Kodak Laboratories first produced emulsions which were sensitive to singly-charged particles of minimum ionization. The chemists were also able successfully to produce emulsions of even greater thickness, up to half or one mm; this development was crucial to the recording of complex events relying for their identification on bringing to rest energetic particles by ionization loss inside the emulsion layer.

The above advances in the production of sensitive photographic plates would have been of little avail if they had not been paralleled by other innovations in technique and organization which had been put in hand simultaneously by Powell. The first of those related to the exposure of the emulsions to the cosmic-ray beam. The first experiments had been carried out on mountains—at the Jungfraujoch and the Pic du Midi—but, at an early stage, Powell realized that, in order to obtain a reasonable number of events and investigate in detail the pion production process itself, it was essential to transport the emulsions to the stratosphere, where they would be exposed to the primary cosmic ray protons. He therefore embarked on a programme of balloon flights. At first these were made using strings of Meteorological Office rubber balloons. These were not really suitable for long flights at high altitude, and they were replaced by open-ended plastic balloons filled with hydrogen. These were manufactured in Powell's laboratory at Bristol by heat-sealing sheets of commercial Polythene, and were launched at the crack of dawn from such favourite spots as the university sports field and Savernake Forest. Powell's car covered many miles chasing such balloons over the countryside. Powell and his 'balloonatics' excited curiosity on the part of the populace at large, flying saucer stories in the press and the intense wrath of British Rail; on one occasion a balloon came down in the path of an express on the Bristol-Bath line. Questions were asked in the House, and it soon became apparent that the days of balloon-flying in England were numbered. This had what later turned out to be a providential effect, in removing the site of balloon operations to the Mediterranean area.

The chemical processing of the thick photographic emulsions used in these researches posed special problems (as had been recognized by Kinoshita as early as 1915) in securing a uniform degree of development of the latent image with depth in the layer. These were elegantly solved in 1948 by the introduction of the temperature cycle method by Dilworth & Occhialini. The ultimate step in the perfection of the photographic method took place in 1952, when Powell published a paper describing how emulsion layers could be stripped off the glass backing, assembled like a pack of cards for exposure, and later re-mounted on glass plates for processing. This procedure, which Powell later found had been suggested originally by Kinoshita & Ikeuti some forty years before, was of fundamental importance in achieving the realization of very large volumes of continuously sensitive material. Without it, the identification of the decay modes of the K-mesons and hyperons, the precise measurement of their mass and the study of their interaction characteristics, would have been very difficult. In turn, these observations had an important bearing on the formulation of the hypotheses of associated production of kaons and hyperons, and the introduction of the strangeness quantum number, by Pais, Nishijima & Gell-Mann.

Besides the names of new particles, like the 'pion' and the 'muon', Powell was responsible for the introduction of another phrase now common in high energy physics—'scanning girl'. A vital innovation necessary for the successful prosecution of the researches with the emulsion method, was the creation of teams of girls to perform the tedious examination of the emulsions by means of high-power microscopes, for events of interest. It is not clear how the idea first originated, but Powell soon convinced everyone that it was possible to train young women, with no formal knowledge of physics, to perform this exacting work with expertise and meticulous accuracy. The enthusiasm with which these girls searched out new and unexpected events was certainly no less than that of the physicists who measured and interpreted them. The employment of teams of scanners soon became commonplace, and was later extended in numerous laboratories to the more modern techniques of the bubble chamber and spark chamber.

Characteristically enough, although the success of his researches depended not only on ideas but on the detailed organization of the several different aspects of technique and method, Powell was always able to give the impression that his work was simple and unsophisticated. This probably cannot be better illustrated than by recalling a lecture he gave at an informal conference in Bristol in 1948. He was discussing the measurement of the lifetime of the charged pion in an experiment at the Jungfraujoch. In closing, he told his audience that he would like to show them the most vital part of the apparatus employed. He reached under the lecture bench, and, with a flourish, produced an empty cocoa tin!

