Bad Luck in Attempts

by C. J. Gorter

People who get prizes are usually expected to tell the assembled dinner guests about the research that won them recognition. The recipient of the Fifth Fritz London Award chose instead to describe how he had just missed making a number of very important discoveries.

WHEN THE Committee for the Fifth Fritz London Award asked me to give my recipient's address a somewhat personal nature, I hesitated about how to combine the modest review that I could give of certain recent advances made in the Kamerlingh Onnes Laboratory with this request. Not finding a satisfactory solution, I remembered that from time to time I am asked why I have just missed making certain discoveries. Since I have received a favorable reaction from the committee chairman, Dean Boorse, to my enquiry about whether it would be admissible to drop all discussion of recent work and to speak only on apparent bad luck in my attempts to make certain scientific discoveries, I shall do so in the hope of satisfying once and for all those who have asked or would like to ask me about these matters. I shall talk mainly on attempts to observe nuclear and electron magnetic resonances, gamma anisotropy after orienting atomic nuclei, anisotropy of beta emission, and flux quantization in superconductors.

As a Leiden student I was strongly influenced by our professor of theoretical physics Paul Ehrenfest, whose spontaneous and sagacious personality exerted just as strong an attraction on his young students as it did on the great scientists of his time. (In senior students and average scientists he showed less interest.) And he repudiated those theorists who, without showing much real understanding, liked to deal in formal statements and untransparent mathematical techniques. He had a slightly aggressive and very lucid way of formulating objections and questions that I seemed to recognize many years later in Lev Landau's way of presiding over his Moscow colloquium.

Rf magnetic fields in physics

To Ehrenfest I owe two suggestions which guided the choice of the first scientific work I did after completing my thesis[1] on low-temperature paramagnetism in 1932 under the supervision of W. J. De Haas. W. Lenz and Ehrenfest[2] had pointed out in 1920 that interaction with thermal motion is essential to obtain the Curie-Langevin magnetization in a paramagnetic substance. This meant that relaxation phenomena should occur, which Gregory Breit[3] tried in vain to detect with the insufficient means at his disposal in 1926 during his Leiden time. After a visit to an industrial laboratory Ehrenfest once exclaimed to me that, although he understood hardly anything of the wonderful techniques being developed in the radio industry, he felt that such techniques might become of great benefit to pure scientific research. In a proposition accompanying my thesis[1] I advocated carrying out spectroscopic research with short radio waves, recommending the 2s-2p structure of hydrogen and the use of a magnetic field.

In A. D. Fokker's laboratory at Teyler's Foundation in Haarlem I then started to learn about radio techniques. But in my first attempt to detect paramagnetic relaxation I tried merely to observe the mechanical couple acting on a paramagnetic substance in a low-frequency rotating magnetic field. Having obtained a doubtful result, I decided that a gas thermometer, indicating the heat developed under the influence of a strong radio-frequency field, might be a better tool, in particular at low temperatures where susceptibilities are high and heat capacities low. De Haas invited me to carry out the experiment in Leiden, where it met with immediate success.[4]

Nuclear magnetic resonance

I also studied the influence of an external magnetic field, and that brought me back to my proposition[1] of 1932. A primitive but sensitive foil manometer was constructed, and I tried to observe a sudden rise in the temperature of the sample upon very slowly varying a transverse magnetic field. The substances were lithium fluoride and potassium alum, and I searched in the radio-frequency region where nuclear magnetic resonance of lithium-seven and hydrogen could be expected. In the short paper in which I announced

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the, negative, result, I stated that in the case of resonance the occupation of the higher levels had been obviously increased, the corresponding increase of spin temperature by a factor of at least one hundred having cancelled the expected effect.

In the quiet atmosphere of Haarlem I had, following the example of De Haas, carried out without success several simple but rather fantastic experiments, including attempts to detect nonlinear optics with concentrated sunlight, to detect a universally present neutron gas, to observe an electronic Raman effect, to concentrate the heavy component in water by biological means, etc. In the mean time I pondered simple theoretical problems with H. B. G. Casimir,[5] Fokker,[6] E. C. Wiersma[7] and L. Nordheim.[8] But in the long run the modest facilities and technical assistance of Teyler's Laboratory did not match my ambitions. I appear to have almost been appointed by Ernest Rutherford to share Peter Kapitza's former position in Cambridge with Rudolf E. Peierls, but the decision finally went in favor of Jack F. Allen.

