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MEMORIE

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Far-Flung Physicist

Looking back at my 57 years in physics, I note that one characteristic feature is the frequent changes in place, institution or company. Some of these changes were of my own volition, others were forced by circumstances. Such frequent changes might be feared to bring the danger of interrupting and disorganizing one's ordered learning and development, but I seem, on the contrary, to have profited by them.

The first interruption of the regular routine of studies occurred when in 1925 I completed the course at my "Gymnasium" in a suburb of Berlin, where I had had the benefit of being taught mathematics by an outstanding teacher, with only indifferent teaching in physics. My school was unusual in that their year ended at Easter, instead of the conventional autumn date, and entering university in the summer semester was inconvenient. So I used the six months' gap by working as a trainee in the laboratory of a telephone factory, learning to use tools and machine tools, as well as to design automatic relay circuits (no electronics in 1925). Although this had no direct relation to my studies, I never regretted the time spent in learning something about the practical side of things.

At the University it had been my desire to study engineering, but my parents and friends advised me that I would not make a good engineer, and while I did not find their view convincing, I followed the advice of my elders, and chose the next best thing, which seemed to be physics. I did not have a very clear idea what this involved.

I was thought too young to leave home, and therefore enrolled in the University of Berlin, my home town. It was not clear whether I wanted to become an experimental or a theoretical physicist, but at the time there was a shortage

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of space in the physics laboratories, so physics students were required to take as many of their mathematics and theoretical physics lectures as possible in their first two semesters, and postpone their practical work. So I started on theoretical work, and have stuck to it ever since.

Amongst my professors was Max Planck, of whom we knew, of course, that he was very famous, but I did not know what he was famous for. His lectures were about the worst I have ever listened to. He would read verbatim from his books on theoretical physics, and if you possessed a copy of the relevant book, you could follow the text line by line.

Einstein was also in Berlin, but I never met him — he did not lecture, and I only saw him from a distance at colloquium meetings. But it was in lectures by Bothe (later famous as nuclear physicist) called "X-ray physics" that I heard for the first time words like the quantum of action, the Bohr orbits and the K shell, etc., which made me realise that there were new things in physics which from our school work one had not suspected.

I also took a number of mathematics courses, which gave me a good background. Altogether, I was registered for something like 35 lectures a week, and while attendance was not compulsory, I did attend most of them most of the time, without feeling hard pressed, and finding time to enjoy the many cultural activities of Berlin in the twenties (there must have been more hours in the day then).

After two semesters, I decided that I was old enough to leave home, and being advised that Sommerfeld was the best teacher of theoretical physics, I moved to Munich. At that time there was in Germany no bachelor's degree in physics, and no distinction between undergraduate and postgraduate students. So I joined the group of students working with Sommerfeld, while attending lectures on basic physics.

At that time quantum mechanics was new, and the papers of Schrödinger had just appeared. Sommerfeld was attracted by wave mechanics, because of his great experience with differential equations; he hoped for a long time that one could develop the theory with the wavefunction having physical reality, as in electromagnetic or sound waves.

He was indeed a great teacher. His lectures presented theoretical physics in a way which never let you forget that this was an empirical science, and that one had to look at the experimental evidence for each piece of the laws. He also was a master at finding problems within the capacity of a good student, yet non-trivial enough to be worth spending time on. Of course one learned not only from the professors, but also from the other students. There was, above all, Hans Bethe, one year my senior, with whom I formed a lasting friendship.

My first assignment was to give a seminar talk on the transformation theory of Dirac and Jordan, which was then new. Quite a tough assignment for a student of two years' standing or less, and I learned a lot from this — I hope the audience learned a little, too.

After my third semester in Munich, Sommerfeld went on sabbatical leave to the United States, and on his advice I moved to Leipzig, where his pupil Heisenberg had started a school of theoretical physics. Here I found another great teacher, but with a very different approach from Sommerfeld's. Heisenberg had a powerful intuition, which made him see the answer to each problem on physical, rather than mathematical grounds. When he had understood what the answer must be, he would look for a mathematical method which would give that answer. This was a very effective approach when used by someone with Heisenberg's intuition, but dangerous for others to imitate.

