

Rutherford with students. "... one would receive occasionally ... a more or less unannounced visit from Rutherford at one's own working bench. He ... would seat himself on a laboratory stool and put one through a quite searching examination: 'What, precisely, are you doing?' " (Photo by Paul Harteck.)



Recollections of Rutherford and the Cavendish

During his years as head of the laboratory, Rutherford came to symbolize its greatness, and the two remain intertwined in the memories of a former Cambridge student.

Samuel Devons

A cat may look at a king, and in Cambridge in the 1930's there were many kings—eminent, renowned or celebrated in the sphere of learning and yet in Cambridge quite homely figures. Cambridge was their home, as it was ours, the lowly undergraduates. There was Hardy the "pure" mathematician, Housman the scholar-poet, Keynes the economist, Gowland-Hopkins the "father" of biochemistry, Wittgenstein the iconoclast of philosophy, Eddington, Dirac and many others. We might only occasionally see, and seldom hear, these illustrious individuals, but their presence gave us a sense of living in history. Education at Cambridge seemed, then, to be more a matter of inspiration by example than instruction by precept.

To a student of science, and particularly physics, it was Ernest Rutherford who symbolized and exemplified this living greatness, and in the recollection of my undergraduate days it is impossible to separate Rutherford, Cambridge and the Cavendish Laboratory. They all seem, at a distance of nearly 40 years, so much a part of each other.

Scientists in many parts of the world commonly associate Rutherford and his work with the Cavendish Laboratory, and it is not hard to see why. Rutherford returned to Cambridge in 1919 and the last 18 years of his life were spent as Cavendish Professor (the fourth, suc-

ceeding J. J. Thomson, Rayleigh, and Clerk Maxwell). Cambridge, and the Cavendish Laboratory especially, was an established, renowned center of science. In the early 1930's its lustrous reputation was as high as ever. These years were indeed the "golden age" of the Cavendish, and they were Rutherford's golden years.

To be sure one could read of his great achievements before he came to Cambridge, in the 20 years of immense activity first at McGill in Montreal and then at Manchester (see box): Most of his own great personal contributions to science date from this period. Rutherford was already a great figure in science when he returned in 1919 to Cambridge and to the Cavendish with its long tradition as a center of science. Rutherford the individual and the Cavendish the institution became fused in one, and together radiated a brilliance rarely matched. At Montreal it had been Rutherford himself, at Manchester Rutherford and his school, and now at Cambridge it was Rutherford, the impersonation of the great Cavendish tradition and part of its glory.