I was then appointed reader at the University of Groningen, where F. Brons and I completed the discovery of paramagnetic relaxation by investigating also the real component of the paramagnetic radio-frequency susceptibility and thus studying paramagnetic dispersion.[9] We then realized the importance of the excellent theoretical analysis[10] of the paramagnetic relaxations which I. Waller had already given in 1932 and which was then extended by my colleague and friend Ralph Kronig[11]—who was a most helpful advisor in our work—as well as by J. H. Van Vleck and others.

The plan to observe magnetic resonances had not been given up, and with that plan in mind, I spent the summer of 1937 in the USA. I had the choice between Columbia University—where I. I. Rabi had developed a refined molecular-beam technique that might make it possible to avoid the increase of spin temperatures and thus the compensation of absorption by stimulated emission that had been fatal for the Leiden nuclear-magnetic-resonance experiment—and the University of Michigan where C. E. Clee-

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Presentation of the Fifth Fritz London Award

by Henry A. Boorse

IT SEEMS especially fitting that the opening session of the Tenth International Conference on Low Temperature Physics should include the Fritz London Award. For the Award has served the dual purpose of commemorating the debt which low temperature physics owes to one of its great founders, Fritz London, and of honoring one of our contemporaries for his distinguished research. All of us who were privileged to know Fritz London remember his warm, friendly personality and all of us know the legacy he left us from his deep insight into the problems of superfluidity, whether in the domain of superconductivity or liquid helium.

Today, as chairman of the Committee for the Fifth Fritz London Award, I am acting on behalf of my fellow Committee members, Ernest Ambler, US National Bureau of Standards; Henry A. Fairbank, Duke University; William A. Little, Stanford University; Paul M. Marcus, International Business Machines Corporation; and Darrell W. Osborne, Argonne National Laboratory. I am acting also on behalf of the Union Carbide Corporation which, through its generosity and public spirit, has contributed the funds to make this Fifth Award possible. Because the previous awards have not been so numerous as to preclude naming the recipients, may I remind you who they have been: Nicholas Kurti, F.R.S., Lev Davidovich Landau, John Bardeen, and David Shoenberg, F.R.S. To add to this illustrious company, the Award Committee now presents the fifth recipient: C. J. Gorter, director of the Kamerlingh Onnes Laboratory of Leiden University.

Many of you know Professor Gorter personally and those who do not have certainly read some of his papers. I say "some" advisedly because the breadth of his contributions to low temperature physics may well surprise all but the most initiated. As one of our Committee members commented: "If there were a gold medal for a scientific decathlon, Gorter would be an easy winner." But, before I touch briefly on his contributions, let me say something about him personally. He was born in Utrecht, and his university training and doctor's degree were obtained at Leiden, the latter under W. J. de

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Haas. In those early years, he was also interested in astronomy but, after he was rained out on an eclipse expedition in Norway, his enthusiasm seems to have been considerably dampened. However, in 1929 he tried again as a member of an expedition to the Dutch East Indies. This was more fortunate, and although I know nothing of any further attempt to pursue heavenly events, the earthly urge to travel has remained with him ever since. Besides an enthusiasm for traveling, he has long been an enthusiastic sportsman, and even a broken ankle has not prevented him from risking a limb by re-