Here I had my first opportunity to make a contribution to physics, and to publish my first paper. Heisenberg suggested that I might try to apply the picture of electrons in metals, which Felix Bloch had then developed in his department, to the problem of the Hall effect. On the old electron theory of metals it had been impossible to understand why this had the "wrong" sign in some metals, as if the electricity was carried by positive carriers. It was very gratifying to find that one could indeed explain this in terms of nearly-filled bands, in which the conduction depended on the few empty places, or "holes". It was a glorious time for young theoreticians, because you only had to pick up any paradox that had worried people in the days before quantum mechanics, and re-examine it in the light of the new mechanics, to have resolved another puzzle and added to the confirmation of the new system.

Heisenberg was very helpful to his students, and if one ran into difficulties one could be sure of a sympathetic hearing. But he did not take much interest in the students' writing of papers, and I now find some of my Leipzig work rather hard to read.

The others in Heisenberg's group included, besides Felix Bloch, who had opened the field in which I started to work, Guido Beck, who was then Heisenberg's "Assistent", and George Placzek, one of the finest and most cultivated men in physics.

After two semesters in Leipzig, it was time for another change: Heisenberg was going on sabbatical leave to the United States, and on his advice I went to the Federal Institute of Technology (E.T.H.) in Zurich to work under Pauli. Here was another great man, perhaps not as organized a teacher as Sommerfeld or Heisenberg, but with a clear mind, a quick understanding of problems that interested him, and above all, extremely high standards of rigour in physical thought. He was very impatient with sloppy thinking or half-baked theories. He was also famous for his very outspoken critical remarks, always very painful for the victim, because they always touched sensitive points, but never resented for long, because one knew that he was equally critical of himself and his work.

When I came to him in April 1929, he suggested I might look at the problem of heat conduction in insulating crystals, in which the heat is carried by lattice waves (or "phonons" in modern terminology) as Debye had pointed out. The free motion of the phonons is limited by their mutual coupling through the anharmonic forces. Pauli had tried himself to study the anharmonic forces to find the damping of sound waves in a crystal, but he suspected his solution, and wanted it investigated properly.

I found indeed that the true answer was rather different from his, and from all other published attempts, and that one could understand the essential features of the thermal conductivity. In the course of this I introduced the concept of "Umklapp" processes, which are, in a sense, collisions of phonons combined with a Bragg reflection in the lattice. This led me to predict the exponential rise of the conductivity of pure crystals, which was confirmed by experiment only over 20 years later.

I wrote my thesis during the summer and typed it on a new typewriter, a birthday present from my parents. One semester in Zurich was of course not enough residence to qualify for a doctor's degree, so I had to submit my thesis to Leipzig. One had to pass an oral examination in three subjects, mathematics, physics and chemistry. Heisenberg was still abroad, so my physics examiner was Hund, also Professor of Theoretical Physics at Leipzig. Instead of the searching questions on quantum mechanics, which I expected, he asked me mostly about the classical mechanics of a spinning top. I struggled through somehow, and when the examination was over, Hund explained: "I asked you these questions only so you should not think you knew everything".

In the autumn of 1929 I returned to Zurich, where Pauli had offered me the post of "Assistent", and I stayed there three years, my longest stay in one place until I got to Birmingham. The duties of the post were not heavy and it was mainly a research appointment, the topics of research to have Pauli's approval (which one would have wanted to have anyway).