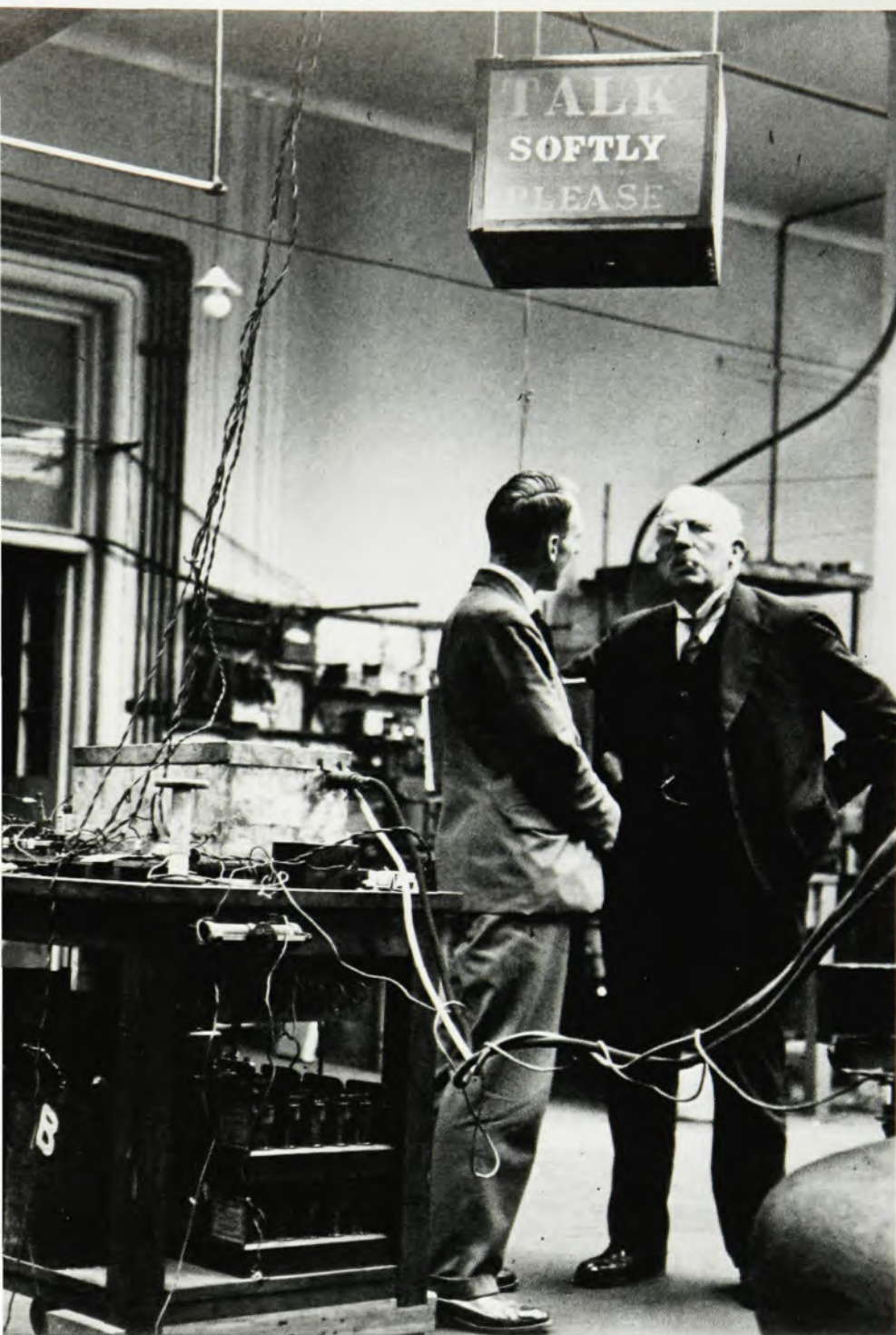
Undergraduate students at Cambridge at that time did not, as I recall, exercise themselves consciously with such broad historical perspectives about what was going on under their very eyes: These things were felt in a more immediate and homely sense. Rutherford was not only one of Cambridge's illustrious Professors; in physics, in the Cavendish, he was *the* Professor. In England at that time it was quite nor-

mal for a professor of science to regard his laboratory as a sort of personal domain, over which he exercised undisputed authority. The Cavendish was Rutherford's domain, his sphere of influence. But one never felt that this stemmed from any formal title; his influence there seemed a wholly natural phenomenon. Benevolent guidance, leadership and intellectual authority flowed from him, and admiration, respect, trust and loyalty were returned. One would no more question his influence on those around him than one would that of the sun on the satellite planets. Rutherford, the Cavendish Professor, was the center of influence and the center of light and warmth and life. It was the natural order of things. Young undergraduate students were way out on the periphery of this constellation, but we could bask in the sunlight just the same.

Lectures as inspiration

In Cambridge one "read" for a degree. This was more than a manner of speech: Books (and laboratories as well as libraries) were the main source of knowledge. There were also lectures. Some lectures, for the less "intellectual," were substitutes for reading, but there were others that were as much sources of inspiration as of knowledge. These were the lectures of the great, and one attended them to see and hear the great men. Rutherford lectured (officially) three times a week—Monday, Wednesday and Friday at 12—and the title of his lectures was "The

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With J. A. Ratcliffe in 1935. The illuminated sign warned Cavendish people not to disturb the newly developed electron counting tubes. (Photo by C. E. Wynn-Williams.)

Constitution of Matter"! They were intended primarily for students who were specializing in physics, but they were open to all.

Rutherford's lectures were held in the old "Maxwell" lecture room, the main lecture theatre of the Cavendish. It comprised the usual tiered rows of wooden benches. There was an audience of about 40 students, who half filled the theatre. Although formal in the Cambridge pattern, in style and subject matter they were highly personal; a quasihistorical, or rather biographical, account of the development of "atomic" physics of the past few decades. It was no easy matter to separate the development of physics from Rutherford's own life's work, and Rutherford did not try.

