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OTTO STERN

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A Biographical Memoir by

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O. Stern

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BY EMILIO SEGRÈ

OTTO STERN was born to a family of prosperous German Jewish millers and grain merchants on February 17, 1888, at Sohrau, upper Silesia, now Zory in Poland. Otto was one of five children, two brothers and three sisters. The father, Oscar, had married Eugenie Rosenthal, of Rawitsch. The family moved to Breslau in 1892 and thus Otto went to school there; his home, however, strongly supplemented the school instruction, and offered to Otto ample occasion for reading scientific books for children, hearing instructive conversation, and even for making chemical experiments. He attended the Johannes Gymnasium in Breslau from 1897 to 1906, at which date he obtained the *Abitur* certificate. As usual with German students, he wandered from one university to another; he went to Freiburg im Breisgau, to Munich, and finally returned to Breslau, where he spent most of his university years studying chemistry, and especially physicochemistry. During his university years he attended lectures by Sommerfeld on mathematical physics, and by Rosanes on mathematics. The latter was the man from whom Max Born, also a student at Breslau, but a few years older than Stern, learned matrix algebra. Stern, however, did not learn much mathematics at school. Among his physics teachers were

Pringsheim and Lummer, famous for their experimental work on blackbody radiation, and also Schaefer, the author of a most popular book on classical theoretical physics, but here too the formal classwork did not prove very profitable for him. He liked physics and studied it in books; in particular he mastered Boltzmann's work on statistical mechanics, and Clausius on thermodynamics. Physico-chemistry at the time flourished in Germany: Nernst and Haber were among its luminaries, and, in Breslau, Abegg and especially Sackur had a great influence on Stern. It was under the guidance of Sackur that in 1912 Stern did his doctoral dissertation on the kinetic theory of osmotic pressure in concentrated solutions. The subject was Stern's own choice. This work consists of a theoretical part and a longer experimental part in which the theory is applied to CO_2 solutions in different solvents.

After receiving his Ph.D. degree, Stern took advantage of his economic independence to join Albert Einstein at Prague. He said many years later that what prompted him to take this fateful step was a "spirit of adventure." He had not met Einstein before but he knew he was a great man, at the center of modern developments in physics. Sackur, his professor, talked to Haber who knew Einstein, and Einstein was willing to accept him.

Stern was with Einstein from the spring of 1912 until 1914, first in Prague and then in Zurich, where in 1913 he became *Privatdozent* for physico-chemistry at an unusually early age. This period had a deep influence on him. Although Stern was primarily trained as a physico-chemist, he found plenty to talk about with Einstein. It was from Einstein that he learned what were the really important problems of contemporary physics: the nature of the quantum of light with its double aspect of particle and wave, the nature of atoms, and relativity, although the last was not much discussed by them because Stern was not deeply interested in it. Einstein at that time was studying radioactivity and the decay law. This research brought later fruits in his famous analysis of radiation where he extended the law to the probability of emission of quanta. Einstein had a great respect for thermodynamics and statistical mechanics and used these disciplines in an uncanny way to establish properties of quantized systems. Stern became also most proficient in the same direction. They wrote a paper together on the then actual problem of the zero-point energy of a harmonic oscillator, trying to calculate its influence on the specific heat of H_2. They tried to obtain experimental evidence from the existing data in favor of the $(1/2)\ h\nu$ value of the energy of the ground state of the harmonic oscillator.

Shortly thereafter Stern wrote the first of his papers of great permanent value, "The Calculation of the Entropy Constant of the Perfect Monatomic Gas." Classical thermodynamics leaves an arbitrary constant in the expression of the entropy of a perfect gas. Such a constant, unlike the arbitrary constant in energy, has observable consequences. For instance, it influences the vapor pressure of a gas in thermodynamical equilibrium with a solid. Much effort had been devoted to the theoretical calculation of this entropy constant, which appeared to be connected to quantum phenomena, and in fact Sackur and Tetrode had found a formula for the entropy constant. Their arguments, however, were not very substantial. We know now that this was so because a perfect monatomic gas at low temperature shows degeneracy phenomena which must be treated with one of the quantum statistics, Bose-Einstein or Fermi-Dirac. Their discovery lay far in the future and thus it was not possible to calculate directly the entropy constant. Stern bypassed the difficulty by considering the gas only at high temperature where classical

Boltzmann statistics is adequate and the degeneracy phenomena have disappeared, leaving their trace only in the entropy constant. He then put the gas in equilibrium with an idealized solid modeled on the one frequency crystal used by Einstein in his theory of the specific heat. The solid obeys the Nernst Theorem, which says that its entropy at absolute zero vanishes, and this takes care of the quantum aspects. Calculating the equilibrium thermodynamically and by kinetic theory applied to the model, Stern found the entropy constant confirming the Sackur-Tetrode result in an unobjectionable fashion.

