George de Hevesy

LIFE AND WORK

A Biography
by
Hilde Levi

RHODOS

not represent all facets of his identity. Throughout his youth and adolescence he had belonged to the catholic Magyar nobility, and until Hitler decreed new definitions of ethnic terms, there was no conflict on Hevesy's mind. The introduction of racial laws in Germany, however did give rise to a serious conflict – as it did for innumerable others who had considered their "racial" background a matter of little or no concern and felt like equal citizens of their country. For the second time in his life, Hevesy was made homeless and uprooted – this time not in consequence of the overthrow of an old social order, as was the case in Hungary 13 years earlier, but primarily because his ancestors belonged to a minority group which – according to Hitler – no longer was permitted to be assimilated into the society of which they were a part.

It is remarkable that Hevesy – at least in his conversations and his letters to friends and acquaintances – could express optimism and see the positive rather than the negative aspects of his new situation. When he realized that everything he so successfully had built up in Freiburg was about to collapse, he accepted Bohr's offer with great anticipation and did not worry too much about the uncertainties which again burdened his future in Denmark. Quite to the contrary, he convinced himself that Denmark was the place where he really belonged, and he made himself believe that the losses he suffered by leaving Freiburg were more than compensated for by the gains of being reunited with Bohr.

Hevesy wrote to Paneth on 25 August 1933.
"... We left Hornbæk on the 17th, stayed for 4 days with the Bohrs in their princely mansion and are now in Hungary. I found Bohr grander and greater than ever. Most people do not grow any more when they have reached the age of 40. His fantastic personality develops more and more. I left with the impression that even without the unpleasant development in Germany, my place is really in Copenhagen and if one has the chance to live in the environment of such a unique person, one should not live somewhere else. I have already completely overcome leaving my institute in Freiburg. What I have not yet overcome is saying goodbye to my colleagues and students with whom I had a cordial relationship. For example, I saw Staudinger almost every day and we always discussed all the details of our teaching and carried them out together. By the way, I

Second Copenhagen Period 1935-1943: The Development of the Tracer Method

During the span of time Hevesy was a professor of physical chemistry in Freiburg, nuclear physics had progressed at an enormous pace. Frédéric and Irène Joliot-Curie in Paris had observed that bombardment of aluminum with alpha particles led to the formation of an unstable, that is to say radioactive element which – within a short time – disintegrated and returned to a stable state. James Chadwick in England had found a new elementary particle, the neutron, which carries no charge but a mass equal to that of the proton. Shortly thereafter, Enrico Fermi and his co-workers in Rome used this new particle to bombard a number of light elements and they observed that one or more unstable, radioactive isotopes were formed in each case. It soon became clear that almost any stable element in the periodic table – when bombarded with neutrons – was transformed into at least one radioactive isotope which was chemically different from the bombarded one. In many cases, for example that of radio-phosphorus, the radioactive nuclei formed emitted an electron and were transformed back into the element of the original target. The rate at which this spontaneous re-transformation occurred varied from element to element, ranging from fractions of a second to thousands or even millions of years.

Hevesy had always regretted that the radioactive elements he had used in his chemical indicator experiments were so toxic that they could not be applied in biological studies. Shortly before he left Freiburg, he had taken up heavy hydrogen as an indicator, but in view of Fermi's results, the situation improved dramatically. Artificially produced radioactive elements were to bring Hevesy world renown, he became an innovator, especially in the life sciences.

For a relatively long period before the 1930ies, there was no direct interaction between science and the political situation in Western Europe.

With the fateful rise of the Nazi regime in Germany, a situation of this kind suddenly existed. The coincidence of a number of factors, some of them scientific, others political or economic, strongly affected the course of events for several decades to come. These factors played an important role also at Niels Bohr's institute in Copenhagen where Hevesy resumed experimental work early in 1935.

The author of the present account entered the scientific scene at the start of this period. As a newly hatched Ph.D. in physics and a Jewish refugee from Germany, I was accepted at the institute in Copenhagen in the spring of 1934, and was asked to assist Professor James Franck who arrived in Copenhagen almost at the same time. He had resigned from his position in Göttingen. Although Franck was a close friend of Bohr's and had stayed at the institute on several occasions since its foundation in 1921, his visit was not expected to be of very long duration. Negotiations with American universities were in progress, and early in 1935 Franck accepted a chair at the Johns Hopkins University in Baltimore. His departure in the summer of that year was deplored by all his friends and colleagues in Denmark.

Several months before Franck left for the United States, Bohr suggested that I continue my work at the institute as an assistant to Hevesy who was expected to arrive from Freiburg in the late fall. I recall how Bohr explained to me that the exciting new results with artificially produced radioactive isotopes undoubtedly would capture my interest, and that both he and Hevesy were eager to take up this new field. Since the institute was equipped mainly for spectroscopy, it was imperative at this point to concentrate on the building of instruments for radioactivity measurements. This was a field I knew very little about but Otto Robert Frisch, who presently worked in England, was due to come to the institute, and maybe – said Bohr – he could teach me how to build these instruments, so we could follow up and carry on Fermi's research.

Thus, although my work with Franck continued while Hevesy moved into his new apartment and re-established himself in the familiar Copenhagen surroundings – in between travelling to various places – Frisch and I began to build Geiger counters and simple amplifiers; we used the telephone company's call counter to register the number of beta particles that passed the counter for example from a small sample of a uranium salt.

Hevesy started his work in the early spring using the precious rare earth preparations he had obtained from Auer von Welshbach. It was revealing to discover recently that Fermi had written to Hevesy in October 1934 asking him for small quantities of these elements for experiments with neutrons; but Hevesy gave a rather evasive answer, referring him to other people who might have some of this material. As his first project he intended to investigate the induced radioactivity of the rare earths and a few other elements which he had studied earlier, such as hafnium, scandium, and potassium.

His approach to the field proved to be most successful and important; first: the induced radioactivity and the radiation properties of these elements were established and described. Second: on the basis of his observations, Hevesy could prove that the potassium isotope K-40 is responsible for the natural radioactivity of potassium – a problem he had tried to solve earlier without arriving at an unambiguous result. Third: the most important discovery from this short period of research was the basic development of neutron activation analysis.