There is not space here to catalogue all the contributions which Powell and his group made with the photographic method to the field of high energy physics, and we can mention only the most outstanding work. First and most important was the discovery of the pion, described in a series of letters and articles in *Nature* in Powell's own style of impeccable English. Noteworthy among the later papers was one in 1950 by Carlson, Hooper & King, on the analysis of electron-pairs in balloon-borne emulsions, presenting evidence for the production, in energetic nuclear collisions, of a neutral pion decaying to two γ-rays. This paper showed that positive, negative and neutral pions were produced with roughly equal frequency, and established a limit for the neutral pion lifetime of $<10^{-14}$ s. These experiments were in fact simultaneous with those carried out at the Berkeley synchrocylotron, using a pair spectrometer to analyse the γ-rays emitted when artificially-produced negative pions were brought to rest in hydrogen. The richness and variety of the cosmic ray work at Bristol was demonstrated with the observation of the decay of charged heavy mesons (now known as kaons) in a variety of modes, by Fowler, Menon, O'Ceallaigh and others; the determination of the charge spectrum of the primary cosmic ray nuclei by Dainton, Fowler & Kent; and the first observation of direct pair creation by energetic electrons (tridents) by Hooper & King. At the 1953 Rochester Conference, Leprince-Ringuet, of the École Polytechnique, paid a unique tribute to this record. Describing the new and exciting events found in cloud chambers and emulsions during the previous few months, he explained that, in Europe, 'Bristol est le soleil'.

An important development in the research which Powell directed took place in 1952, following a discussion at a Conference on Heavy Mesons at Bristol in December 1951. This was the decision, already noted, to remove the site of balloon-flying operations to the Mediterranean. This move was rendered necessary not only by the more stringent controls of air-traffic over England, but also by the need to obtain better and more stable weather conditions, allowing longer flights and the recovery at sea of the emulsion stacks and other equipment when they came down by parachute. These flights were made from Sardinia, from the Po Valley, and from Southern Italy, and in appropriate cases there was welcome support from the Italian Navy. Powell made many films of the launching and recovery of these balloons, and delighted to recount hilarious stories of the frequent incidents between the naval authorities and the men of the local fishing fleets, who, if they found the scientific equipment floating in the sea, were inclined to regard it as treasure trove.

Powell realized that, to organize these distant expeditions on a useful scale, international cooperation of many institutions was necessary. First of all, he arranged that the manufacture of the plastic balloons, under the direction of H. Heitler, should be duplicated at the University of Padua. The cooperation of the Italian Navy and Air Force in tracking and recovery has already been mentioned. Ten universities were involved in the 1952 flights, and even more in the following year. The list is impressive: Bristol, Dublin, Bern, Caen, Catania, Copenhagen, École Normale Supérieure and École Polytechnique, Paris; Oslo, Sydney, Trondheim, Uppsala, Warsaw, Brussels, Genoa, Lund, Rome, Padua, Milan, Turin, Göttingen and London. Each university group sent at least one representative to assist in the launching of some thirty separate balloon flights, and each group was responsible for the preparation of different items of the varied balloon-filling, tracking and recovery equipment.

The very large volumes of emulsion carried in these Italian flights were too great to analyse in one laboratory, and they were divided between the many participating universities. The main purpose behind one of the most important of these stacks—the so-called G-stack—was a systematic analysis of the various decay modes of charged K-mesons. This entailed following tracks of secondary particles over great distances through as many as a hundred emulsion sheets, which might be located in two or three widely separated laboratories. Many thought a cooperative venture on such a scale would not work. However, it did work, and Powell, who had first conceived this project, was proved right. The experience with these emulsion experiments therefore first demonstrated that large-scale collaborations in high-energy physics between many groups were feasible; indeed they have now become commonplace, in both this field and others in physics.

An extremely important aspect of the early European collaboration with the emulsion technique was that it enabled small and rather isolated university groups, with very limited financial resources, to take an active part in discoveries in a new and exciting field of research. As an example, the minimum 'share' required for a group to partake in the 1953 expedition was £1000. Powell saw more clearly than anyone the need for, and the advantages of, such cooperation, and his experiences in making them possible were of inestimable value in the later setting-up and running of the CERN organization, in which he played such a large part.

Following on the Nobel Prize award in 1950, Powell was brought on to the wider, and more political, international stage of science. A wide acquaintance with scientists of many nations helped him in this. In his speech at the banquet in honour of Nobel Laureates he had been able to say: 'I am the fortunate representative of a group of many scientists, drawn from more than 20 nations, who have worked together in great harmony in Bristol in contributing to the development of a new tool in nuclear physics.' In assembling that remarkable international team he had followed a tradition established by Tyndall from the very first days of the laboratory in Bristol.