He always had a few loose pages of notes, which he extracted from the inside pocket of his coat at the beginning of each lecture, but he consulted them only occasionally. There were frequent experimental demonstrations, always a copious stream of slides, diagrams, photographs—experimental records—which provided the basic thread of continuity. There was no doubt that we were listening to a great man relating an epic story, rather like the story of some great scientific expedition as told by its leader. We were being told not so much what Rutherford (or anyone else) thought about this or that, but rather how Nature did its work and how this had been discovered. It was, as Rutherford was so fond of emphasizing, "the facts" that were important. But when one is spokesman for "Nature," there is no need to be overly didactic, and no cause to be unduly modest. And in Rutherford's case—as has been so often remarked—he had so little to be modest about!

Students in 1930 were not particularly docile (intellectually at least), so that although, as young neophytes in the presence of the most eminent authority and a benevolently and vigorously magisterial personality, we were always attentive, respectful and at times a little awestruck, yet we were not wholly and uncritically receptive. To us, Rutherford, then in his sixties, appeared as a patriarchal figure, somewhat archaic, vaguely Victorian in dress and manner—and youth is always apt to feel indulgent in paying its respects to age and experience.

There was a paradox in this combination of an elderly conservative gentleman of the "old school" and the proponent, nay the discoverer, of the latest word in this most modern field of knowledge: atomic and subatomic physics. I remember puzzling over this paradox with fellow students, but we made little headway in resolving it. In any event, in Cambridge, where time is (or was) measured in centuries as well as terms and years, the contrast

between ancient and modern was everywhere and was accepted as the natural order of things. The "old" Cavendish Laboratory itself, with its cobbled courtyard and its archways and massive oak gates, locked and unlocked religiously twice a day with much clanking of iron keys in locks—buildings of ill defined but unquestionably archaic style—might almost have been a deliberate camouflage for the science of the future that was being vigorously created inside. It was all part of the scene: Cambridge, the Cavendish and Rutherford alike; traditional forms and radical ideas; an enduring, time-beaten outer shell containing and protecting the vital, quickening activity within.

Rutherford's lectures were lively, up-to-date, and regularly well attended, part of the mainstream of the institutional activity of the Cavendish. There were other, seemingly timeless, institutions where tradition and function were less readily distinguishable. At 4:45 pm ("tea-time" for most undergraduates) in one of the smaller, dimly lit recesses of the Cavendish, "special" lectures were delivered, to which "the attention of the students was drawn." These were once-a-week institutions, delivered to small, often rapidly diminishing audiences; a sort of "extra-curricular" activity (although to imply any formal curriculum of lectures or studies at Cambridge would hardly be accurate). J. J. Thomson on the "Conduction of Electricity in Gases" was the most venerable of these. One could also hear C. T. R. Wilson—on the "Condensation of Vapors and Electricity," or F. W. Aston—on "Isotopes." All this seems, now, as far removed from current notions of "courses of instruction" and "curricula" as the style of Plato's Academy. But it also appears, in retrospect, as an ineluctable part of Cambridge and the Cavendish in the 1930's.

One focal point of the intellectual activity of the Cavendish was the weekly Wednesday afternoon meeting, in the Maxwell Lecture Room, of the Cavendish Physical Society, over which Rutherford presided. This occasion, when the lecture was usually given by a distinguished visitor, was also one of the few on which other Cambridge luminaries—R. H. Fowler, P. A. M. Dirac, G. I. Taylor, Arthur Eddington, professors of "mathematics" and astronomy—and some of their disciples would assemble together with the experimental physicists of the Cavendish.

Rutherford was no passive chairman of these weekly meetings; his vigorous amiable personality was an essential and memorable feature. After the invariable preliminary tea, Rutherford would introduce the speaker and often his subject, and would be seated near him, in front of the audience, through-

Rutherford's career

Although we associate Rutherford with the Cavendish and Cambridge, he was, of course, associated with other laboratories and other cities. Here is a brief summary of his life in physics.