At Zurich I continued work on solid-state problems. One piece of work which gave me particular pleasure concerned the problem of an electron in a weak periodic potential. For the work I had previously done on the Hall effect, it was essential that there were energy bands with the energy being a smooth function of the wave vector also near the top of the band. Bloch had given the general theory of this, and had given explicitly the solution for the case of tight binding, but I could not see how this connected with the case of nearly free electrons. This worried me, because without some knowledge which was generally valid for the structure of the energy bands, my explanation of the Hall effect was lacking in completeness. Then one day I saw the connection, in terms of Bragg reflections, which cause gaps in the spectrum even for a very weak potential. I was very happy with this insight, which I demonstrated only for the case of one dimension. It was later generalised by Brillouin to three dimensions, leading to the well-known picture of Brillouin zones.

Another result of the Zurich period was a closer study of the interplay of electrons and phonons in metals, with the conclusion that certain metals, when very pure, should show a conductivity rising exponentially at very low temperatures. This conclusion was verified by experiment only in the 1960s. There was also a major study of the absorption spectra of solids and the lifetime of excitons.

One of the visitors to Zurich was Landau, who became a close friend. We wrote a joint paper about expressing quantum electrodynamics in configuration space, which was of some methodical interest, though not very sensible physically. Later we wrote a paper on new uncertainty relations for relativistic field theory, which got us involved in controversy with Niels Bohr. Landau had an impressive breadth and depth of understanding of physics, and to argue with him was most profitable.

At this time electrodynamics was of great concern to theorists. In classical theory the self-energy of the electron was infinite, unless one assumed the electron to have a finite radius, an assumption not easily reconciled with special relativity. With the advent of quantum mechanics, so many difficulties of the classical theory had resolved themselves that one naturally expected the infinite self-energy to disappear also. This seemed plausible because its origin was the point nature of the electron, and in quantum mechanics the position of a particle was a less simple concept. But it turned out that in quantum electrodynamics the infinity remains, and in fact there are further infinities arising from the infinite number of ways in which electron-positron pairs can be generated. We were all worried about these problems, and I spent much time discussing them with Pauli, and making attempts to avoid the troubles, but got nowhere.

Other members of Pauli's group from whose presence I benefited included Rosenfeld, Delbrück, Racah, Oppenheimer, Bartlett and Freenberg. Wentzel was the professor at the University, and in close contact with the E.T.H. group.

During the Zurich years I paid several visits to Copenhagen, where it was wonderful to have the opportunity of discussions with Niels Bohr, whose charm and depth of understanding captivated everybody, in spite of some difficulty in understanding his use of language, which was not simple, and in spite of his habit of dominating a discussion and talking more than listening. I was to return to Copenhagen many times though I never stayed for a prolonged period.

Another important journey from Zurich was to a Soviet physics conference in Odessa in 1930, to which a few foreigners were invited, and where I made the acquaintance of many Russian colleagues, in particular of Frenkel, a very lively and imaginative theorist, and Tamm, a man of immense charm and absolute integrity.

At the conference there was also a girl who had just completed the physics course in Leningrad, and of whom I saw a good deal during the post-conference excursions. We corresponded during the following winter. In the Easter vocation of 1931 I gave a course of lectures in Leningrad, and a week or two after my arrival we were married.

In 1932 I was awarded a travelling fellowship by the Rockefeller Foundation. It was a condition of these fellowships that the holder had to have a post to return to after the year of the fellowship, and accordingly Pauli certified that I could return to the post in Zurich, but at the same time it was understood between us that I would not come back, since he liked to give other young people a chance.

I chose to divide my fellowship year between Rome and Cambridge, spending the winter in Rome and the summer in Cambridge, thus improving on the timing of Hans Bethe, who had done the same in the previous year, but in the opposite order. The winter spent in Rome with Fermi's group was a great experience. Fermi was then already planning to do experiments in nuclear physics, but it took time to get the equipment together. Most of the group were then occupied in doing calculations about atomic spectra, and this needed solving the Schrödinger equation numerically by means of manual calculating machines. I was allowed to join this project and learned how easy it is to solve simple differential equations that way, and how much more informative than elaborate analytic solutions. If one raised some physical problem with Fermi, he would usually take one of a number of notebooks from his shelf and turn up a page where the solution to the problem was worked out. That was assuming the problem was simple — he did not like complications. But simplicity is in the eye of the beholder, and there were many problems which appeared obscure and complicated until Fermi had shown how to look at them to make them simple.