This masterful paper had considerable influence on later developments; for instance, Fermi studied it thoroughly before he discovered the Fermi statistics. Stern himself extended the results obtained to diatomic molecules and applied again the method of comparing results obtained by classical thermodynamics and molecular models.

The informal teaching from Einstein was supplemented in Zurich by long discussions with Debye, Herzfeld, and especially Ehrenfest, who visited there for a prolonged period. Thus Stern became well acquainted with the problems of quantum theory before the revolutionary application of the quantum ideas to the hydrogen atom by Bohr. Nevertheless, when the Bohr paper appeared and was studied by Stern and von Laue, both in Zurich, it shocked them by the novelty of the ideas propounded. After having discussed the paper on a long walk, they decided to take an oath, "If this nonsense of Bohr should in the end prove to be right we will quit physics." They did not suspect what was in store for them.

When World War I broke out in August 1914, Stern was drafted in the German army, first as a private and later as a noncommissioned officer, and was assigned to meteorological work on the Russian front at Lomsha in Poland. The meteorological observations had sometimes to be done at temperatures of —30° C., and it was necessary to follow the balloons for as long as thirty minutes. Between observations, however, there was time for theoretical physics, and Stern pursued his studies on the Nernst Theorem, investigating its validity in solid solutions. In one paper Stern showed a deep understanding of the nature of the third law of thermodynamics and concluded that it was universally valid provided the system has *one* lowest energy state. The energy states of a mixed crystal were further investigated in another Lomsha paper in which Stern became an expert on determinants. He received from home the books he needed for this work. This is perhaps the only paper in which there is a trace of mathematical virtuosity; otherwise Stern uses everywhere only the simplest mathematics.

The Nernst Theorem was a subject of deep interest to Stern all his life. In America many years later he had long friendly arguments with the physical chemists in Berkeley when he tried to explain his point of view to people such as G. N. Lewis. Although Stern was right, he did not always succeed in convincing his opponents. In his last years he came to attribute to the Nernst Theorem a fundamental character which gave him hope to find there a foundation for quantum mechanics. The last paper he wrote, in 1962, is on this subject.

During the war Unteroffizier Otto Stern met Leutnant James Franck, in France, it seems, and the informality between a lieutenant and a noncommissioned officer surprised the German military, to say the least. Late in the war Nernst obtained Stern's transfer to an office in the War Department in Berlin. Stern continued there to use as much time as he could to pursue scientific questions. Max Born, A. Landé, and others were at work in the same office. The supervisor was R. Ladenburg, later of Princeton University. Nernst visited very frequently. It seems that pure physics interested the group much more than their official tasks. In this period Stern also met M. Volmer, who became a lifelong friend. Stern, Volmer, and James Franck used to take long walks together discussing science and politics.

They must also have found time for experimental work, because in January 1919, barely two months after the armistice that ended World War I, Stern and Volmer submitted for publication a paper on the decay time of fluorescence. The experimental work had been accomplished in Nernst's institute. Another interesting paper of that period was an attempt with Volmer to separate by diffusion a hydrogen of mass 2 from ordinary hydrogen. The motivation of this work was the hypothesis that ordinary hydrogen of atomic weight 1.008 might contain about 4 parts per thousand of the heavier isotope. The attempt failed but was prophetic.

In 1915 Stern had been admitted as *Privatdozent* for theoretical physics at Frankfurt am Main but could not take the position because of the war. Von Laue was then the professor of theoretical physics at Frankfurt. At the end of the war von Laue wished to go to Berlin, and Born, who was then at Berlin as professor of theoretical physics, exchanged his chair with him and went to Frankfurt. Stern became Born's assistant.