As discussed in the previous chapter, the separation and purification of the different elements within the group of rare earths was exceedingly complex. Chemists in many countries were still laboring with this problem using the classical methods of analytical chemistry. Hevesy suggested that – as a means of identification – we make use of the characteristic decay period of each of these elements and of their relative intensities of activation after neutron bombardment. In this way, their presence could be determined in any unknown mixture. In the early phase of this work we observed that, with the neutron sources available, the strongest activity was induced in the rare earth element dysprosium. It was therefore easy to detect even a minute amount of a dysprosium compound present as an impurity in the salt of any other rare earth element. This finding was published in 1936. The classical test object Hevesy used one year later was a sample of gadolinium which Luigi Rolla of Florence had tried to purify from traces of europium. We could see from the decay of the induced radioactivity that the sample consisted of two components decaying at different rates. Hevesy then added varying quantities of "impurity" to the purified gadolinium, and we compared the resulting changes in intensity of radiation from the two components. It was possible with this technique not only to identify the impurity but also to estimate how much of it was present. As we look back on these pioneer experiments today we must keep in mind that the neutron sources available at that time – the well-known mixture of radon gas with beryllium powder enclosed in glas ampullas – were very weak ones, and so was the resulting radioactivity of the bombarded sample. Fifty years later, a nuclear reactor is used as the neutron source; it is at least ten million times stronger than Hevesy's sources. Therefore, the sensitivity of this method of analysis is now correspondingly higher. The term "neutron activation analysis" was coined in the fifties when the method was adapted to the powerful neutron source and was made much more sophisticated by means of electronic analyzers. Today the technique has wide application in radiochemistry, technology and studies of environmental contamination.

While the exciting work with artificially produced radio-isotopes of the rare earths was in progress, Hevesy became more and more interested in the production of a radio-isotope of one of the lighter and biologically interesting elements. Radio-phosphorus seemed to be the obvious candidate. Its production and properties had been described by Fermi whose results were confirmed at the Bohr Institute. Radio-phosphorus can be produced by bombarding sulphur with neutrons; it decays under emission of a fairly penetrating beta radiation (electrons) and its rate of decay (half-life) is about 14 days. These are most attractive properties. The half-life is conveniently long so that biological experiments on animals or plants can be performed; the electrons are easy to detect by means of a Geiger counter, and the element phosphorus plays an important part in living organisms, f.ex. as calcium phosphate in the skeleton or as inorganic and organic phosphorus compounds for example nucleic acid and the energy transfer molecule ATP present in practically all tissue.

Thus, Hevesy set out to produce radio-phosphorus so that he could feed or inject radioactive sodium phosphate into animals and find out what happened. Today the description of his procedure and of the home-made instruments used to measure the distribution and excretion of the phosphorus injected, impresses us as rather primitive; at the same time we marvel that Hevesy could arrive at such epoch-making results.

In retrospect it is equally impressive that the concept of "radioactive indicators", first conceived and applied in Manchester and Vienna before the First World War as a tool in analytical chemistry, grew far beyond its original use into much wider, more complex and less tangible domains. When Hevesy ventured into this entirely new field, he had not designed his experiments with any clear expectation of what he might observe. However, he must have discussed his plans with Ole Chievitz, the head surgeon of the Finsen Hospital, a classmate and close friend of Bohr's and also a friend of Hevesy. Chievitz placed the experimental animals, laboratory facilities, and even a technician at Hevesy's disposal, so that all practical problems were taken care of. After the animals had been injected, samples were taken, and brought into a manageable form by combustion or by being dissolved in acid followed by precipitation of phosphate.

They were then brought to the institute where the Geiger counters were located and all radioactivity measurements were carried out for many years to come.

The first publication dealing with the new application of radioactive indicators described the pioneer experiment with P-32 injected into rats. This report had the form of a letter to the editor of *Nature*. It was submitted in September 1935 and signed by both Hevesy and Chievitz. The authors not only described the experiment and presented the results, they – that is to say Hevesy – made a rather sweeping statement of interpretation which was not in agreement with the views widely held at that time. It reads: "The results strongly support the view that the formation of the bones is a dynamic process, the bones continuously taking up phosphorus atoms which are partly or wholly lost again and are replaced by other phosphorus atoms". The editor of *Nature*, an anonymous ruler over what was accepted and what was rejected by this most prestigeous of science journals, was Mr. Gregory with whom Hevesy had travelled to South Africa in 1929. He accepted this remarkable paper but took some precaution in an editorial comment printed a few pages further on, where he laconically noted "The authors further believe that the formation of the bone is a dynamic process, involving continuous loss and replacement". It seems that he wanted to place himself at some distance from such untraditional thinking.

This pioneer experiment was a signal to the biologists: while Hevesy's earlier application of the indicator method was of interest mainly to analytical chemistry, it was now the biologists' turn to learn what isotopic indicators are all about. The basic fact is this: artificially produced radioactive phosphorus atoms are chemically identical with the naturally occurring, stable phosphorus atoms, and therefore they follow the stable phosphorus atoms in metabolic processes. However, they can be observed because as they decay they emit electrons which give rise to a pulse in a Geiger counter. In other words, the use of radioactive indicators enables us to distinguish between atoms which entered any given organism at the time of the experiment from those (of the same element) which had been there before. Consequently, the location and the movement of these "labelled" atoms or molecules as a function of time, their exchange with stable ones present in the system, and their incorporation into different compounds can be observed. This means that processes which up to the present were unobservable in principle can now be studied. Naturally, the method lends itself especially to the investigation of dynamic processes.

In the course of the next few years Hevesy sometimes placed his observations in a most untraditional context. It was not his style to plan and let his co-workers perform laborious experiments which had to be repeated several times; nor would he plough through the literature looking for supporting or contradicting evidence, nor make elaborate calculations. Quite to the contrary: he performed only very few experiments, launched into an interpretation of the results, and – since writing presented no difficulty for him – put it all on paper immediately. He sent off the manuscript for publication within a short time. I even remember situations where Hevesy had the manuscript ready before the experiments were concluded. He impatiently asked for the last results and inserted the figures in the text. He was convinced that the experimental results would confirm his expectation.

Hevesy had a flair for choosing the problems which could be clarified by means of the indicator method, for example the interrelation between the components of a complicated system, and in the large majority of cases, his intuition brought him on the right track in spite of the fact that – when he began this work – his knowledge in the fields of biology and biochemistry was scanty, to say the least. Thus, about half a year after his arrival in Copenhagen, Hevesy had introduced a new technique into the life sciences, which he had been dreaming of for almost 20 years.