His political position in the years following the war was well to the left of the majority of his countrymen. Abroad, this was a help to him, in

G-Stack Collaboration
1955, Novembre
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On the Masses and Modes of Decay of Heavy Mesons Produced by Cosmic Radiation.

(G-Stack Collaboration)

J. H. Davies, D. Evans, P. E. Francois, M. W. Friedlander, R. Hillier, P. Iredale, D. Keefe, M. G. K. Menon, D. H. Perkins and C. F. Powell.

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L. Crane, R. H. W. Johnston and C. O'Ceallaigh

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F. Anderson, G. Lawlor and T. E. Nevin

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(ricevuto il 2 Ottobre 1955)

Contents. — 1. *Introduction.* – 2. *Objectives of the experiments.* 1) Modes of dècay of Heavy Mesons. 2) Importance of accurate mass measurements. 3) Significance of relative frequency of occurrence of different modes. 4) Energy spectra of secondary particles from modes $\varkappa$, τ' and K_β. 5) Extent of the Collaboration. – 3. *Experimental Results.* 1) Methods

Figure 1.—Publication of the G-Stack collaboration: 36 authors from 10 institutions, illustrating the new style of European team-work ushered in by Powell.

bringing representatives of many countries together. At home (though never explicitly an issue in his University) it was something of a hindrance. Near the end of his career, when the University of Bristol had come to regard him as one of its most stable statesmen (most especially after his strenuous, patient and eminently just contributions to dealing with the University's small share of student unrest in 1968), it seemed strange that the University had passed over its Nobel prizeman in appointing a new head of the Physics Department in 1954, waiting another ten years for the next opportunity. It can hardly be doubted that nervousness regarding his political stance influenced that decision. Powell would have known better himself. He never allowed political difference to impinge on his loyalty to a colleague, or to the University, or to colour his judgement of the quality of scientific work.

In 1954, as President of the Association of Scientific Workers, moving a motion before the Trades Union Congress which called on the Government to prepare a basis for a conference of the nuclear powers to secure abolition of atomic and nuclear weapons, and to speed up research on the development of atomic energy for peaceful purposes, Powell gave a strikingly plain and factual (and consequently shocking) exposition of the nature of these weapons, with perhaps too rosy a view of the other side of the picture (looking forward to climate control). In an address to the London Co-operative Society, on the same theme, 'The hydrogen bomb and the future of mankind', 26 February 1955, he said: 'We are in a situation of great difficulty and danger in which it is very important to create a serious and informed body of public opinion, all over the world, in favour of an early negotiated settlement between the powers. Such a body of public opinion must, if it is to be effective, embrace people with conflicting opinions on almost all other issues, but who can be united on this.' His political position from then onwards is expressed in that final sentence.

A broadcast by Bertrand Russell on 23 December 1954 was devoted to alerting the public to the implications of the hydrogen bomb. At the same time the World Federation of Scientific Workers, especially its President, F. Joliot-Curie, was pressing for a declaration on the matter by a group of eminent scientists, to be followed by a conference. Powell at the time was a Vice-President of the W.F.S.W., becoming Chairman of its Executive Council in 1955 and President in 1957. Russell was in favour of a statement signed by individual scientists of repute, without reference to any organization. The outcome was the Russell–Einstein manifesto of 9 July 1955, of which Powell was one of the eleven signatories. In its opening words this called for scientists to 'assemble in conference to appraise the perils that have arisen as a result of the development of weapons of mass destruction'. The Prime Minister of India, Jawaharlal Nehru, had also called for such a conference in 1954, and as a result of talks between him and Powell on a visit to India early in 1956, invitations were sent out for a conference to take place in New Delhi in January 1957. This was frustrated by lack of funds and by the Suez crisis. In the end, by the generosity of Mr Cyrus Eaton, the intended conference took place at Pugwash, Nova Scotia, 7 to 10 July 1957. Russell's age prevented his attendance, and Powell presided at the plenary sessions in his stead. The three principal topics: (1) The hazards arising from the use of atomic energy in peace and war, (2) problems of the control of nuclear weapons, and (3) the social responsibility of scientists, define the principal areas of the subsequent series of conferences under the various names of Pugwash, COSWA, or Pugwash-COSWA ('Conferences on Science and World Affairs'). The first conference established an executive, the Pugwash Continuing Committee, with Russell as Chairman, but frequently absent, Powell in fact, as Deputy Chairman, always taking the Chair. He was elected Chairman in 1967. He also regularly took the Chair at the final and most difficult Plenary Session of each Pugwash Conference, at which statements were agreed upon. Rotblat, the Secretary, records that 'during the whole period when Powell chaired the meetings of the Continuing Committee there has never been any need to take a vote, even though the Committee discussed very complex issues' (and, one may add, even though the Committee was, by design, composed of representatives of nations in conflict with each other). Powell's gift of keeping people with seemingly irreconcilable views working together enabled the Pugwash conferences to fulfil the valuable function of defining areas of agreed fact as a background to the distressingly slow progress of international negotiations.