During the war, or even earlier, Stern had learned of the invention of molecular beams by the French physicist Dunoyer. He immediately saw the vast possibilities of this discovery. A molecular beam is obtained by letting molecules of a gas stream freely in a vacuum from the orifice of an "oven"; a partition with a slit separates the space in which the molecules enter from another evacuated chamber. Molecules which thread the orifice of the oven and the slit fly undisturbed in the second chamber and form the "molecular beam." It is essential for the working of the system that the vacuum be such that the mean free path of the molecules is large compared with the dimensions of the apparatus. The molecules in the molecular beam are free, not subject to collisions, and for this reason the method is especially suited to the study of questions relating to schematic simple situations such as are often considered in theoretical physics. In fact, many *Gedankenexperimenten* dear to the hearts of theoreticians involve molecular beams. It is thus no wonder that the method appealed to a professor of theoretical physics and a person whose previous successes lay more in the field of theory than in that of experiment. In Stern's own words, "The most distinctive characteristic property of the molecular ray method is its simplicity and directness. It enables us to make measurements on isolated neutral atoms or molecules with macroscopic tools. For this reason it is especially valuable for testing and demonstrating directly fundamental assumptions of the theory."

As soon as Stern arrived at Frankfurt he started experiments using the new technique. In this he not only had the enthusiastic support of Born, his superior, but the latter even gave some public lectures to collect funds needed by Stern for building the apparatus. Stern had performed experiments before in Breslau, but never anything so ambitious. Stern's abilities were more in the direction of planning and evaluating experiments than in executing them; he liked to associate with skilled experimenters who could complement his talents. Thus we find him associated, in succession, with Gerlach, Estermann, Knauer, and Frisch.

It must be realized that vacuum techniques in 1919 were still primitive, compared with present ones, and that the simple generation of a molecular beam was a major achievement. The vacuum was often obtained by a diffusion pump, but coconut charcoal cooled by liquid air was still the most efficient pump.

Ramsey grease was extensively used on ground joints and elsewhere. The final vacuum was tested by observing the aspect of a discharge in a Geissler tube. The estimated pressure achieved was 10^{-4} mm of Hg.

As time went on the techniques were steadily improved, but they remained on an artisanlike basis. Stern himself was a good glass blower and had much interest in technical details, fully aware of their importance for the final results of the experiment. Alternately a professor of physical chemistry and of theoretical physics, interested in the most fundamental questions of science, he knew that the way to their solution passed through minute technical details. Thus he had great, if different, rapport both with his colleague Pauli and with the mechanics who built his apparatus in Frankfurt and Hamburg. He had uncommon ingenuity and delighted in any ingenious "trick" he could invent. His respect for the shop and the machinists and his contempt for sloppy work are interestingly demonstrated by a footnote to a paper on the monochromatization of de Broglie waves of helium atoms. The toothed wheels used in his monochromator were supposed to have 400 teeth. They were built using a precision lathe and according to the instructions given with the machine. They turned out to be mislabeled and to have 408 teeth! Stern was unaware of this fact and had found a discrepancy between his result on λ and the de Broglie formula $\lambda = h/mv$ greater than the estimated experimental errors until on disassembling the apparatus he was able to clear the mystery. However, he told the facts in the paper, advertising by name the firm that had built the lathe.

Stern's laboratory remained in the forefront of the art of making molecular beams and in a situation of virtual monopoly for about fifteen years. Until about 1934, all important experiments using molecular beams originated in his laboratory;

furthermore, all successive molecular beam laboratories were founded by people who had learned from Stern, or by pupils of his pupils.

The first experiment he tackled at Frankfurt was the direct verification of the Maxwellian distribution of velocities. It is noteworthy that Stern thought that in the beam the velocity distribution should follow the law

$$dn = Cv^2\, dv\, e^{-mv^2/2kT}$$

where dn is the number of molecules with velocity between v and v + dv, C is a normalization constant, and the other symbols have the usual meaning. Einstein pointed out to him that the true law to be expected was

$$dn = C'v^3\, dv\, e^{-mv^2/2kT}$$

Experiment confirmed the predictions of kinetic theory: although, as Stern wrote, there could be little doubt that kinetic theory was correct, a direct measurement of the molecular speed was new.