The others in Rome at the time included Majorana, whose creative power is by no means adequately reflected in his published papers, Wick, with whom I had many enlightening discussions, and Amaldi, Rasetti and Segré.

In Rome we made good use of our spare time to look at the sights; six months is not enough to see everything worth seeing in Rome, but one can get a fairly full impression. We were of course conscious of the Mussolini regime, and depressed by it. When one went to the cinema, there was usually before the main film a short documentary called "Opere del regime", showing the latest new buildings, which we usually found in rather bad taste. When on our wanderings through Rome and other parts of Italy, we encountered particularly hideous buildings in Mussolini's taste, or some piece of obvious social injustice, we would exclaim "Opere del regime!".

I was then keeping my eyes open for a position to go to after the end of the fellowship, as we were not returning to Zurich. The offer of an assistantship at Hamburg seemed attractive, because of the presence of Lenz, a good theoretician, and particularly of Stern, a very inspiring experimentalist. So I accepted informally, and was even ready to forgo the second half of my fellowship, as the post was to be filled as soon as possible. But then the situation in Germany became worse. Hitler had not yet come to power, but when the conservative, but reasonable, Chancellor Schleicher was removed, and von Papen put in his place, we knew that Germany had become the wrong place for us, and I withdrew my acceptance.

So we arrived in Cambridge in April 1933, amongst the first of a long stream of Germans and later Austrians, who had to, or wished to, leave their country. This was a time of economic depression, and jobs, particularly in universities, were short. It would have been understandable if the British academics had resented the arrival of so many competitors. But their reaction was wonderfully generous. They made the new arrivals feel welcome, and they took the initiative in setting up machinery, known as the Academic Assistance Council, to help displaced university people settle down and find their feet.

The summer in Cambridge got me in contact with Dirac, Fowler and Mott, and with many visitors, including Weisskopf. I also made friends with S. Chandrasekhar, then a young and promising astrophysicist. I divided my time between further work on the electron theory of metals, including a study of diamagnetism, and attempts to get around the difficulties of electrodynamics.

I was surprised to see many advertisements of academic posts in the scientific journals, a practice with which I was not familiar, and which gave the impression that there were plenty of openings. I hopefully applied to those that seemed attractive, but of course there were many other applicants. Eventually I was offered by Manchester a grant from a local fund similar to the Academic Assistance Council, and spent the next two years at the University of Manchester.

The Physics professor was W.L. Bragg, and the only theoretician in the department was E.J. Williams, a well-known expert on radiation problems, while Hartree was the professor of Applied Mathematics. Bethe came to Manchester at the same time as I did and stayed for a year. He became our lodger; I collaborated with him, first on the statistical theory of superlattices, inspired by the interests of Bragg and Williams, and later, when we heard of Chadwick and Goldhaber's experiments, on the photodisintegration of the deuteron. This led to some work on the scattering of neutrons by hydrogen. Fermi's theory of beta-decay, which came out about that time, inspired us to publish two short notes about its consequences.

Manchester was an ugly industrial town, which was quite a change after Zurich, Rome and Cambridge, and one had to get used to the fog, which at that time, before the introduction of smokeless zones, was frequent, and dense. Sometimes, after trying to cross a wide road, one had to ask some passer-by on which side of the road one was now. But all this was made up for by the great charm of the people.

After two years in Manchester I was offered a research appointment in the Mond Laboratory in Cambridge, which belonged to the Royal Society. This was a laboratory for work at low temperatures and with strong magnetic fields, built for the Russian physicist Kapitza. He was in an exceptional position among Soviet scientists in that he had a permanent passport, and could go in and out, working in Cambridge, but returning to Moscow for his vacations. But in 1934 the Soviet Government decided that he should stay, and during his summer visit cancelled his passport. To enable him to work in Moscow they bought from the Royal Society the equipment developed for him, and a replica was made for the Mond Laboratory. Now the money set aside for his salary in Cambridge was left over, and this was used to create two research fellowships, of which I got one, the other went to an experimentalist, J.F. Allen.