Powell had established his position as a leader of European Science in directing the high altitude balloon flights in Sardinia, 1952, the Po valley, 1954, 1955, 1957, and in Southern Italy, 1961.

Powell became Chairman of the Science Policy Committee of CERN from 1961 to 1963. Victor F. Weisskopf, who was Director of CERN at the time, writes: 'As Chairman of that Committee, and later as a member of it, he was most helpful in establishing the long-range development which was accepted by the Council at the end of my period of service. You may remember that this development included the following three important items: (1) the construction of a giant bubble chamber; (2) an improvement programme to increase the usefulness and the flexibility of the proton cyclotron; and, finally (3) as the most important item the construction of intersecting storage rings. Cecil always had a long perspective in mind, and he always emphasized the fact the European physics needed a long-range programme of fundamental physics which moved toward new methods and new discoveries of an unconventional kind. This was why he was such a strong supporter of the storage rings.

'But there is much more to Cecil's contribution. The task which we had in the early '60s was the creation of a European spirit of scientific daring and of venture, a spirit of scientific leadership of the type that Europe had had in the '20s. It was the purpose of CERN to provide this spirit and—most importantly—to provide it on an international or, let me say, supra-national basis. I believe we succeeded in doing so but it would have been impossible to have done so without Cecil's vision; his unquestionable