This work, finished in 1920, was immediately followed by an experiment of revolutionary import: the famous Stern-Gerlach experiment on space quantization. In Bohr's theory of the atom, as then understood, the angular momentum of the atom was expected to have a component in the direction of an external magnetic field having the values $nh/2\pi$ with n integral $\neq 0$. For the case $n = \pm 1$ the atom had two possible orientations. This apparently strange prediction, if correct, should have given, classically, a birefringence of the gas, because the index of refraction for rays polarized in a direction perpendicular or parallel to the magnetic field should have been different. No such birefringence had been observed. This fact, plus the difficulty of reconciling the Bohr prediction with Larmor's theorem, which seemed to preclude a mechanism for the fixed orientation of the component of the angular momentum, worried Stern. One morning he woke up early and was lying in bed cogitating on these problems when the thought came to him that the difficulty could be tackled directly by the molecular beam method. Associated with the atomic angular momentum should be a magnetic moment $\mu = eh/4\pi mc$, and in an inhomogeneous magnetic field having a certain value of $\partial H_z/\partial z$ the atom is subject to a force

$$\mu \frac{\partial H_z}{\partial z}$$

This force produces a deflection of the molecular beam, easily calculable from elementary mechanics and the geometry of the apparatus. In the classical hypothesis there should be a continuum distribution of deflections corresponding to all possible orientations of the magnetic moment. According to the hypothesis of space quantization, on the other hand, the beam should split into a discrete number of components. Stern decided to try this experiment and, in collaboration with W. Gerlach, using a beam of silver atoms, carried it through in the years 1920-1923. The ultimate conclusion was the experimental proof of the space quantization and the measurement of the magneton, $eh/4\pi mc$. Stern and Gerlach devoted five papers to the description of this memorable experiment, which was a turning point in the study of the mechanics of the atom. In the last, most comprehensive paper they described all the technical refinements developed. Of course, reading it with hindsight one wonders why the interpretation of the experiment did not point to the electron spin. Stern said in his old age that the surprise and the excitement of the result were overwhelming. He had started the experiment without knowing what to expect. The classical arguments and the quantum arguments were both strong and he wavered from day to day in his expectations. The experiment was really a question put to Nature, to use Stern's own words, and the final result was a surprise. It could not, however, be completely understood without spin and quantum mechanics, both still in the future.

In the meantime Stern had been called as professor of theoretical physics at Rostock and thus left Frankfurt even before the termination of the Stern-Gerlach experiment, returning to Frankfurt during vacations in order to finish the experiment. He held the Rostock position for a short time only; from September 1921 to December 1922. The University of Hamburg then offered him the position of ordinarius for physico-chemistry and promised him a special laboratory for molecular beams, which in due course was built in the Jungiusstrasse. His colleagues, Koch, Lenz, R. Minkowski, Unsöld, and the younger theoreticians mentioned later, contributed to create a pleasant and stimulating atmosphere. Stern enjoyed their company even outside the institute, and they helped him solve the severe space problems which arose while the new institute for Stern was being built. Stern remained at Hamburg until the advent of Hitler forced him to emigrate, and while there he accomplished the greater part of his work.

Although work with molecular beams was to remain his main activity, it was not his only interest, and in the year 1924 we find an important paper of pure physico-chemistry on the theory of the electrolytic double layer which contains ideas important even for practical applications in mineral flotation, plus another paper on the equilibrium between matter and radiation which has had considerable following among the cosmologists.

In 1926 Stern inaugurated a series of papers which he called Untersuchungen zur Molekularstrahlmethode (U.z.M.) This series reached No. 30 before it ended with the words "Nevertheless the experiments had to be prematurely interrupted owing to external causes," meaning that the institute had been in effect dismantled by the Nazis. In the first of the U.z.M. dated September 1926, Stern explains the advantages and disadvantages of the method, discusses technical details, and then gives the program for future work. In this section he mentions (1) the measurement of magnetic moments of molecules, including those due to the electrons, to the nucleus, and those induced by diamagnetic action; (2) electric dipole moments, including the so-called permanent dipole moment as well as moments of higher order (quadrupoles); (3) the measurement of the field of force of molecules (molecular forces); (4) fundamental problems such as the recoil on emission of a quantum, the de Broglie waves of matter, and others. The execution of this colossal program kept him busy and gave work to many assistants, students, postdoctoral fellows, and guests of his institute. Among his assistants were, chronologically, I. Estermann, F. Knauer, R. Schnurmann, and O. R. Frisch; students were E. Landt, A. Leu, E. Wrede, B. Lammert, and M. Wohlwill; among the guests or postdoctoral fellows were T. E. Phipps, I. Rabi, E. Segrè, J. B. Taylor, L. C. Lewis, and J. Josephy.