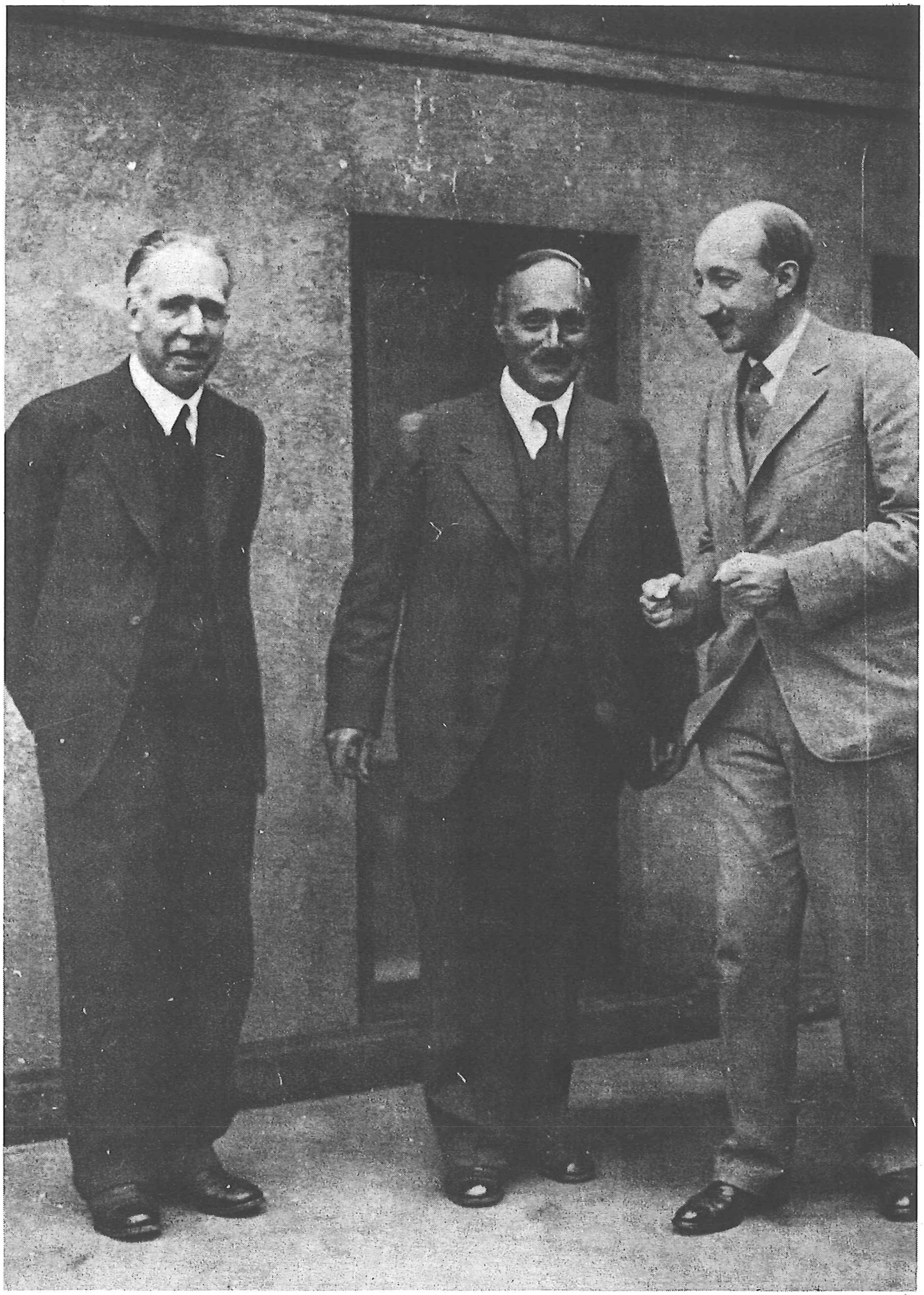
I have already given a brief outline of the development in nuclear physics, which made the application of the indicator method possible, and also of Hevesy's departure from Freiburg and the consequences of his move. In Copenhagen, Hevesy had very modest laboratory facilities, no economic backing, just one young assistant. But in his opinion – already quoted in the preceding chapter – he had truly gained because he valued the re-union with Bohr and with the Copenhagen institute so highly. I vividly remember an episode which took place in the spring of 1935: James Franck and Hevesy met in the laboratory and Franck made a nostalgic remark about the "old days in Göttingen" which he missed painfully. Hevesy responded in an almost merry and at the same time encouraging tone: "Herr Kollege", he explained, "I am happy to be rid of all these troubles of running an institute. No more administrative duties, no more worries about raising funds, and, first of all, no more problems with co-workers, their future careers and their personal conflicts. I feel so relieved" – said Hevesy. Franck shook his head sadly; it was not his approach. He was lonesome for his co-workers whose sorrow and happiness he had shared. A greater contrast in outlooks of two great personalities can hardly be imagined.

At this point it may be appropriate to describe the general impression Hevesy made on his young assistant during the first years of our collaboration. Hevesy was tall and slender, his head and face were long and oval. As he moved about meeting people in the building, he had a friendly somewhat ironic and sometimes mocking smile on his lips. He was exceedingly polite and always addressed his subordinates in the same friendly manner as his colleagues and personal friends. He used to be dressed in an elegant, somewhat old-fashioned style; his cut-away or other tailored suits, for example, had been down-graded to everyday use. In bad weather he wore galoshes so that the presence of one – or sometimes two – of these boots outside his office door was a signal to us that he had either come or gone. Hevesy had the perfect manners of the Austrio-Hungarian aristocracy, which were noticeably different from those of the Scandinavian educated middle class, the background of most of his colleagues. Thus, my first impression of the new boss was that of an affable, remarkably polite gentleman, very conventional in his behaviour and impersonal in his relation with people. I felt there would probably never develop the kind of close personal contact – and friendship – that had made my relationship with James Franck such a unique experience.

When Hevesy entered the laboratory, his first remark inevitably was "good day, good day – are you fine, – yes." As the concluding "yes" indicated, he took it for granted that the person so addressed was feeling fine. It did happen – although rarely – that my answer was "no". This caused a complete derailment of his thoughts and forced him to ask what was wrong rather than to proceed to whatever scientific topic he had meant to talk about.

Hevesy spoke many languages with ease, including Danish, of course, and all of them with the typical Hungarian accent; he often mixed them freely – both in speaking and in writing – using whichever term first came to his mind. Although, for periods of several years, Hevesy had lived in an English, a German, and a Danish speaking country, aside from his native Hungary, he did not master any of the languages correctly, as is apparent from the numerous verbal quotations I have used to document his views. I soon found out that Hevesy was not a dexterous experimentor. He often hurt himself and became a well-known figure at the nearby emergency ward of the Rigshospital. When he did hurt himself badly, his reaction was that of a stoic. One of the episodes I remember clearly occured when Hevesy had burnt his arm with hot concentrated sulphuric acid. He came back from the hospital with his arm thoroughly bandaged, his face was rather pale. His secretary suggested that he go home and take a rest, but his laconic reply was: "Do you think it hurts less when I go home?" Instead, he disappeared into his office and closed the door.

Regardless of several minor accidents, Hevesy loved to fiddle with the instruments or with chemical procedures. When he found himself alone in the laboratory for a few moments, he would invariably take some readings of the counters and splash the notebooks with ink from his malfunctioning fountain pen; he would alter the voltage over the counter in any arbitrary direction, or change the sample that was being measured – as a rule to the



Niels Bohr, James Franck, George Hevesy at the Copenhagen institute 1935.
Courtesy: The Niels Bohr Archive.

despair of whoever was in charge of the measurements. Many years later, while reading his autobiographical notes and his letters from the early years of his career in which he described his classical experiments, I often wondered whether, as a younger man, he was in better control of his manual skills. However, I have never seen him angry or reproachful when something went wrong in the laboratory, regardless of who was at fault.