1871	Born on 30 August in Brightwater, New Zealand.
1889–1895	Student at Canterbury College, Christchurch, N. Z.
1895–1898	Research student, Cavendish Laboratory, Cambridge University.
1898–1907	Professor of physics, McGill University, Montreal, Canada.
1902–1903	Working with Frederick Soddy, Rutherford explained natural radioactivity as the spontaneous transmutation of one element into another.
1907–1919	Longworthy Professor of Physics and Director of the Physical Laboratory at the University of Manchester. Identified the alpha particle and detected (with Hans Geiger) individual atomic particles.
1908	Nobel Prize in Chemistry
1911	Proposed the nuclear atom.
1919	Achieved deliberate atomic transmutation by bombarding nitrogen with alpha particles.
1919–1937	Cavendish Professor of Experimental Physics and Director of the Cavendish Laboratory, Cambridge University.
1923	President of the British Association for the Advancement of Science
1925–1930	President of the Royal Society of London
1937	Died, on 19 October, in Cambridge.

out the lecture. To the sprinkling of undergraduates who sat at the back of the lecture room (the meeting was primarily for the faculty, visiting scientists and research students, but senior undergraduates—or for that matter anyone else—were not unwelcome), Rutherford would often appear to have dozed off a bit during the course of the lecture, but at its conclusion he certainly sprang to life, with appraisal, appreciation, criticism and questions. And after offering his own comments he would, if necessary, try to stir up some questions or reactions from the senior members of the audience in the front row.

"What are the facts?"

At these meetings, as in his own lectures, Rutherford's own attitude to physical problems was always unambiguously expressed. There was always the demand for the "objective" and, if possible, simple reality. Almost invariably there was the question "What are the facts?" Facts were to be respected and treated quite differently from theory, which was, in a sense, "opinion." "As our friends the theoreticians might say. . ." was a familiar term of reference—especially to theories (and theoreticians) whose formal logical reasoning might be somewhat unfamiliar but whose achievements he respected. There was an extraordinarily transparent honesty and a deceptive simplicity about the clear distinction between fact and theory (opinion). Rutherford would never allow himself to be deflected from his aim of seeking out and identifying the essential, the "simple" facts, certainly not by what he might have regarded as a smokescreen of argument

or theorizing. He was impatiently hostile to any attempt to obscure or to conceal or to complicate unnecessarily. And no matter how prestigious its proponent, Rutherford would denounce sophistry point blank if he felt it were deliberate. Naturally we students, at the back, would relish these rare opportunities to see some distinguished worthy of science being publicly "debunked" by "our" Rutherford in this way.

And all the time we were mildly perplexed. Could physics (or science) really be so simple as Rutherford disarmingly made it to be? Why was it all so simple *afterwards* and so complicated or subtle *before*? Was this complete, transcendental honesty enough? Utter honesty could also be simply lack of imagination, even stupidity, which did not appear to be a rare or difficult attitude to cultivate! I'm sure we grossly underestimated the difficulty of honestly observing, in experimental science, how things are rather than how one thought they should be. But it was the remarkable combination of a most powerful imagination counterbalanced by a sense of utter honesty that was most impressive and mystifying.

Rutherford's emphasis on simplicity is proverbial. ("I am a simple man myself. . .") Simple ideas and simple apparatus, but powerful, conclusive results; simple, unpretentious appearances, but striking inferences; these were the Cavendish trademarks. It was a tradition older than the Cavendish Laboratory, at least as old as Henry Cavendish, the famous, eccentric 18th-century scientist whose name the Laboratory honors.

Function rather than form was the keynote, and if, in the last decade of Rutherford's life, new functions were demanding new forms—cyclotrons, high-voltage accelerators, not to mention the cryogenic equipment in Peter Kapitza's modern new laboratory right inside the sanctum of the Cavendish—Rutherford might have been reluctant, but he was not slow to adopt them. At the time I was admitted into the charmed circle of those who did research at the Cavendish, the Laboratory was right in the middle of this transition phase. But as a novice one had first to imbibe the traditions of the past. In any event technique was a secondary, subordinate matter: Spirit and purpose were primary and these had to be tested before admission.

Interview with the Professor

In his final (third) undergraduate year, a student at Cambridge could indicate his aspiration to stay on to do research, and would then normally become a candidate for one of the handful (some four or five in a class of 40 students) of government scholarships for which he could be recommended by the Professor (Rutherford). The major determinant in this competition was performance in the final-degree examination. After surmounting this obstacle there was a period of a few weeks of suspense before one's fate was determined, and during this period one was summoned for an interview by the Professor and his senior staff.