The formidable program in U.z.M. No. 1 was almost entirely executed during Stern's tenure at Hamburg. The most notable result, most notable because totally unexpected, was the determination of the magnetic moment of the proton to an accuracy of about 10 percent as 2.5 nuclear magnetons. Before the experiment started, several theoreticians had deemed it a waste of time and effort, so sure were they that the magnetic moment of the proton was one nuclear magneton. This experiment was supplemented, in the last days of the Hamburg laboratory, by the measurement of the magnetic moment of the deuteron. The experiments on the de Broglie waves of the He atoms confirmed in a striking fashion the results predicted by quantum mechanics, but gave also some unexpected results on the interaction of atoms with surface lattices. However, when these elegant and conclusive experiments on the wave aspects of helium atoms were published they did not cause surprise because quantum mechanics was by then so solidly established that one could not doubt the outcome. Stern himself felt that the experiments were perhaps not sufficiently appreciated. He always loved them and thought that the diffraction of a helium atom, a true chunk of matter, was more significant than the diffraction of an electron.

With the Stern-Gerlach experiment, Stern had acquired worldwide fame and became a frequent visitor to foreign countries, including the United States. He had a special preference for Berkeley, in part because he liked his colleagues in chemistry and physics there, in part because he was very sensitive to its pleasant climate. The University of California invited him as a visiting professor and gave him an honorary degree of LL.D. in 1930. In 1933 Professor G. N. Lewis gave him a precious few drops of D_2O with which Stern measured the magnetic moment of the deuteron. Stern recognized immediately the potentialities of the cyclotron and as early as 1931 reported in Europe on the work of Lawrence and his associates with great enthusiasm. However, when Stern was forced to emigrate he did not receive an offer from Berkeley.

Stern also attended regularly the Copenhagen conferences in Bohr's institute and participated very actively in European meetings.

When the Fermi group, somewhat isolated in Rome, decided to send some of its younger members to learn modern experimental techniques abroad, it befell me to go to Hamburg for about a year on a Rockefeller Fellowship starting in 1931. Stern was then at the peak of his powers and I intended to learn all I could from him. The first thing I noticed was his habit of calculating everything amenable to calculation, of measuring everything he could, and of not proceeding unless theory and experiment agreed or he had found the reason for the disagreement. He was most systematic and meticulous; for example, he never relented until the shape and intensity of a molecular beam were those he had calculated before starting the experiment. He was interested in minute technical details and showed me how to hunt for leaks in an apparatus, but otherwise left me pretty much to my own resources. Being in a room near his laboratory, I could go and see him and Frisch work on their experiments. Frisch gave me numerous technical suggestions. There was considerable discussion on the theoretical aspects of the work of the laboratory and I wrote to my friends in Rome and told them during vacations about the problems in Hamburg: Hamburg had had a distinguished succession of professors of theoretical physics—Lenz, Jordan, Pauli, Gordon, Jensen—but in 1932 there was a sort of interregnum and outside help was particularly welcome. This is why in the U.z.M. publications of that period one finds mention of Majorana, Einaudi, Wick, Fermi, Fano, and others. They were all in Rome, admired Stern's work, and liked to help, even at a distance, by solving the problems I reported to them from Hamburg.

The experiment on which I worked purported to demonstrate the dynamics of the establishment of space quantization, by flipping over oriented potassium atoms. It had been started by Stern and Phipps, but left unfinished when Phipps's fellowship ended. I inherited his apparatus, but could not make much headway until on reading Maxwell's *Electricity* I found a trick by which one could achieve a certain magnetic field configuration essential to the success of the experiment. The experiment is of some historical interest because its results elicited a remarkable explanation from Rabi, who connected them with nuclear spin.