I was happy to return to Cambridge, then very much the centre of British physics, and happy to have a regular position, even though it was not permanent. The connection with the Mond Laboratory got me involved in low-temperature physics, but I was also in touch with the Cavendish Laboratory and thus with nuclear physics. I got to know Rutherford more closely, and was impressed not only by his personality and his stature as a physicist, but also by his charm, simplicity and sense of humour. Cockcroft, while working on nuclear problems, kept an eye on the Mond Laboratory, and I saw a lot of him. I did some teaching at Cambridge, which involved some lectures, and also some individual teaching, so-called "supervisions" where one works with two or three students once or twice a week.

With contacts in the Cavendish and Mond Laboratories, and with R.H. Fowler's group, which was interested mainly in statistical mechanics, my research interests were spread over a wide field.

One amusing experience came when I attended a seminar in which a mathematician talked about a problem in statistical mechanics. He was reporting about a famous paper by Ising, a German theoretician, proving that a certain model does not, at low temperature, go over into an ordered state. This much is undoubtedly true, but the speaker then went on to argue that the same must also apply in two or three dimensions. I knew intuitively that this was not right, and got irritated. It would have been difficult to find the error in his reasoning, so I decided it would be simpler to prove the contrary, that is, to prove that in two dimensions there is an ordered state. This turned out to be very easy, and I wrote a short paper about it. The method used in this paper seems to have remained for about 40 years the only one available to prove results about ordered states, though now some better methods have been invented.

The next change in my place and position came in 1937, with a professorship in Birmingham. Oliphant, one of the senior people working in Cambridge with Rutherford, had been appointed to the chair of experimental physics in the University of Birmingham and persuaded the university to create a theoretical chair, which was then called Applied Mathematics, and belonged to the Mathematics Department. At Oliphant's suggestion, I applied, and eventually was fortunate in winning the appointment.

Here I was joint head of Mathematics, together with the professor of Pure Mathematics, G.N. Watson, a great mathematician and a great original, who did not like to use fountain pens (this was before the invention of the ball pen) or the telephone. But he graciously agreed to a telephone being installed in the office we shared, provided it was understood that he did not have to answer it when I was out.

We easily agreed on how the teaching was to be shared. As regards the research, I thought I should make a modest start in a place which had no tradition in theoretical physics. I would perhaps find some able students and train them in modern theory, and in this way gradually build up a research group. While I had indeed a few good pupils, I discovered that this was not a possible way to build up a research group. It takes a certain "critical size" to get a start, and a variety of approaches, so that not all the members of the group have to acquire their knowledge through one teacher.

But by 1939, two years after my move to Birmingham, this problem had been made academic by the outbreak of war, and later I had an opportunity to make a new start. The war made some difficulties for me, as I was still a German citizen, and therefore technically an "enemy alien". At first my wife and I were subject to various restrictions. We were not allowed to own a car (a curious legalistic rule was that we could drive, but could not own it!) to have maps or town plans in the house, etc. But soon tribunals were appointed to grade the enemy aliens into three categories, those who were trusted, those of whom not enough was known, and those who were regarded as real enemies. We were fortunate in having enough friends vouch for us to get into the first category, and in February 1940 we got our naturalization papers, although no fresh applications for naturalization were then accepted.

But regardless of any questions of formal status, I was of course enthusiastically on the side of Britain in the war against Hitler, and anxious to contribute. My status of a favoured enemy alien, and later as a naturalized British subject, was not enough to take part in secret war research, and I was not allowed to join Oliphant in his work on Radar. But I was accepted as a volunteer for the Auxiliary Fire Service, and I spent many nights during air raids fighting fires.