From the very beginning of our collaboration, Hevesy's personality made a great impression on me. He was not the gentle, loving, solicitous father figure that was James Franck; he was not the shy and at the same time overwhelmingly impressive Niels Bohr whose radiant personality pervaded the whole atmosphere at the institute. Hevesy was the dynamic promoter of new adventures, he was inspiring, his enthusiasm was contagious and demanding – as many of his "victims" will confirm with some misgivings. He expected his co-workers to make serious efforts and to work hard, and he got what he wanted by way of his friendly but insistent demands. He also impressed us with his daring ideas, his fabulous memory, and last but not least with his unusually great working capacity. His insomnia troubled him, but also gave him many extra hours for work and reading. On the other hand, there was something in Hevesy's personality which remained a mystery through all the years I knew him. He was remote and unapproachable as a human being. He surrounded himself with a shield of conventionality which very few of his friends were able to penetrate. Moreover, he did not make an effort to get to know his associates as human beings, he took no interest in their personal fate, although when asked for it, he was always willing to help, for example by writing carefully worded recommendations.

During the thirties the political situation in Europe and its consequences for a large number of people as well as the threat of war were the dominant topics of discussion everywhere. But even in this connection Hevesy displayed a detached, impersonal, and very unemotional attitude. To me, this seemed strange behaviour. After all, Hevesy had left Germany because of the Nazis. He never said a word about his own situation. Rumors would have it that the reason for his departure from Freiburg was "a Jewish grandmother". As many will remember, during the Hitler regime in Germany, "a Jewish grandmother" was like a collective code, or a measuring unit, in which a group of people's undesirability, their chance for escape or survival, could be expressed. "A Jewish grandmother" was the misfortune that had befallen many a distinguished family whose members did not consider themselves Jewish – neither in a religious nor in a "racial" sense. Apparently, Hevesy belonged to this group. He did not want to be mistaken for a person who had left Germany because of "race", rather, that he had taken this step because he was definitely opposed to the vulgar Nazi regime and its atrocities.

At the same time, he was so pre-occupied with his science that he was prepared to ignore a co-worker's or a colleague's political orientation as long as he found his work interesting or useful. This dualism became apparent within the group of scientific associates who joined Hevesy in the second half of the thirties. Among them were a few black sheep. One of them carried the swastica under the lapel of his coat, another later was unmasked as a spy for the Nazis, and a third aired such outspoken sympathies for the Third Reich that one wondered whether Hevesy ever talked with him about anything except the experiments they were performing together. I once went to Hevesy and complained about the uneasiness and embarrassment I felt in this company, but he brushed my concern aside: I should not pay any attention to this foolishness of my colleagues. He did not think it was worth troubling him with such matters.

During this period, a large number of refugee scientists came to the institute for longer or shorter periods seeking help and encouragement from Bohr. Between them existed the intangible and hardly ever verbally expressed recognition of a common fate. In this sometimes tense and troubled atmosphere, Hevesy appeared always unconcerned, almost superficial, pretending that he, personally, had no part in this drama. However, as I found out recently, this was not true; he was involved both on behalf of a few close friends whom he tried to help, and probably even more so in anticipation of his own and his family's future. It is astounding how well he was able to conceal this conflict behind the mask of his conventional attitude, his detachment, and his sarcasm.

An attempt at describing Hevesy's personality at the time he was about 50 years old would be incomplete without a few words about his sense of humor or, rather, the lack of it. I have never heard Hevesy laugh. He appreciated good stories and he told innumerable anecdotes, mainly about people from the world of science. But he had no sense for sharing any kind of fun or gaiety. His kind of humor was part irony, part sarcasm; he did not at all like being himself the subject of a good story. Nobody ever dared to make him the butt of a joke. On the other hand, he provided the essence of countless anecdotes about himself owing to his absent-mindedness, his sometimes surprising reactions, and his linguistic confusion. His associates must bear the blame for not having collected and preserved the hilarious Hevesy stories that cheered and amused the first generation of radio-isotopists all over the world.

My part in the intriguing project Hevesy had initiated was fascinating. The building of instruments and later their continuous adaptation to new tasks was entirely left to me under O. R. Frisch's supervision. Since Hevesy continued to travel often and for weeks at a time, he used to outline a plan of research and to provide the necessary samples, for example the rare earths, for me to work with; on his return he expected to find the work done and the results properly written up. This was an excellent education for a fledgling scientist.

Until the summer of 1935 most of our time and effort was devoted to studies of induced radioactivity. The production of radio-phosphorus, P-32, began slowly, since most neutron sources first were used for physics experiments, and later were placed into the large flask with carbon disulphide. Thus, before the production of radio-phosphorus started, the sources had lost considerably in strength. About every fortnight, Hevesy would extract the P-32 formed by chemical means. Nevertheless the work with radio-phosphorus grew steadily in the course of 1936; thereafter it expanded dramatically. Hevesy was impatient, he had plenty of ideas but the availability of neutron sources depended upon the Radium Station in Copenhagen whose doctors used most of the radon for the treatment of cancer patients. New sources were forthcoming only once in a while, sometimes at intervals of a couple of weeks. Hevesy hated to wait. In the summer of 1935 he had an excellent idea: if the institute owned a radium-beryllium source which has a constant strength (Ra having a half-life of 1600 years) it would make the work independent of the deliveries of sources from the Radium Station. Hevesy talked to some influential people and suggested that Bohr on his 50th birthday should be presented with a large sum of money enabling him to purchase one gram of radium. This plan did materialize: funds were raised all over Denmark and 100,000 Kr. were presented to Bohr in October 1935. Two Ra-Be sources were ordered from the firm Radium Belge but they were not ready for use in Copenhagen until the summer of 1936.

Although the quantities of radio-phosphorus Hevesy could extract from the irradiated carbon disulphide were exeedingly small – always less than one microCurie in each portion – he managed to initiate many different investigations. He did not have a laboratory properly equipped for animal experiments nor sufficient knowledge and experience to perform this kind of work on his own. He therefore established contact with at least half a dozen different research centers in Copenhagen, besides the laboratory of animal physiology of August Krogh who from the start had shown special interest in the application of the indicator method to biological problems. As I have already mentioned, Hevesy could be most persuasive, his enthusiasm was irresistable, and he succeeded in winning his colleagues' collaboration. He was, in fact, an excellent "salesman" of his new technique. Very soon he made experiments on plants at the Carlsberg Laboratory, on muscle at the Physiology Department, on teeth at the School of Dentistry, and on membrane permeability with Krogh, to name just a few.