ton and N. H. Williams[12] had opened the field of microwave spectroscopy by observing the absorption of ammonia gas, which might present a starting point for the detection of electron spin resonance. I would have been welcome at both universities and chose Ann Arbor, where a well known summer school also offered possibilities for experimental work. This proved to be the wrong choice, since a secretary had forgotten to inform Professor Williams of my intention to acquaint myself with his microwave technique. He received me kindly but then departed for his vacation. No microwave work had been done recently, and the kind help of a part-time technician was of little use to me. So I spent a very interesting time at the summer school and particularly enjoyed lectures by Enrico Fermi, George E. Uhlenbeck and Luis Alvarez but learnt hardly anything about microwaves. On the way home—by Greyhound bus and ship—I visited Columbia University. One of Rabi's collaborators showed me the details of the various atomic beam apparatuses. Seeing the set-up in which the beam passed through a magnetic field, the direction of which rotated in space so that the sign and approximate magnitude of the magnetic moments of the nuclei could be evaluated, I realized that replacing this magnetic field by a constant plus a transverse radio-frequency field would make the apparatus immediately suitable for the observation of nuclear magnetic resonance. I then asked to see Rabi, whom I had earlier shown around the Leiden laboratory, and suggested to him the introduction of a radio-frequency magnetic field into his apparatus, showing by a simple calculation that a small oscillator would be sufficient to obtain a strong depolarization of his beam. I did not succeed in convincing him of the advantages of my proposal over his constant field rotating in space, but he promised to consider the matter at his ease. I understood that he intended to visit us soon in Holland and would then continue the conversation. But a few months later I could congratulate him and his collaborators on the discovery of nuclear magnetic resonance announced in a Letter to the Editor of *The Physical Review*.[13] I cannot deny that I felt some pride, mixed with the feeling that my contribution had been somewhat undervalued though my advice was acknowledged in the Letter. I realized quite well, however, that it would have cost us years to set up the adequate equipment in our small group at Groningen. Some time afterward in California, Alvarez and Felix Bloch who apparently had arrived at the same ideas independently,[14] measured the magnetic spins of neutrons by passing a beam of them through a similar transverse radio-frequency magnetic field.

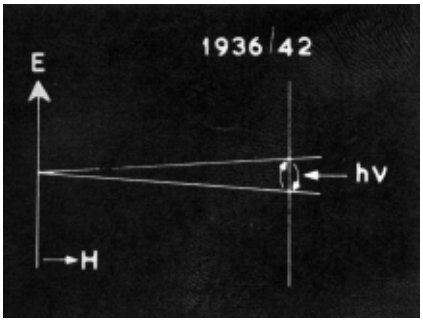
turning to the Alps each winter to ski.

While a Conservator at Teyler's Institute in Haarlem from 1931 to 1936, he began his career with a series of very fundamental papers on the thermodynamics of superconductivity, and was the first to treat superconductivity as a reversible phase transition, a treatment which foreshadowed the Meissner effect. Soon after, with H. B. G. Casimir, he developed the two-fluid model of superconductivity, introducing the notion of an order parameter that has played such a prominent role in the description of superconducting behavior. From 1936 to 1940, he was a lecturer at the University of Groningen and in this period, returning to his thesis interest in paramagnetism, he discovered paramagnetic relaxation and was the first person to try to observe nuclear magnetic resonance in solids. In retrospect, it seems clear that only an unfortunate choice of sample prevented the success of this very important experiment. These early ideas on resonance techniques came to fruition later in experiments on molecular beams by I. I. Rabi and his collaborators and through the successful demonstration of nuclear magnetic resonance by Felix Bloch and by Purcell. From 1940 to 1946, Gorter was professor at Amsterdam and director of the Zeeman Laboratory. In the latter year, he was called to Leiden to succeed to the directorship of the Kamerlingh Onnes Laboratory, a position he still holds with great distinction. In his first years as director, much of his time and energies were required for reorganizing and reëquipping the Laboratory, but soon new papers appeared extending the two-fluid model for He II, and I note especially his paper on the concept of mutual friction, so essential to the description of this strange liquid. I have not described his research on spin-lattice or spin-spin relaxation times nor on the alums and their use in obtaining very low temperatures nor his contribution to the realization of nuclear alignment (the Gorter-Rose method) nor his investigations in antiferromagnetism nor any results from the long list of his subsequent papers. Nevertheless, from this enumeration you can form some idea of the range of his interest and the fundamental nature of his contributions.

Long ago the English historian George Macauley Trevelyan said, "A man and what he loves and builds have but a day and then disappear; nature cares not and renews the annual round untired." It is the part of human nature to care and in this ceremony we honor the spirit and the contributions which Gorter's love of low temperature physics has built, not for a day, but for many years to come.