Stern's extremely successful work at Hamburg came to an end in August 1933 with his resignation, which was caused by the Nazis. The pretext was the dismissal of his colleague and old pupil Estermann and an order to remove Einstein's portrait from his office. He emigrated, and whatever was left of the institute, deprived of its soul, soon petered out. The Carnegie Institute of Technology at Pittsburgh, Pennsylvania, created a research professorship for Stern and he reestablished there a molecular beam laboratory with the help of Estermann. They did valuable work in molecular beams and obtained some pioneering results, without the use of molecular beams, when, together with O. C. Simpson and J. Halpern, they showed that parahydrogen has a much greater transparency than orthohydrogen for slow neutrons. This fact determines the sign of the singlet n-p scattering length as opposite to that of the triplet. Another paper by Estermann, Lewis, and Stern is devoted to the measurement of the change in density of potassium chloride upon X-ray irradiation. This paper was connected with some war work.

Stern and his associates further perfected the Hamburg techniques but did not use the new powerful resonance methods that Rabi and his associates were then developing, and through which they were reaching precision in the measurement of magnetic moments unattainable by Stern's methods.

Stern was naturalized as an American citizen on March 8, 1939, and during World War II served as a consultant to the War Department. At the end of the war, Stern resigned his Pittsburgh position and retired, making his home in Berkeley where he had two sisters. He visited Europe almost every year, especially Switzerland, attracted by the mountains of the Engadine, his favorite places being Chantarella in the Engadine and Zurich, where he could have the company of his old friend Pauli. He never revisited Germany nor collected the German pension due him, expressing in this way his abomination for Nazism. On the other hand, he maintained cordial relations with several of his old German friends, and as soon as it was possible, at the end of the war, supplied them with packages of some of the amenities or necessities of life they were missing. He managed also to see them, outside of Germany.

Stern never married. As a young bachelor he liked to dance, and was a good tennis player. By the time he lived in Hamburg he had become somewhat of a *bon vivant*. He went only to the best hotels, liked good cuisine, excellent cigars, and in general all the refinements of life. He had many friends among physicists: Lise Meitner, his colleagues at Hamburg, Franck, von Laue, Bohr, Volmer, and others were close to him and he saw them relatively frequently. He was always invited to conferences because he always had something important to say.

He was easily accessible, at least by German standards of the time, to his students and postdoctoral fellows, with whom he had lunch regularly. He went frequently to the movies, but had to be told by his companion Pauli whether he had already seen the film or not.

When Stern came to the United States he tended to isolate himself. After retiring from Pittsburgh, in Berkeley, he attended the Physics colloquium regularly. During his last years he remained interested in the great discoveries in particle physics and astrophysics. A few days before his death he argued vehemently in favor of explaining the enormous energy output of quasars by a reaction between matter and antimatter and was dissatisfied that astrophysicists rejected that interpretation! On his seventieth birthday, the practitioners of the art of molecular beams honored him with a special book devoted to him, but by then he was seldom seen at any meetings. A few old friends visited him occasionally and he received them with unvariable courtesy, although it seemed clear that the thing he cherished most was his privacy.

Stern was stricken by a heart attack while at the movies and died in Berkeley on August 17, 1969, at the age of eighty-one. He had been elected to the National Academy of Sciences in 1945; he was also a member of the Royal Danish Academy since 1936, and of the American Philosophical Society of Philadelphia. He received the Nobel Prize in Physics for 1943.

Stern was one of the greatest physicists of this century. He wrote relatively few papers, but of what power were those he did write! The reader does not know whether to admire most the simplicity and profundity of the theoretical ideas, the ingenuity of the techniques employed, or the inescapable force of the conclusions.

The material for this biographical sketch comes from papers kindly made available by the heirs of Otto Stern and from materials in the Archives of the American Physical Society. An autobiographical tape dictated by Stern exists at the Eidgenossische Technische Hochschule in Zurich, but was not made available to me. I am indebted also to Professors R. Minkowski and I. Estermann for pertinent information.

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KEY TO ABBREVIATIONS

Ann. Physik = Annalen der Physik
Phys. Rev. = Physical Review
Physik. Z. = Physikalische Zeitschrift
Z. Electrochem. = Zeitschrift für Electrochemie
Z. Physik = Zeitschrift für Physik
Z. physik. Chem. = Zeitschrift für physikalische Chemie

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