The interview was held in Rutherford's office (then the *only* office in Cavendish). Rutherford sat at the head of a long table flanked on either side by the leading figures of the Laboratory: C. T. R. Wilson, J. Chadwick, C. J. Ellis, J. D. Cockcroft, M. L. Oliphant, P. I. Dee, J. Ratcliffe and others. Individually they were all familiar to me (I had attended lectures by Wilson, Cockcroft and Dee; Oliphant had frequently taken over from Rutherford in the later stages of his lecture course; Ellis had been my college "supervisor;" Dee was director of the undergraduate laboratory). But collectively they were quite formidable, and Rutherford himself, a familiar figure always, seemed even larger than life. I had never been in the Professor's office before, nor in fact as an undergraduate had I ever "met" Rutherford. I am sure it was not so intended, but the atmosphere felt inquisitorial. "Do you really *want* to do research? Why? Is there a particular problem in physics you would like to resolve?" and so on.

The questions were not unexpected, so the answers were not unrehearsed. During the year I had, with stimulation and guidance from Ellis, studied (and written about) a few topics in physics rather intensively. One of these was the

nature of cosmic radiation, and I thought it would be safe to venture this as a possible research topic. I hazarded the suggestion that there was a great lack of knowledge as to what cosmic rays *really* were, how they produced the observed effects: the much studied ionization, its variation, fluctuations, large bursts, and so on. (I had little idea of how one would go about repairing the lack of knowledge!) This suggestion was received by Rutherford with a friendly grunt of acknowledgment and some mild curiosity as to who had "put me up to" this notion. He glanced round the table to see if anyone acknowledged responsibility, and Ellis offered a word of explanation. Rutherford did not appear much impressed by cosmic-ray research, or with my formulation of the problem or the subject. I felt I had made a mistake in "choosing" this subject at all. But I was dismissed with what I interpreted to be encouraging signs, and as I recall, with a remark from Rutherford such as, "Well, I suppose we'll find some problem for you if (when?) the occasion arises." A month or so later I was admitted as a research student.

There were in those days other initiation rites. New students were often anxious to start research right away, in the middle of the summer, and stayed around in Cambridge hopefully. Something had to be done with them. An attic room full of discarded or temporarily unwanted apparatus, mostly junk, was nicknamed the "nursery" and used as a breaking-in ground. I recall spending five or six weeks there, dusty and directly under the roof, which was hot even in an English summer, vaguely learning some experimental "techniques" and trying to construct or repair or improve a sort of string-electrometer ionization-chamber arrangement. For apparatus we scrounged anything that was lying around: I recall best a number of hand-cranked suction (vacuum!) pumps that looked (in the mind's eye) as if they might have previously been used by Otto von Guericke in Magdeburg.

Later in the year I (together with a fellow student, G. J. Neary) was given a problem by Ellis to examine the radiations (beta and gamma) from radium C'. The experience gained in the nursery had not been wholly inappropriate. As a research novice, either one had to make one's own apparatus, using hand (or foot-operated) tools and bits of metal and wood that had been used and reused by generations of research students, or one might inherit and make do with the residue of some earlier research. And, of course, one was expected to be able to do one's own glass blowing.

I have often wondered, in subsequent years, why Rutherford himself or his

associates did not display more active interest in cosmic rays. No doubt this recurrent question had been initially prompted by his reaction to my own youthful suggestions. It was not that cosmic rays were uninteresting, but perhaps they were too complex and not something that could be brought under control in the laboratory. Possibly they might require elaborate equipment and very lengthy observations. Also—as some notable contemporaneous examples indicated, and as Rutherford had commented—interpretation could prove a trap for the unwary. Rutherford was certainly alive to the possibilities, as his remarks to the London International Conference in 1934 show:

"It will be interesting to examine the effects of different projectiles at much higher energies. The swiftest particles known to accompany the cosmic rays have energies of the order of 100 million volts. . . . some are believed to have energies even greater than 1000 million volts. . . ."

It was the doubtful feasibility of *decisive* results from cosmic-ray studies that, I believe, made him wary. That study of these rays would one day become laboratory physics would have certainly surprised him, for he went on to say:

"Information of this kind is of great value, for it is unlikely that we shall ever be able to produce particles of such energy in the laboratory."