Professor Gorter, it is my great pleasure and privilege to proceed now with the Fritz London Award. I hand you first a check for $1000, the monetary part of the prize, and next the certificate which bears the signature of J. F. Allen, secretary of the Commission for Very Low Temperature Physics, International Union of Pure and Applied Physics; Kenneth Rush, president of the Union Carbide Corporation, the source of the funds and the certificate; P. L. Kapitza, chairman of the Organizing Committee of the Tenth International Conference on Low Temperature Physics; and finally, my own as chairman of the Award Committee. ■

I did not take up the problem again until I was appointed as Pieter Zeeman's successor at the University of Amsterdam and had built up a small but able research team there in spite of the war and German occupation which did not make my Amsterdam years a particularly agreeable period. In 1942 I profited anew from Leiden's hospitality, and L. J. F. Broer and I transported the most stable of our paramagnetic dispersion equipment there. The idea was to observe the frequency shift of an LC oscillator as it slowly approached the nuclear-magnetic-resonance line so that no energy absorption would equalize the occupations of the higher and lower energy levels. The results of a few days' observations were again negative[15] for lithium-seven in lithium chloride and fluorine-19 in potassium fluoride, although we sometimes saw apparently irreproducible irregularities in our frequency. Several years later we understood that we had used too pure chemicals and therefore had still too long relaxation times. Nicolaas Bloembergen then measured the relaxation time of the lithium-seven spins in our

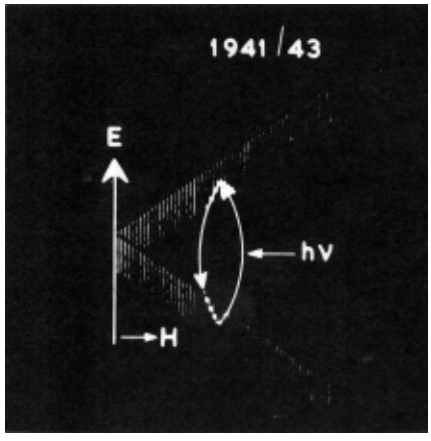


sample and in fact found several minutes at liquid-helium temperatures.[16]

The inconveniences of traveling up and down in war time and lack of space in Leiden contributed to our decision not to continue this research then.

Electron spin resonance

Around the same time (1942) L. J. Dijkstra and J. Volger made several attempts in the Zeeman Laboratory to discover electron spin resonance in paramagnetics at much higher frequencies.[17] In the usual nondiluted hydrated paramagnetics, dipole-dipole interaction should cause a width of the resonance line of the order of 200 oersteds, corresponding to about 600 MHz. Volger's paramagnetic relaxation equipment went up to 80 MHz, which was much too low; though, looking later at the data, we could imagine we saw a flat maximum at low perpendicular fields in some of them.



Cornelis Bakker, then at the Philips Laboratory in Eindhoven, secretly placed a small 10-cm klystron installation at our disposal, with which Dijkstra and Volger carried out some attempts to observe a maximum of magnetic absorption at higher fields. But the experimental technique was primitive, and the three klystrons we received had an average lifespan of about half an hour each. When later electron spin resonance in solids had been clearly detected in the Soviet Union, we again imagined we could see some indications of weak maxima in some of our early experimental data. E. K. Zavoisky[18] carried out extensive investigations in Kazan on paramagnetic relaxation with a very sensitive set-up and then, encouraged by J. H. Frenkel,[19] went to considerably higher frequencies in order to search for spin resonance[20] at a frequency of 133 MHz at flat maximum of the absorption in a perpendicular field of about 40 oersteds in $CuCl_2 \cdot 2H_2O$. Frequency and field were much lower than what corresponds to the dipole-dipole interaction in this salt. It was later understood that observation had been made possible by narrowing of the resonance line due to exchange interaction.[21] Zavoisky soon confirmed the identification of his maximum with electron spin resonance by moving up to higher frequencies.

The discovery in 1946 of nuclear magnetic resonance in solids and liquids by the groups of Bloch and of E. M. Purcell is well known.[22] They worked at room temperature and thus profited from short relaxation times. They also had modern electronic equipment.

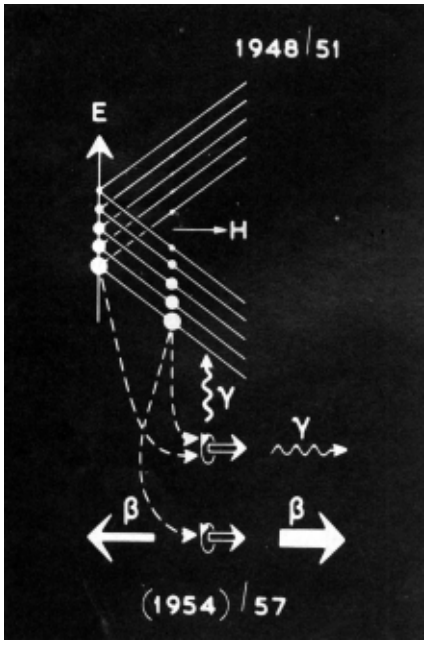
In 1934 during the discussion after a lecture by P. Debije,[23] I suggested orienting atomic nuclei in order subsequently to obtain much lower temperatures by their adiabatic demagnetization than could be reached by means of electron spins. Nicholas Kurti and Francis Simon[24] made a similar suggestion, and Simon[25] in 1939 gave a valuable analysis of the various possibilities.