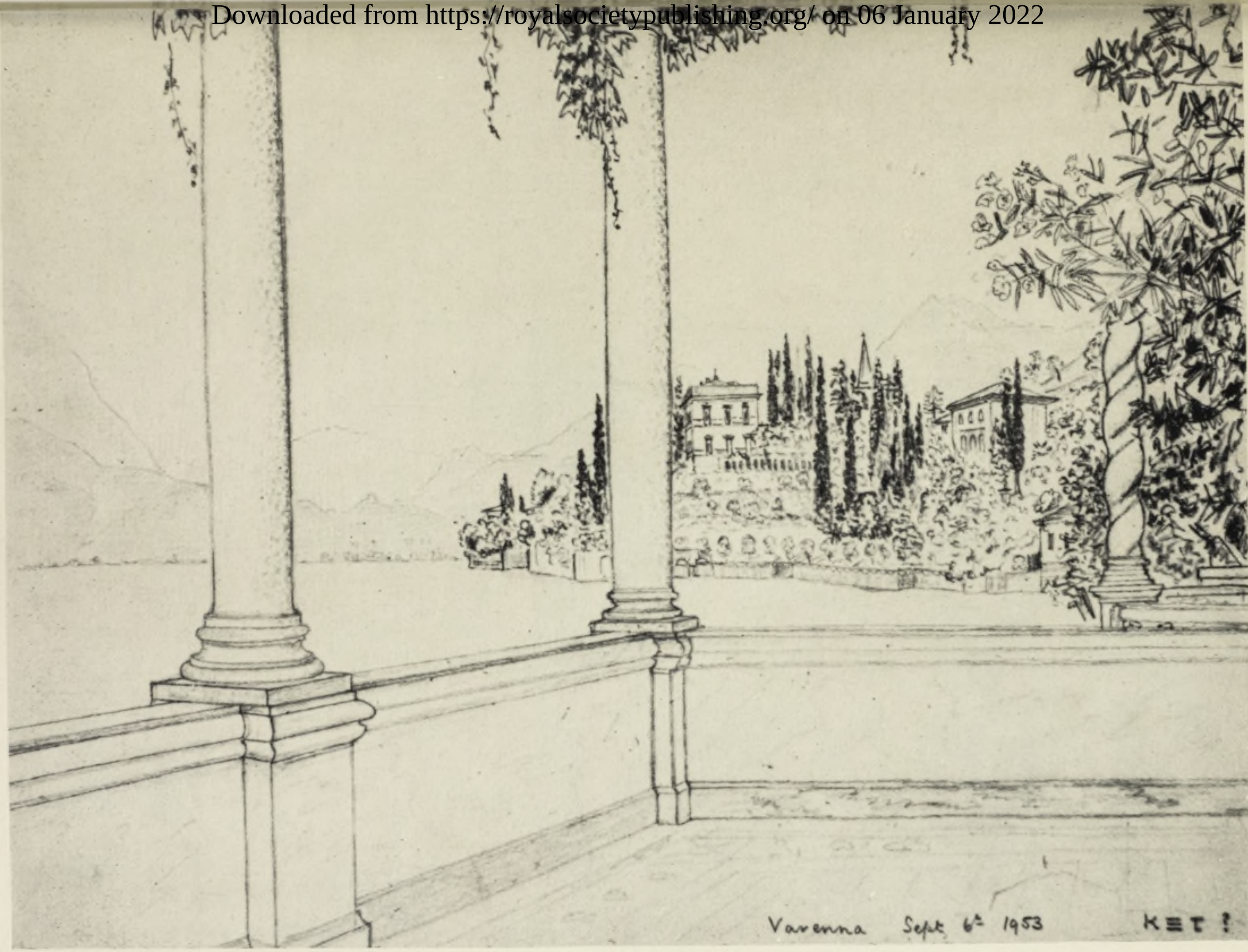

FIGURE 2.—View from the Villa Monastero, Varenna, Lake Como, sketched by C. F. Powell during the International Course on Cosmic Rays held there by the Italian Physical Society, 19 August to 12 September 1953. Powell gave the opening talk on 'Hyperons and heavy mesons' at this course, and '$K \equiv \tau$?' was a crucial question at the time.

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leadership both in conceiving of these aims and in formulating them. We all know that there is nobody, and there will be nobody for a long time, who is able to formulate the aims of science and the role of science in society, and the role of science in bringing nations together, such as was Cecil Powell. One should not underestimate the impact of a good formulation of aims. It was essential and we could not have done this without him.'

It is well said: but we have left out the poetry, and the fun, and his chuckle.

We are much indebted for information supplied by Professor E. H. S. Burhop, F.R.S., Professor M. Delbrück, Professor H. Fröhlich, F.R.S., Professor W. H. Heitler, F.R.S., Mr R. L. Mercer, Professor J. Rotblat, C.B.E., and Professor V. Weisskopf, for which we record our grateful thanks.

The photograph is by Derek Balmer.

F. C. Frank
D. H. Perkins

APPENDIX

EARLY HISTORY OF C. F. POWELL'S CONTRIBUTION TO THE STUDY OF NUCLEAR PROCESSES BY THE PHOTOGRAPHIC METHOD

by Professor A. M. Tyndall, F.R.S.

(From a rough MSS. found on Tyndall's desk at his death on 28 October 1961, together with an undated covering letter to the Executive Secretary of the Royal Society reading 'It is probable that by the time an obituary of C. F. Powell is required no one will be living who watched from the beginning, as I did, the work which led to his Nobel Prize. Many months ago I offered to supply an account, which might be of interest to his biographer. Here it is'.)

Powell first came to Bristol as my research assistant in 1928, and I quickly learned the value of his experimental ingenuity and manipulative skill. After a few years in that post he struck out in new fields of his own, and about 1935 he took part in departmental discussions on the possibility that our laboratory might enter the nuclear field by acquiring some form of generator.

H. W. B. Skinner and W. R. Harper independently made some preliminary tests on ideas of their own, but Powell set out with departmental help to build a Cockcroft generator on the top floor of the Wills Laboratory tower (the Fourth Floor as it became to be known).