There were still some students about — the brightest scientists were allowed to complete their studies, and there were some men unfit for military service, and women students. So there was some teaching, and I continued doing research, some of it in collaboration with O.R. Frisch, who had arrived from Copenhagen just before the outbreak of war.

We knew of course of the discovery of uranium fission, in whose elucidation Frisch had played a major part, and we had learned from the work of Niels Bohr that it was not possible to make a weapon of ordinary uranium. Then one day Frisch asked "what would happen if one had a quantity of the light uranium isotope?" I had then just written a paper on how to calculate the critical size for a chain reaction, and we could make a guess at the nuclear parameters from the Bohr theory. So we estimated the critical size on the back of an envelope and found it surprisingly small. We then asked how far such a chain reaction would go before the heat it develops would disperse the material and stop the reaction. The back of another envelope showed that an appreciable part of the available fission energy would be released. Here was the possibility of a weapon of enormous power, which would justify a very major effort in separating isotopes on a large scale, which had not been done before. Even, we said, if the isotope plant costs as much as a battleship, it would be worth while. This proved a gross underestimate.

We were particularly appalled by the thought that the same idea might have occurred to the German scientists and that Hitler might acquire such a weapon. We then wrote down our arguments and our conclusions about the nature of such a weapon, including the fallout it could cause. This memorandum was sent on to higher authority, and resulted in a committee being formed (nicknamed the MAUD Committee) to commission further study of the problem. In about a year, after further experiments and theoretical studies, the committee concluded that the production of a nuclear weapon was feasible, and recommended that this work be carried on at great speed. This decision may have influenced the authorities in the United States to take the possibility seriously and to start work on a large scale.

To complete such a large project in wartime England was clearly very difficult, and the alternative of working together with the Americans was attractive. But after an initial exchange of information in 1942, political difficulties developed and for a time the exchange of information with the United States was stopped.

Eventually after an agreement between Churchill and Roosevelt at the Quebec conference in 1943, collaboration was resumed, development work in Britain was halted, and a number of people were seconded to assist the American work. I moved first to New York to work with the designers of the American isotope separation plant, and in the summer of 1944 to Los Alamos, to work on the design of the bomb.

Los Alamos was a very unusual place, a very isolated community in gorgeously beautiful surroundings, and containing an exceptionally powerful collection of physicists, and other scientists, many of whom we already knew from all our previous wanderings. All were working under great pressure, knowing the urgency on the work, and having no illusions about its seriousness. At first the motivation was to make sure the Germans did not get this weapon first. After the defeat of Germany the work continued under its own momentum. It seemed right to develop this powerful weapon to help end the war against Japan. The scientists did not give much thought to the use the military would make of the bomb, since this would depend on many factors we had no information about. The feeling was that the statesmen and military leaders were reasonable and decent people; our job was to explain to them the nature of the new weapon and its consequences, and trust them to act responsibly. Perhaps we were naive; we were certainly optimistic about the capacity of political leaders to appreciate the facts we were trying to explain.

Watching the first test explosion in Alamogordo, one felt awe at the tremendous power which had been released, mixed with satisfaction that the theoretical forecasts had proved right, and that our colleagues had mastered all the difficult challenges. A few weeks later came the destruction of Japanese cities by atomic bombs. It brought relief at the end of the war. While we regretted the large number of casualties, we knew they were no greater than in the fire raids on Tokyo, carried out by "conventional" means. Later many of us concluded that the use of the bombs on cities had been unnecessary, at least without a first use on a smaller target, which would have shown the power of the new weapon, and that in any case the second attack was certainly unnecessary.

After the end of the war we returned to England and to normal academic life — normal except for the fact that the prestige of physicists was high, and support easy to get. I persuaded the university to create a number of research fellowships, as well as another teaching post in my department, which was growing out of the confinement of a joint Mathematics department and became the Department of Mathematical Physics. This new start was the last of the frequent changes: I remained in Birmingham as Professor of Mathematical Physics until 1963.