Since these activities were scattered all over town, it necessarily meant carrying active samples and pieces of equipment around, – but Hevesy was not disturbed by such minor technicalities. When the radium-beryllium sources had been delivered to the institute he decided on an experiment that involved the irradiation of wheat seedlings at the Carlsberg Laboratory. Hevesy wrapped one of the radium-beryllium sources in old newspapers and took the streetcar to the west side of the city. He carefully placed his valuable and strongly radioactive parcel on the rear platform of the streetcar and took a seat near the opposite platform, however keeping a watchful eye on his parcel. As he reached his destination, he picked it up and walked the rest of the way. I never learnt whether the source was carried back to the institute via the same route.

But "salesmanship" alone cannot bring about the enormous expansion and within a short time also the general recognition of a new method. Additional ingredients are needed, for example intellectual support and acceptance in the scientific community, co-workers in the laboratory, and – last but not least – funds.

As I have indicated earlier, a number of factors combined to make the expansion of Hevesy's activities possible: Niels Bohr did not consider biology a branch of science remote from his own. Quite the contrary. In 1932 Bohr had given a lecture entitled "Light and Life" before a congress of light therapy in Copenhagen, which was the first carefully worded presentation to deal predominantly with the extension of his complementarity principle into the life sciences. In Bohr's view it was quite natural that there must be room at his institute for a close contact – even collaboration – between physicists and biologists. While Hevesy approached biological problems from a practical viewpoint, that of the experimental biochemist, Bohr was fascinated by the philosophical aspects common to physics and biology and by the "unity of knowledge". Both were eager to pursue the new possibilities which had arisen through the use of radioactive indicators. The backing and encouragement offered by Bohr was essential, but it was not sufficient for Hevesy's dynamic moves. Large scale funding of his project was needed. Fortunately the Rockefeller Foundation was in a process of re-orientation of their general policy in favour of substantial grants for the biological sciences.

A joint effort to raise funds for Hevesy's activities was considered to be of crucial importance. August Krogh, the wellknown Danish physiologist and enthusiastic supporter of Hevesy's plans, enjoyed the high esteem and confidence of the Rockefeller Foundation. Besides, he was the most qualified to evaluate the importance of the indicator method for biological research. Bohr, on the other hand, argued that the production of radioisotopes could be enhanced manyfold if, instead of using radon- (or radium-) beryllium sources, bombardment was performed in a high voltage accelerator or, especially, in a cyclotron like the one that had been built a few years earlier by Ernest Lawrence in Berkeley. Thus, the construction of these machines at the institute would be of immense value for Hevesy and also for the physicists who wanted to study nuclear reactions and transformations by means of accelerated particles.

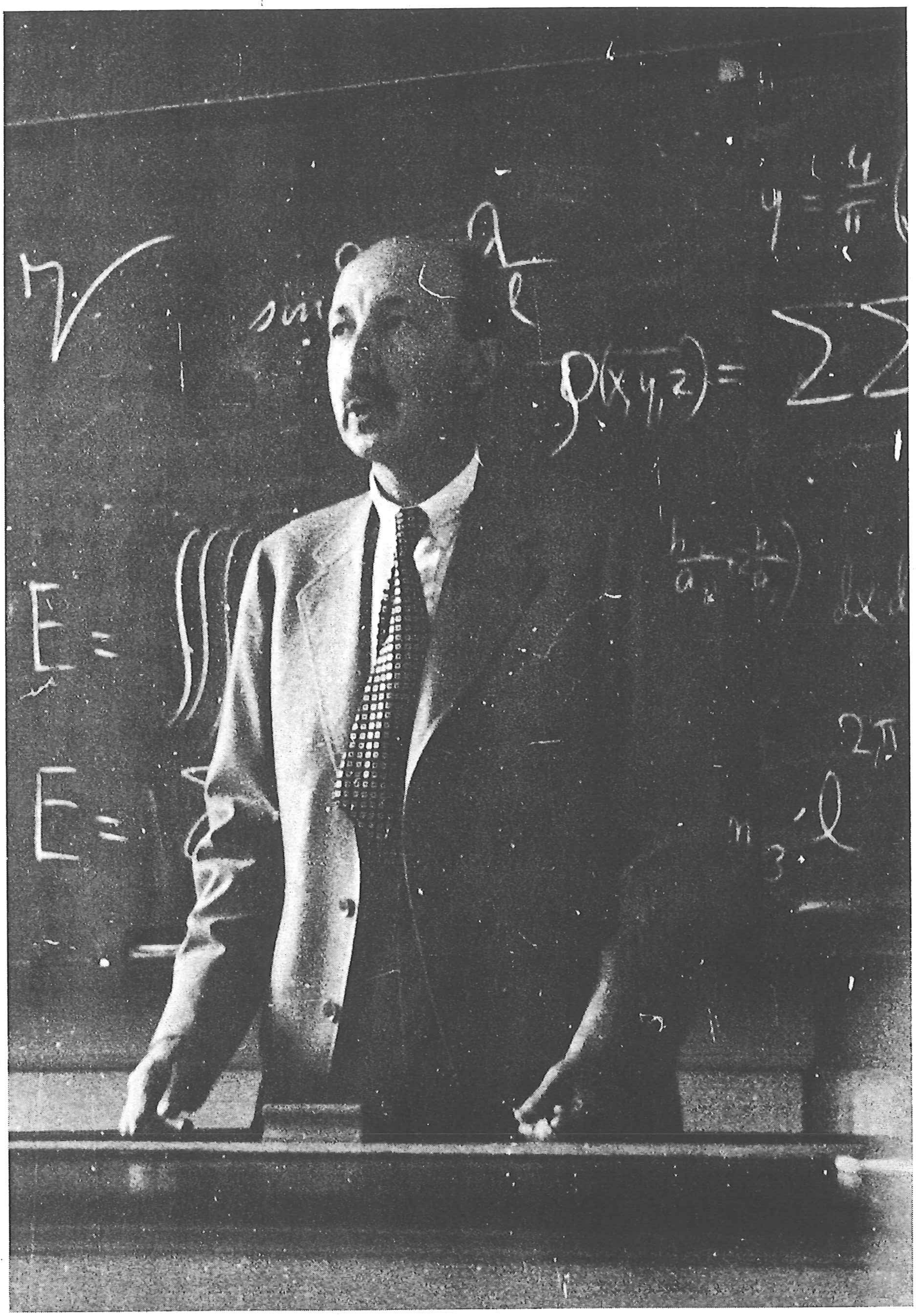
After many consultations, both personal and by letters, with the directors of the Rockefeller Foundation, several versions of the project outline were drafted by Bohr, Hevesy, and Krogh. A detailed description of the project, entitled "physico-biological studies" and an application for funds were submitted to the Foundation. The final document was signed by Bohr alone.