For a laboratory of the Rutherford-Cavendish tradition this was all too true.

Working with radium

For the initial exercise in research, and especially for a more substantial piece of research that I later undertook (the resonant scattering of alpha particles), the crucial item was the Cavendish radium source. For decades this radium—there was approximately one gram of radium bromide in the Cavendish—had provided the basis for much of Rutherford's research, but now it was hardly in demand at all. Cockcroft and others were busily exploiting the high-voltage equipment he had built, and a newer, better such equipment had been ordered from Philips, in Holland; Rutherford had had built for himself a smaller equipment of some 250 kV, which he and Oliphant used; a cyclotron had already been planned and construction was about to begin; Chadwick had left Cambridge and was busy with similar preparations at Liverpool. Sealed radium-beryllium sources were still used for the new neutron investigations, but the old treasure, the 1000-millicurie source—in solution—was freely available even to a humble graduate student.

"Milking" the radon emanation from the radium source was, wisely, not entrusted to students. This operation was presided over by Mr Crowe, Ruther-



Nursery. "New students were often anxious to start research right away . . . an attic room full of discarded or temporarily unwanted apparatus, mostly junk, was nicknamed the 'nursery' and used as a breaking-in ground." (Photo courtesy Cambridge University Library.)

ford's long-time assistant, who constantly bore, in his gloved hands, a reminder of the price that could be paid for careless practice. Nevertheless about once a week the emanation would be pumped off the radium source, and I was presented with a small glass capsule, precariously sealed off by immersion in a small mercury-filled crucible, containing several hundred millicuries of radon. From this once or twice a day it was possible to prepare active deposits of radium A, B and C, which were potent sources (100 millicuries or more) of alpha particles.

Of course there were precautions to be taken, but to describe these and the whole routine of source preparations as primitive can only appear now as the extreme of understatement. There was an elaborate ritual of wearing rubber gloves, of washing and scouring the hands and changing jackets on the way in and out of the small room (the "Tower") at the top of the Laboratory where the radium was housed. Inside the radium sanctuary itself, the residual activity was so high from the "contamination" everywhere and from the residues of innumerable sources of the past that it was difficult to charge up the gold-leaf electroscope (mounted on the wall) for long enough to measure, even roughly, the strength of a newly prepared source of some 100 millicuries. But then one could always estimate the

strength of such a source by its smell!

I was strongly advised not to get "the stuff" on my skin, or in my lungs, but even stronger was the warning not to contaminate the laboratory. Many experiments (including my own) had to contend patiently with counting rates of one or two a minute: Imagine how easily they might be wrecked by even the minutest amount of contamination—even one microcurie is more than a million counts per minute, and I would be carrying around hundreds of millicuries. As long as the sources were "carefully" sealed up (in a glass test tube with rubber bung), and kept away from other people's apparatus, no one appeared to mind—or to know—that I had a 100-200 millicurie source in my pocket. Nor was I myself unduly alarmed, when shortly after a visit to the "Tower" (where I spent a couple of hours each day), I found that by simply blowing on a geiger counter, its register would rattle furiously or completely choke in the attempt to record the activity. After a day or two of radioactive abstinence my breath always returned to normal. For more than a year I enjoyed this virtual monopoly of the radium source—perhaps the first (and last) student to be so privileged.

Guidance by Rutherford

All research students were members of the Cavendish family and as such

from time to time enjoyed brief personal contact with the patriarch of the family, Rutherford himself. Theoretically we were all students of "the Professor"; in practice of course Rutherford was during this period very much preoccupied with the major transformation that the laboratory was undergoing, and also, as a leading national figure in science, he had many commitments that took him away from Cambridge. The direction of research students was in effect delegated; my research director was initially Ellis and later Norman Feather. Nevertheless, research students (or at least those with scholarships) did submit, once a year, a written report on their research, which was subsequently returned enhanced by Rutherford's signature—"Approved-Rutherford"—or some other appropriate remark. More significantly, one would receive occasionally, perhaps once or twice a year, a more-or-less unannounced visit from Rutherford at one's own working bench. He would briefly examine the apparatus and then would seat himself on a laboratory stool and put one through a quite searching examination: "What, precisely, are you doing? How? Why?" And of course this led rapidly to the request: "Now let's see what the results are."