During these years my main interest was to look after the growing number of graduate students and other collaborators, and I became accustomed to doing my own research in collaboration with students. Of course, undergraduate teaching and administration also needed attention. Fortunately the structure of the university was such that formal administration and committee work did not take much time. If one counts as administrative the care of research students, such as correspondence about their admission, recommendations for those who were leaving or had left, and attention to their personal problems from housing to medical care, this represented a non-negligible load. But the more personal problems were largely looked after by my wife, without whom the school could not have functioned so satisfactorily.

My research interests remained as wide as ever; I looked, with or without students, at problems in solid-state physics, nuclear physics, field theory, and even some cosmology. At the same time I regarded it as important to explain the ideas and results of physics to general audiences, and there were many opportunities for popular lectures, and for articles and a book, "The Laws of Nature".

I was also concerned with the consequences of the development of nuclear weapons, in which I had taken part, and tried to work for disarmament, and the control of nuclear weapons, first through the Atomic Scientists' Association, and later through the Pugwash Conferences.

My desire for changes of scenery was in part satisfied by visits to conferences and summer schools and by two sabbatical periods at the Institute for Advanced Study, Princeton, and at Columbia University.

It is said that the specialist is a person who learns more and more about less and less, until he knows everything about nothing, whereas the generalist learns less and less about more and more, until he knows nothing about everything. In the sense of this classification, I certainly belong to the generalists, and I do not feel apologetic. Science could not make progress without the specialist, but the presence of some generalists is also essential, and perhaps there are not enough of us.

But with the rapid growth of knowledge and techniques in all parts of modern physics I became very conscious of the depth of my ignorance expressed by the "nothing" in the last phrase. It meant that in running my department, I had to rely more and more on the judgment of my younger colleagues. I was fortunate in having many outstanding younger collaborators, including G.E. Brown, P.T. Matthews, S.F. Edwards, G. Chester, R.H. Dalitz, S. Mandelstam, but, as is normal with good people, they were tempted away sooner or later, and it became hard to ensure continuity.

I therefore accepted in 1963 an invitation to the Wykeham Professorship of (theoretical) Physics, at Oxford, in a department which contained several permanent members of outstanding ability. Here I was able to continue keeping my interests wide, and to rely on such people as R.H. Dalitz, R.J. Elliott, J.C. Taylor, D.M. Brink, to keep their areas up to date.

It was fun to learn the ropes in an old university, where the procedures are very different from what I knew at Birmingham. Superficially, the administration at Oxford seems much more bound by tradition, but in substance there is a surprising amount of flexibility, and one can do remarkably unorthodox things, once one knows the right approach to them.

One attractive feature of life in Oxford is the connection with the colleges. A professor is not allowed to take part in the teaching work of the college, and is conscious of being, in a sense, a parasite. But I enjoyed being a fellow of New College, in a friendly community of distinguished people from all fields.

One more sabbatical period in 1967 took me to the University of Washington in Seattle, which combines a first-rate and very friendly physics department with a uniquely beautiful environment amongst mountains and waters. Seattle became almost our second home, and we returned there nearly every summer. When I retired from Oxford in 1974, I was given an apointment as half-time professor (six months a year) in the University of Washington, which lasted until 1977, when I reached their age limit.

My retirement from Oxford was the occasion for a two-day symposium, attended by some 200 former students, collaborators and colleagues, which for me was a very moving and pleasing occasion.

Since then much of my time has been spent travelling to more places than could be enumerated here, mostly as visiting professor or informal consultant, thus keeping up my habit of change. But recently I have tended to spend more of my time in Oxford, to become more sedentary.

The "nothing" in the formula is wearing very thin, and I am looking mainly at small problems which are still in need of tidying up. I appreciate the freedom of a pensioner, but miss the contact with students.

I also find myself increasingly in demand from historians of science, for whom the 1920's and 30's are of great interest. Having been a spectator (and sometimes even an actor in a minor part) I can often help them, though one must remember the fallibility of human memory and check one's statements against written records wherever possible. So I am becoming an amateur historian of science myself.