While the generator was being built, his attention was drawn to a letter in *Nature* by Blau & Wambacher (1937) giving an illustration of a track of a cosmic ray which they found in an emulsion exposed to radiation in its light-tight wrapping, if I remember rightly, on the Jungfrau. It happened that when the generator was completed, the Wilson Chamber that he was also constructing to use with it was not ready. So, while he waited, he tested his proton beam by placing tangentially in it a photographic plate taken from a box for making lantern slides, bought across the counter of a dealer. He was so pleased with the result that he immediately set out to test its quantitative value by studying reactions which had already been investigated by counters, such as the distribution in range of protons produced from a boron target placed in a beam of deuterons. By adjusting the range scale and origin of the graphs from the two methods so that the main peak in each coincided, exceedingly satisfactory agreement was reached.

Now all through this phase of the work he was completely unaware of the long history of literature of tracks of particles in photographic emulsion—as, indeed, I was. Whereas English nuclear physicists of longer experience had already accepted as final the results of Taylor, published in 1935 with all the authority of the Cavendish Laboratory, that the photographic method was not suitable for quantitative work. Indeed, in two laboratories I was told that Powell was wasting his time for this reason. But by then Powell was completely convinced that they were wrong.

The reasons that he gave for this optimism convinced me:

(1) That the best microscopic technique was essential. It was rare for a Physics Department to possess a really good microscope. He had never seen one in the Cavendish;

(2) There was no evidence in the literature that proper attention had been paid to precision geometry or steps taken to control the processing of the photographic plate so that it was uniform throughout the depth of the emulsion; and so that distortion due to shrinkage was reduced to a minimum when the free silver was removed;

(3) The photographic plates had been designed for other purposes. Approach should therefore be made to the makers to induce them to produce special emulsions richer in silver content and thicker in depth.

All these points were attended to in Bristol and the Wilson Chamber in course of construction was never even completed. The best Leitz microscope was bought, slow and temperature controlled development was introduced and Messrs Ilford were approached about special plates. But the first trials on new emulsions shortly before the 1939 war were not successful, and Powell had to wait until 1945 before Ilford's could return to work on the subject and produce the 'Nuclear Research' emulsions with which the π-meson was discovered.

With these new emulsions available after the war, the 'Fourth Floor' of the Wills Laboratory became an exciting place in which to work. Additional high grade binocular microscopes were purchased; a group of women 'scanners' were engaged, most of them with no scientific knowledge, and just trained to scan the plates and record everything they saw, together with the position of the event. They were soon to be referred to as 'Cecil's beauty chorus'.

In plates exposed to cosmic rays on high mountains, thousands of nuclear events were there to study. Rarely during an almost daily visit did I fail to see something interesting; first a proton proton collision or a proton heavy atom recoil through very large angle; or a disintegration star of novel type; then a meson track (since named μ-meson) as at least a daily event, at a time when not more than half-a-dozen had been detected in the world by other means.

Occhialini, who joined the Laboratory in 1945, added to the ease of viewing by setting up a projection microscope in which the stage traversed slowly horizontally and oscillated also vertically to follow tracks which dipped into the emulsion. Occhialini, enthusiastic, vivid and picturesque, called this instrument a Telepanto: 'Tele, I see; Panto, everything.' Here visitors could sit and run through the whole gamut of nuclear events in the course of a quarter of an hour.

Up to then, however, the method had not yielded any phenomenon which was fundamentally new, and outside of the laboratory physicists seemed inclined to dwell upon its disadvantages—for instance, the inability to use a magnetic field—rather than the fact that each technique had a field of its own in which it was superior.

Then came the climax and excitement of the discovery of the π-meson and its verification a few days later. This firmly established the method, and visitors swarmed to the Laboratory to learn more about it.

Finally the production of electron sensitive plates first by Kodak and then by Ilford disclosed the full reaction $\pi-\mu-e$ and led the way to the discovery of other fundamental particles, and today (1961) to the successful search for cosmic particles of enormous energy.

Future historians in assessing Powell's contribution to physics will probably think of him primarily as the discoverer of the π-meson: whereas I would lay the emphasis on his experimental insight and manipulative skill in creating the emulsion technique which others had failed to do with originally the same material. It was he who overcame the difficulties in making it a quantitative tool. Once this was done, who it was that discovered something new was purely a matter of chance. Indeed many of the rare events, including the π-meson itself, were first observed by a scanner with no scientific training reporting a track of appearance different from anything she had previously seen.

But should Powell have read the literature before embarking on this work? And, if so, would he ever have started it?

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