There is no doubt, as so many with closer knowledge have stressed, that Rutherford's real interest was in the

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results of experiments and not the methods or techniques or difficulties themselves. However, on the few occasions when, as a very junior research student, I was cross examined, I recall being very much impressed by the questions and criticisms regarding both the aim and the methods of my work. In a few thrusts Rutherford's questions penetrated right through the limits of my own thinking and stimulated me to do some more. He was clearly acting in the role of teacher, not critic, and the results of his uncompromising, but not hostile, questioning were undoubtedly salutary. This was all very much in keeping with his expressed attitude towards teaching: A student should think for himself, should ask himself questions; a teacher should not so much supply answers as encourage a student to pose questions to himself.

Quite apart from the few personal contacts with Rutherford that we research students enjoyed, there was no question that we felt his powerful presence and personality around us. It was transmitted to us through Rutherford's disciples, the senior members of the Cavendish, and through Rutherford's (and the Cavendish's) long-serving laboratory steward and assistants. In the occasional advice, criticism or encouragement we got in our research work, or in scientific discussions and colloquia, it was not hard to discern Rutherford's guidance and inspiration, and often his style. And in the day-to-day business of the laboratory, Rutherford's authority was clearly heard, especially in the voice of Mr Lincoln, the chief steward, who had served the Cavendish—Rutherford, and J. J. Thomson before him—for some forty years. We would have to justify a request for a half-dozen brass screws as rigorously as if we were putting a scientific proposition to Rutherford himself. And punctually, at six o'clock each evening, the senior laboratory assistant would tour the laboratories announcing to all that it was time, gentlemen, to close. If (as happened rarely) this announcement was challenged, it was promptly reinforced with advice, on the authority, indeed the words of the Professor, that: "If one hadn't accomplished what one wished to by six o'clock, it was unlikely that one would do so thereafter. It would be better to go home and *think* about what one had done today and what one was going to do tomorrow."

There were other less businesslike, but if anything more formal, occasions to meet the Professor. Once or twice a year one would receive a written invitation to tea at Rutherford's home at Newnham Cottage, a short walking distance from the Cavendish and the colleges. This was a very proper, English, Sunday-afternoon occasion,

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over which Lady Rutherford presided. One arrived punctually and departed on cue. About half (the male half) of those present were people from the Cavendish, but it was clearly not intended by Lady Rutherford as an occasion or opportunity to discuss physics or Cavendish matters. After tea there would be a stroll around the garden, which was Lady Rutherford's particular pride. But this stroll also resulted in the party breaking up into smaller groups of twos and threes, and might provide an opportunity for a brief, relaxed, personal encounter with Rutherford and the surreptitious reestablishment of the Professor-student relationship: "Let me see, you are X aren't you? Now what are you working on? How are the results coming along?" But Lady Rutherford was not far away, and we were supposed to be admiring, or identifying, the flowers, and there was soon a recall to the sterner duties of host and guest at a Sunday tea party.

Each year, as a research student gained a little seniority in the Cavendish family, opportunities to meet Rutherford both inside and outside the laboratory increased accordingly. I had eagerly and hopefully looked forward to the day when in reply to the familiar question "What results are you getting?", I would be able to offer not only something that might justify my existence in the Cavendish but some "results" that would be of sufficient significance to merit and evoke Rutherford's interest.

But alas, suddenly and incomprehensibly in 1937 (at the end of my second year as a research student), Rutherford died. To every member of the Cavendish Laboratory, as we stood in Westminster Abbey amongst all who came to express their respect, admiration, affection and sorrow, it was a family bereavement. For the Cavendish, Rutherford, like J. J. before him, had been in person and in spirit a true father.

For a year or so after this great shock, the Cavendish was carried along with the impetus that Rutherford had imparted to it. In 1939 the era came abruptly to an end, and in 1946 (when I returned to Cavendish) the handful of "oldtimers" from the Rutherford days who had reassembled at Cavendish felt that they already belonged, in part, to a past phase of history. In 1946 it was a changed world, a different physics and a new Cavendish.

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This article is adapted from a talk given last August at the Rutherford Memorial Colloquium of the 13th International Congress of the History of Science, Moscow.

Reference

1. International Conference of Physics, London 1934, Volume 1, The Physical Society, London. □

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