The grant was approved rather promptly and went to Bohr whose prestige in the eyes of the Foundation was the highest. Clearly, Hevesy would direct the work with indicators, Krogh indirectly assured that the research was relevant to biology and contributed to the advancement of the field the Rockefeller Foundation wished to promote. A considerable fraction of the grant was earmarked for the construction of a cyclotron which, in turn, was to be used to a large extent for the production of radio-isotopes for Hevesy and his associates. The rest of the grant secured salaries for two assistants: the first to join the group was the Danish chemist O. Rebbe and the second was L. Hahn from Czechoslovakia; it covered the expenditures for Hevesy's experiments over a period of 5 years.

Now, the conditions were established for Hevesy's research activities during the years that followed. In fact, the Rockefeller Foundation continued to support the tracer work in Copenhagen for many years to come, even after Hevesy had settled permanently in Sweden in the early fifties.

Before I report on Hevesy's work and life during the 5-year period until the German occupation of Denmark in April 1940 it seems worthwhile to return for a moment to the year 1920 when Hevesy left Hungary empty-handed. He had no job and no definite plans for his future. Coming to Copenhagen meant resuming contact with recent progress in atomic physics and being close to Bohr's inspiring and encouraging influence. Hevesy responded immediately and soon was on his way towards new and important investigations. As we have seen, in the early 1920ies he discovered the element hafnium and established one more decisive proof of the validity of Bohr's theory.

In 1934 the situation was different in many respects, but there were also some noteworthy similarities. Hevesy was not empty-handed. In the intervening years he had become a well-known scientist and there were



G. Hevesy lecturing at Bohr's institute ca. 1936. Courtesy: the Niels Bohr Archive.

other openings for him besides Copenhagen – had he been interested. On the other hand, his Freiburg period had not yielded much that was new or scientifically spectacular. Hevesy was on his way to becoming an established professor who followed the traditional pattern of teaching and supervising his students, a situation typical of the German professor beyond his prime. He did not really expect to make another great contribution to science – much less to cause some kind of scientific revolution that would change the direction in which the life sciences developed in the years to come. Rather, he considered himself to be beyond the age of the productive scientist. After his short visit with Bohr in the summer of 1933 he expressed this indirectly to Paneth (25 August 1933) with the words "Ich fand Bohr grossartiger und grösser als je. Die meisten Menschen wachsen nicht mehr wenn sie die Vierziger erreicht haben, Seine fabelhafte Persönlichkeit entwickelte sich immer weiter und weiter" (cf. p. 72) Hevesy's re-union with Bohr triggered a new start. Bohr's excitement about the latest development in nuclear physics and his eagerness to see the Copenhagen group of experimentalists contribute to this field induced Hevesy to perform the experiments with artificial radioactivity of the rare earths. The step from there to the application of radioactive indicators to biological research was not so very large. It is therefore justified to maintain that in the mid thirties the scientific climate at Bohr's institute in Copenhagen again inspired Hevesy to move ahead. There can be little doubt that his achievements during the thirties had a much more profound effect on the development of the sciences than those of the twenties.

From the publication of his first letter to *Nature* in September 1935 until the outbreak of the war in 1939, Hevesy published 25 papers on biological topics besides a dozen general essays on his method and its applicability. As early as December 1936 he mentioned to Paneth that he wanted to write a book on isotopic indicators – both the heavy and the radioactive ones – outlining in detail what this book should contain. Undoubtedly he would have started but for the trouble he and Paneth had with the translation into English and the publication of their textbook on radioactivity which finally appeared in 1938. The book on radioactive indicators had to wait until after the war. During the early years of his biological studies with the aid of isotopes, Hevesy must have lived in a state of elation as he tried to choose between the score of important problems which the indicator method could help to solve. Twelve papers appeared in 1940 and by the end of the war, although his activities were impeded considerably, he had published about 20 more. Even if we take into account that Hevesy usually wrote two or three times about each subject, discussing his results in different languages or at different levels of specialization, the fact remains that he had turned his attention to about 20 topics ranging from general to animal and plant physiology via biochemistry to odontology and medicine. He later moved on to studies of the effect of X-rays on the biochemistry of cancer. This prolificness was not new, he had written just as diligently about hafnium and related subjects. His bibliography lists 50 titles during the first decade (1910-20) and around one hundred during the second (1920-30), not including the books he had published in the course of these years. This kind of statistics is of course superficial and objectionable, it serves no other purpose except to illustrate that Hevesy had no difficulties nor restraints in communicating his scientific ideas to his fellow scientists or, for that matter, to interested laymen. This was in striking contrast to Bohr, who laboured over every sentence and re-wrote each paper many times. It was also in striking contrast to Hevesy's incommunicative attitude towards people outside his scientific circle.

Those were indeed very exciting years. Hevesy's vision about the applicability of radioactive indicators and his vigor in promoting their use is well illustrated by the "international physico-biological conference" held at the institute in Copenhagen in the spring af 1938. Several prominent guests were invited and lectures were given by Joseph and Dorothy Needham (Cambridge), Joseph Parnas (Lwow) and Otto Meyerhof (Heidelberg) besides Krogh and Hevesy himself, – each speaking about their special field of interest. Many foreign and Danish scientists participated in the discussions. It was Hevesy's first exhortation to the international scientific community, drawing attention to this important new method and seeking to bring together researchers with different scientific orientations so they could share their anticipations and experiences. As was to be expected, the 1938 Copenhagen meeting was the first of a great many large and small conferences to be held in the following three decades all over the world. All centered on the use of radio-isotopes and the fantastic progress that was made in following up Hevesy's pioneer work.

A detailed discussion of the wide range of our activities would lead far beyond the scope of this biography; it may even be regarded as unnecessary because Hevesy himself wrote so extensively, also presenting his main topics in general survey articles. Around 1960 he decided to publish a collection in two volumes of what he considered his most important papers. They are arranged in about 10 groups, the headlines of which indicate the gradual change of his interests from the earliest period till the latest, when the tracer method had become a research tool used in many science laboratories all over the world.

The first 4 topics listed under "Life Sciences" in Hevesy's "Adventures in Radioisotope Research" are: Skeleton – Phosphatides – Permeability – Labelled Blood Corpuscles. These titles cover most of the work done in the first 10 years (1935-45) after Hevesy had started to use radiophosphorous. The results of the pioneer experiments – as described on p. 80 – focused on the dynamic processes occurring in bone. Many turnover studies in other organs were made; they confirmed the interpretation Hevesy had suggested from the very beginning. I recall especially the laborious and time consuming work on teeth showing the difference in phosphate uptake between dentine and enamel. The quantities of radiophosphorus at our disposal were so small that the activity found in the enamel of a rat's or a cat's teeth was hardly detectable. We measured these samples relative to the counter's natural background for hours, sometimes for days, in order to make sure that they were different from zero. Here again we have reason to marvel at the patience and persistence with which these studies were carried out although they dangerously approached the limit of what was feasible.