It was in the winter of 1944–45, when experimental work was no longer possible in Amsterdam, that I noted while studying a table of copper spectra, that considerable hyperfine structures occur in states that do not have a single electron in an s shell. This indicated that also the 3d electrons, and probably also 4f electrons, in paramagnetic ions could display a considerable interaction with the atomic nuclei, corresponding to fields of several hundred thousand oersteds at the nucleus. This interaction would be suitable to orient the nuclei at low temperatures. Of course, one should retain a modest external field to orient the electron spins, and adiabatic demagnetization of the nuclei to reduce the temperature would not be possible. I intended to test this idea after 1945 in my own country, which was then lagging behind in all respects, and obtained agreement and support from several colleagues. In coöperation with nuclear physicists from Groningen a long series of experiments was set up in which, among others, M. J. Steenland and O. J. Poppema and later W. J. Huiskamp and H. Postma played important parts. But the start was slow, and I mentioned the basic idea in 1948 at the Paris conference in commemoration of Jean Perrin and Paul Langevin.[26] M. E. Rose independently proposed the same idea.[27]

Anisotropy of gamma rays

In Leiden some very doubtful results were obtained about anisotropy of gamma rays emitted by iron-59 and about the absorption of neutrons by gadolinium and samarium.[28] It was no

secret that the Oxford School was working along the same line, backed up by the outstanding work of B. Bleaney, H. M. L. Pryce and their collaborators on electron spin resonance in crystals and its interpretation. The hyperfine structure of that resonance was discovered in Leiden by a guest from Oxford, R. P. Penrose[29] whose unexpected and early death meant a great loss to all of us. That discovery played an important role in the further developments, and at the second International Low Temperature Conference at Oxford in 1951 it became clear



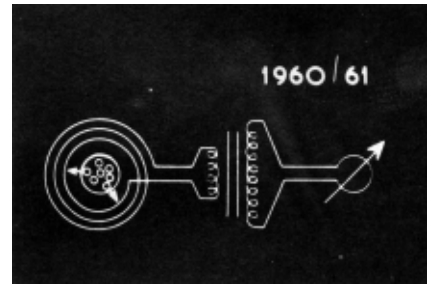
that diluted Tutton-salt crystals containing some radioactive cobalt were favored. The Oxford group[30] observed a clear anisotropy of cobalt-60 gamma rays a few weeks before the Leiden group.[31] Bleaney's recommendation that, in view of the crystalline electric fields and the central symmetry of the gamma ray emission, it would not be necessary at all to retain an external magnetic field, was fully confirmed.[32] In the following years many further observations on oriented nuclei were carried out.

Anisotropy of beta emission

Both Oxford and Leiden missed the boat with respect to the anisotropy of beta emission. In contrast to gamma rays, beta particles cannot easily pass the walls of a cryostat. When I mentioned this difficulty in 1952 in a seminar talk in Berkeley a colleague, whose identity I have not been able to establish, suggested that for positron emission this difficulty could easily be circumvented by observing the annihilation gamma rays of the positrons. When in 1954 a very able young foreign physicist who intended to work for a year in Leiden, asked me for a research subject, preferably in our adiabatic demagnetization group, I proposed to him that he try to observe the anisotropy of positrons emitted by oriented nuclei, making use of the coincidence of the two annihilation gamma rays. He wished to take theoretical advice first, and then raised well formulated objections based on the expectation that the intensity of allowed beta radiation in opposite directions would be equal, while, for the few positron emitters which could be oriented, anisotropic forbidden radiations would be very feeble. When I insisted, reminding him of the discovery of the Zeeman effect in 1896 in spite of H. A. Lorentz's conclusion from the known e/m ratios that the expected separations would be too small to be observable, he simply retorted: "I refuse to waste my time in useless experiments." He then chose to carry out another and quite interesting investigation but he had missed the youthful acquisition of fame, as became clear when T. D. Lee and C. N. Yang[33] two years later demonstrated that the assumption that parity must be conserved in allowed beta emissions, which therefore should be equal in intensity in opposite directions, was not founded. We then carried out the experiment on cobalt-58 positrons with the help of Groningen nuclear physicists in a few weeks' time and observed a considerable anisotropy.[34] But when we reached that result we had already learnt from the newspapers about the much more difficult negatron experiment of P. W. Ambler and R. P. Hudson's Washington group which had started ahead of us by several months as a consequence of C. S. Wu's swift and efficient insight, and the rapid communications between New York and Washington.[35] The young foreign physicist, of whom I have spoken, very fairly assured me that during the rest of his life he would carry out any experiment that I recommended to him. I still hope to be able to use this privilege once by making him a worthy proposal. I later heard by rumor that in Oxford theoretical advice had also stopped early search for beta anisotropy.