These trying experiments also had their amusing moments: I recall our excitement when the P-32 injected cat suddenly escaped; she jumped out of the window and disappeared in the nearby park. Everybody rushed out to retrieve the precious animal. Several wild beasts were caught and wipe tests of their saliva were placed under a Geiger counter – alas in vain! After hours of chasing, the right cat was found and the experiment could proceed in an orderly manner.

The phosphatide group of investigations centered on the route of formation and the turnover of these organic compounds in the organism. The planning of the experiments required a proper understanding of biochemistry, which Hevesy gained within a surprisingly short time. When the Rockefeller Foundation grant came into effect and he had a chance to hire two assistants, he chose two chemists. Also in this group of investigations we find an example of Hevesy's flair for picking one of the important issues, namely nucleic acid formation. In the late thirties he was able to show by means of P-32 that the turnover of DNA is high in the spleen and in the intestinal mucosa of the rat. He assumed that this is due to the high rate of cell production, or protein metabolism, in these organs.

Towards the end of the thirties a few foreign visitors joined the group, some coming from the United States. Our own laboratories were then populated with a variety of experimental animals which the group had learned to take care of without assistance from other laboratories. Our favorits were rabbits which were easier to handle than mice and rats. These peaceful animals are at the heart of one of the "Hevesy classics" told over and over again: one late afternoon Hevesy came rushing down the stairs – obviously he was late, as usual, for some appointment. As he met his assistant on the stairway he exclaimed "Herr Hahn, Herr Hahn, you are lucky, you have your rabbits, – I must go home to my family". This story must not be misunderstood: Hevesy was very fond of his family, his youngest daughter, Pia, had just been born, – but nevertheless, he was a little envious that Herr Hahn could stay in the laboratory through most of the night.

The papers collected under the heading "Permeability" illustrate even better how an entirely new field is opened up as a new technique becomes available. Permeable or semi-permeable membranes play a very important role in living organisms. Every single cell is contained in a membrane. Until Hevesy developed the indicator method, it had not been possible to study in detail the movements of ions and molecules across these membranes. It is therefore easy to imagine that permeability studies – in the widest sense of the term – were in the center of interest after the introduction of isotopic indicators. Krogh and his pupils, first and foremost Hans H. Ussing, were especially interested in membrane permeability. They frequently used the skin of a frog, a membrane which regulates the passage of water, of salts, and also of larger molecules both inward and outward between the organism and the surrounding water. Krogh and Hevesy began their collaboration immediately after Hevesy's arrival in Copenhagen using heavy water, but Krogh recognized early that the usefulness of radioactive isotopes would far exceed that of deuterium as an indicator.

Also the membrane surrounding red blood corpuscles is permeable to many substances, and Hevesy showed that blood corpuscles take up and incorporate labelled phosphate both when it is injected into the organism and when corpuscles are incubated with radioactive phosphate outside the organism. If corpuscles labelled in vitro are re-injected into the blood stream, they will immediately mix with the entire quantity of circulating blood. From the degree of dilution of labelled with unlabelled corpuscles, the total blood volume can be calculated. This study, more than any other investigation Hevesy published, aroused the interest of medical people; they soon realized that the application of the indicator method would not be limited to basic research and would thereby benefit the medical sciences. Clearly, the usefulness of the method in diagnosing disease was anticipated at an early stage. All that was needed to introduce the indicator technique in the hospitals was larger quantities of radioactive material. To this end the construction of the high voltage machine and the cyclotron was pursued energetically at the institute. The first neutron bombardments in the cyclotron to produce radio-phosphorus (1938) yielded samples of moderate strength and purity, – not enough to satisfy Hevesy's needs. So he decided to inquire whether his friend Lawrence in Berkeley, the inventor of the cyclotron, could spare a little radio-phosphorus. Lawrence was most accommodating and shipped by airmail letter a small quantity of a white powder – sodium phosphate – the activity of which was about one milliCurie, a thousand times more than the one microCurie we had been able to make with the radon-beryllium sources. Hevesy was jubilant! Here finally, he had enough P-32 to run all the experiments he had in mind, and he could use a higher activity in each, so that the measurements required less time. It cannot come as a surprise that the number of problems to be investigated and the number of papers to be written increased a great deal. Fortunately, also the group of co-workers grew. The airmail letter shipments from Berkeley mostly handled by Martin D. Kamen continued till the outbreak of the war made this impossible.

The year 1939 brought more unrest and tension, the threat of war was imminent. Slowly, our guests from abroad decided to return home; contact with colleagues in other countries became increasingly difficult. For a short period, Hevesy resumed work with heavy isotopes. Urey had succeeded in preparing the "heavy" oxygen and "heavy" nitrogen isotopes from the naturally occurring mixture of these elements' different isotopes. He made some of this material available for Hevesy who used it immediately for experiments on corn. But Hevesy did not have the equipment for analyzing the samples and started to collect information on how and where to buy a mass spectrometer. For a period of some months the tripartite collaboration between Urey in Chicago, Rudolf Schönheimer at Columbia in New York, and Hevesy was established, and Schönheimer went out of his way to help Hevesy with these analyses. The German occupation of Denmark and Schönheimer's tragic death put an end to these activities.

Even in the fall of 1939 Hevesy appeared unaffected by the turmoil around him; he continued to display the same detached attitude – nobody had any notion that he was deeply disturbed. However, I found a remark in his correspondence with Paneth (14 July 1936) which shows that Hevesy did worry. He wrote "Recently I have pondered repeatedly whether – out of regard for my children – it would have been better if I had settled in the United States and left Europe and her unfriendly tendencies." The letter continues "But, after all, we cannot foresee the future, and my children are of half Danish descent. Whether they will have very great difficulties later – I am not quite convinced."* This is the only mention I

**14 July 1936 to Paneth*
"Ich habe mir in der letzten Zeit wiederholt überlegt ob es – mit Rücksicht auf meine Kinder nicht richtiger gewesen wäre mich in den USA niederzulassen und Europa und ihre unfreundlichen Strömungen zu verlassen. Aber schliesslich kann man die Zukunft doch nicht voraussehen und meine Kinder sind zur Hälfte dänischer Abstammung. Ob sie später sehr grosse Schwierigkeiten haben werden, davon bin ich nicht ganz überzeugt".

have found of the word "Abstammung" in relation to his own family. Did he fear that – in case Germany came to dominate Denmark – his children might be in trouble because of their "Jewish" father?