Flux quantization

In 1948 Fritz London[36] predicted quantization of the fluxoid encircled by a superconductor. It is not clear why it took more than ten years before several—I know of no fewer than five—experiments were simultaneously carried out to test this conjecture. It may have been the success of Lars Onsager and Richard P. Feynman's[37] hypothesis of quantized vortices in helium II or the penetration of A. A. Abrikosov's elaboration[38] of the Ginsburg-Landau description of superconductivity, which finally awoke us. Perhaps it was also the growing admiration for London's far-reaching vision and intuition. In any case, at my suggestion, an investigation was also set up in Leiden. Part of the superconductive chopper-amplifier set-up of A. R. De Vroomen and C. Van Baarle for the study of small low temperature thermolectric voltages[39] was used. A superconducting coil arounding a thin wire placed in a constant longitudinal magnetic field was connected with this electric transforming and measuring equipment. The tin wire was very slowly warmed up to the transition temperature and the escape of frozen-in flux was followed. Flux jumps of the order hc/e were observed, but noise and irregularities proved to be much worse than expected. We had not progressed so far as to replace the wire by a more appropriate ring before the Stanford and München[40] groups announced their result of a flux quantum half as large as was predicted by London. The measuring equipment was then returned to the study of thermoelectricity.



Why the misses?

I shall not ask your attention now for small failures and modest successes in

my scientific work but rather stick to the five missed discoveries and ask merely whether any conclusions can be drawn or lessons learned. But first I should state that in my short exposition I have relied upon my own memory and upon the scarce publications. It may very well be that I have placed accents at the wrong places and underestimated or even neglected essential aspects, achievements and merits. If so, I wish to apologize. And second, I would like to remark that the importance of the problem should not be overestimated. The international progress of physics is rarely retarded by one man's missing a few discoveries.

The remaining question is: should the missing of those various discoveries by one person be blamed on his character, limited abilities, education, methods of work, surroundings, or collaborators, or just ascribed to bad luck?

I am not qualified to give the right answer to this question. I realize that my scientific ability and active interests lie between those of a theorist and an experimenter, and that this ability is not so broad that I could be either an acceptable mathematical physicist or an acceptable engineer. I like to concentrate on making very simple calculations and suggesting simple models and experiments. This narrows down my scientific potentiality. And though I do not easily drop a problem entirely, I cannot concentrate on it completely for a long time. I have also been bothered—and honored—more than the average scientist by time-consuming tasks of management, organization, and representation. Thus, at the age of 32 I was given the direction of quite a well known research laboratory and I have been chairman of various bodies and committees. Feeling myself an outspoken internationalist, I have also been active in several international organizations, and I spend much time in other countries. I do like that, of course, and I also expect and hope that the scientific community will develop into a really international community, and I want to contribute to that advance where I can. Lastly, I loathe pressing people to do things they object against.

I have now indicated several factors that may have prevented me from carrying out some of my main tasks. It would therefore not be difficult to specify where I made omissions in the five cases or did not prevent others from making them. But nonetheless I would not quite delete the factor of bad luck. It would for that matter be improbable if good and bad luck were equally distributed among all scientists. There must be fluctuations, and four cases of bad luck are no protection at all against a fifth case. Personally, I can be happy that for me bad luck has so far not extended to other fields of life, and I am thankful too, that the missing of certain discoveries has not prevented the Committee for the Fritz London Award from doing me the great honor and pleasure it has done me. □

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