Among the documents from the thirties I also found several letters Hevesy wrote in order to help Stefan Meyer whose future was of real concern to him. The Nazis had dismissed him from his position as the head of the Vienna Radium Institute, forbidden him to enter the laboratories and confiscated his savings. He moved to his house in the country where he lived rather isolated and with modest means. Hevesy tried hard and impatiently to find a position abroad for Stefan Meyer, turning to several influential friends and acquaintances for help, but without success. It seems that Stefan Meyer was not too eager to leave his country; his age and his very poor hearing made him feel insecure about being uprooted. Fortunately, he survived all hardships during the war. Hevesy re-established contact with him as soon as this became possible. They did not meet again, but Hevesy repeatedly expressed in writing his fond memories of the Vienna period and his gratitude to the "great old man" of the Vienna Radium Institute and the famous "Vienna school".

V. M. Goldschmidt also caused Hevesy great concern. Following their close collaboration in the twenties on the hafnium content of various minerals and related topics, they had remained in close contact. While Hevesy was professor in Freiburg, Goldschmidt had accepted a professorship in Göttingen (1929) where he stayed until the Nazi regime forced him to leave. Before he could regain his Norwegian citizenship and his position at the University of Oslo, Goldschmidt encountered many personal and economic problems and Hevesy tried to help. A few years later, Goldschmidt again became a victim of the Nazis in Norway, but he managed to escape to England. He decided to return to Oslo after the war, but he was a sick, unhappy man and he died from heart failure in 1947. Ever since they had met early in their careers, Hevesy had admired Goldschmidt for his almost boundless energy and his profound knowledge in geochemistry; he felt sympathy for a man who, time and again, was unhappy and on bad terms with his colleagues.

In the summer of 1939, the Rockefeller Foundation invited Hevesy to the United States, suggesting that he visit – as a consultant – a number of

American laboratories where isotopic indicators were being introduced. The trip was to take place early in 1940. He wrote about this to Paneth (22 July 1939) and in the same letter, he mentioned an offer he had received (confidentially) from India. He remarked regretfully that he would have preferred an offer from Canada, saying that "Consideration of the world-political situation could bring me to leave Denmark, but India – no."* Such a move would almost certainly have been a serious threat to his health.

Again – as in previous years – Hevesy kept his worries to himself and played the role of the well-balanced, but also good humoured fatalist. But after the German occupation of Denmark on April 9, 1940, even Hevesy was driven close to despair. He did not show it, but more than 40 years later, the correspondence between Hevesy and Urey came to my attention, and I then learnt that Hevesy had made desperate attempts at securing passage to the United States for himself and his wife. From America, both Urey and the Rockefeller Foundation sent him new invitations to various conferences and lecture tours, which should have served as a suitable pretext to bring him over. To Hevesy's great dismay all his efforts to obtain passage on a boat or on the trans-Siberian railroad failed. He was trapped in Denmark – at least for some time. Now it is impossible to find out whether or not Hevesy spoke to Bohr about his fears and his intention to leave Denmark for the U.S. If he did, Bohr never mentioned their conversation to anybody now alive.

Until the summer of 1943, the situation in Denmark, the day-by-day life of ordinary people, remained almost unchanged and peaceful, especially as compared to the life of people in some other occupied countries, such as Norway or the Netherlands. But then, the political climate changed drastically, and in the fall the action planned by the Nazis against the Jews in Denmark was revealed through indirect diplomatic channels. This provoked the well-known and widely documented rescue of the Danish Jews by a united Danish population. In the course of 2-3 weeks a few thousand people were brought to safety across the Sound, the narrow

**22 July 1939 to Paneth*

"Rücksichten auf die weltpolitische Lage würde mich dazu bringen, Dänemark zu verlassen, aber Indien – nein".

strait which separates Denmark from Sweden. Warnings that Bohr and his family were in great danger came at the same time. Not primarily because Bohr was half-Jewish and an atomic scientist, but rather because the Germans knew that he was an ardent and influential opponent of the Nazi regime. Bohr was strongly urged by his friends and advisers to give up his stern refusal to leave his country – which he had insisted upon until this moment. He was brought to Sweden the last day of September 1943 and a few days later he was flown to England.

I was helped to safety across the Sound and was immediately offered work and a modest emergency salary at the University of Stockholm. During the weeks that followed, many Swedish scientists were actively engaged in helping their Danish colleagues by preparing for their employment even before some of the Danes had actually arrived. To the best of my knowledge, nobody expected that Hevesy might feel the threat of prosecution, he had never shown his concern. However, in mid-October, Hevesy appeared in Stockholm. Since he had an Hungarian passport, he simply boarded a train in Copenhagen. He came alone and without luggage but he had no intention to go back. Just as in the thirties he did not say a word about his personal situation.

Hevesy did not need the assistance of various committees and organizations which had been formed in order to help the refugees from Denmark who came without their belongings and most of them without money. He had his close friend Hans v. Euler with whom he had conducted experiments on Jensen sarcomas in rats since the beginning of 1941. For almost three years, letters and samples had been sent back and forth every week. Since early in 1943 every single letter had been opened by the censor. Certainly, Hevesy found this kind of collaboration cumbersome and preferred to join Euler at his Institute for Organic Chemistry in Stockholm. It was less fortunate that his "lieber und sehr verehrter Freund" – as they addressed each other in writing – had shown great sympathy for the German Reich. Both within and outside university circles many resented v. Euler's openly pro-German orientation. I have stated earlier that Hevesy was not interested in a colleague's political views as long as scientific co-operation was desirable and productive. His close affiliation with v. Euler illustrates this point.

Although Hevesy worked at the Institute of Organic Chemistry and I was at the Wennergren Institute of Experimental Biology, we met frequently. Hevesy's family arrived eventually, and they settled in some provisional living quarters, later in a beautiful apartment. Just as he had done in Copenhagen, Hevesy soon established contact and collaboration with several laboratories which he visited regularly although, even in Sweden, the war situation slowed down all scientific activities. In the Swedish scientific community Hevesy was met with the respect and friendliness he deserved. Radioactive indicators had made him very well known first of all among all who worked in the life sciences and basic medical research.

In the fall of 1944 the news that Hevesy had been awarded the Nobel Prize in chemistry for the year 1943 was received with great joy and satisfaction by everybody everywhere. At last, Hevesy had been found deserving of this prize which is widely considered to be the highest distinction that can be bestowed on a scientist. Hevesy was pleased although he did not seem to attach too much importance to this event. As mentioned earlier, it is my impression that he had not forgotten his disappointment with the Swedish Academy of Science who failed to honor him for the discovery of hafnium. The prize was also a welcome support at a time of economic uncertainties. Moreover, every laureate has the option to become a Swedish citizen. In rare cases only is this offer of real interest to a laureate, but Hevesy did accept it; he exchanged his Hungarian passport for a Swedish one.