Igor' Vasil'evich Kurchatov at the Leningrad Physicotechnical Institute

A. P. Grinberg and V. Ya. Frenkel'

A. F. Ioffe Physicotechnical Institute, Academy of Sciences of the USSR, Leningrad
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The years of scientific (including organizational) activity of I. V. Kurchatov at the Physicotechnical Institute marked an important period in his development. This period is examined here, with particular attention paid to his studies on the electrical properties of solids (dielectrics, semiconductors, and ferroelectrics) and in nuclear physics (neutrons, nuclear isomerism, the cyclotron). Certain questions pertaining to Kurchatov's pedagogical activities are elucidated. A substantial part of the material presented is based on archival documents.

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I. INTRODUCTION

1. The life of Igor' Vasil'evich Kurchatov has long been of interest to physicists and historians of science, to writers and especially to readers. "A Personal Directory of Literature," concerning Kurchatov published in 1977, lists more than 100 titles devoted to some stage or other of his activity. Of course, the principal focus of this literature is the chief work of Jurchatov's life—research on the uranium problem. Among the many reasons for the success of the projects led by Kurchatov was the remarkable scientific training he received at the Leningrad Physico-Technical Institute (LPTI).[1)] At the same time, however, Jurchatov's studies at LPTI from 1925 to 1943, a time which had a decisive impact on his creative style and wide-ranging scientific interests, have been unjustifiably neglected. The present article is intended to help fill this gap.

2. Information on the first years of Kurchatov's life and training can be found in a number of published works, primarily in the books by Golovina[2] and Astashenkov.[3] Let us give an excerpt from the beginning of one of Kurchatov's many autobiographies: "I was born in January 1903 at Simskiĭ Zavod, formerly in Ufim Province, into the family of a surveyor. From 1913 I lived in Simferopol, where in 1920 I graduated from a gymnasium and in 1923 graduated from the Physico-Mathematical Faculty at the Crimean University. At the end of 1923 and the beginning of 1924 I worked as an observer in the Hall of Electricity at the Pavlovsk Magneto-Meteorological Observatory. Here I did my independent research. At the end of 1924 and the beginning of 1925 I worked as an assistant physicist at Bakinsk Polytechnical Institute, where I studied the electrical conductivity of dielectric solids.

In the fall of 1925 I was chosen to become a Physicist of the Institute by the Scientific Council of the Physico-Technical Institute. I subsequently worked at the Institute in the capacity of Senior Physicist, Department Chairman, Group Leader of the Solid State Group, and Laboratory Director (Archives of the I. V. Kurchatov Institute of Atomic Energy, f. 2, op. 1, ed. khr. 169, l. 57; autobiography written in 1935)."[2)]

One could comment at length about this excerpt. Here we shall only point out that Kurchatov graduated from Simferopol Gymnasium with a Gold Medal and that his studies at the University, which he finished ahead of schedule, were every bit as successful. In 1920, his first year at the University, Kurchatov could attend lectures by Ya. I. Frenkel' on physics and by V. I. Smirnov, N. S. Koshlyakov, and M. L. Frank on mathematics; the Rectors of the University were V. I. Vernadskiĭ and A. A. Baĭkov. He would later be involved with all these people. The Pavlovsk Observatory is a branch of the Voeĭkov Main Geophysical Laboratory, Leningrad. In his autobiographies Kurchatov mentions a brief period at a similar institution—the Central Hydrometeorological Station of the Black and Azov Seas; from July to October of 1924 he worked in the capacity of "Inspector of the Hydrometeorological Station" and published two papers on seiches in the Black and Azov Seas. Concurrently with his work in Pavlovsk he studied for a year at the Ship-Building Faculty of Leningrad Polytechnical Institute, from which he was dismissed for "lack of progress." This episode, which at the time distressed Kurchatov greatly (he took pains to explain the reasons for his dismissal—overwork in Pavlovsk—in one of the documents from the twenties), can only make us smile today.

3. In the many autobiographies of Kurchatov, in the personal-data forms which he filled out for personnel files, etc., the exact time of his arrival at LPTI and his subsequent promotions vary somewhat, though not to any great extent. This is only natural, as one can err in giving the month and, especially, the date of a promotion, or the exact title of a position. Inaccuracies of the latter kind are particularly understandable, since in the pre-War years the staff titles at LPTI, the names

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[1)] The A. F. Ioffe Physico-Technical Institute underwent several name changes in the first 30 years of its existence. For example, up to the time of Kurchatov's arrival (1925) it was called the State Physico-Technical Roentgenological Institute. In the thirties and forties, however, the period of our main concern here, it was called the Leningrad Physico-Technical Institute, and that is the name we shall use in this article.

[2)] References to the documents of the various archives will henceforth be given in the text, and references to published articles will be given in the list of references at the end of the article.

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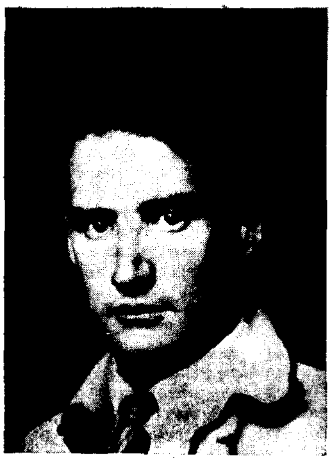

Igor' Vasil'evich Kurchatov. First half of the nineteen-twenties.

of laboratories, departments, teams, and groups changed often. We therefore consider it worthwhile to improve the accuracy of the data using documents from the archives of the A. F. Ioffe Physico-Technical Institue and the I. V. Kurchatov Institute of Atomic Energy (IAÉ), particularly Kurchatov's work-book (included in his personal file, which was transferred in due course from LPTI to IAÉ; see IAÉ Archives, ed. khr. 169). The first entries in the work-book are for 1925. We have tabulated all this material in Table I.

II. STUDIES ON THE PHYSICS OF DIELECTRICS AND SEMICONDUCTORS

1. Kurchatov arrived in Leningrad from Baku in the autumn of 1925 and on October 1 started work at LPTI, which had taken him on as an extra staff member, as a Physicist ("Scientific Worker First Class" in another document) of the Leningrad Physico-Technical Laboratory (LPTL). This was a remarkable time for LPTI. The Institute was rapidly expanding. Only two years before, i.e., in 1923, it had moved from crowded quarters at the Laboratory of General Physics of the Leningrad Polytechnical Institute (LPI) into a new building (which today houses the administration of the Institute, the library, and a number of scientific laboratories), and, nevertheless, by the end of 1925 was again feeling a shortage of space. Even greater difficulties stemmed from a shortage of funds. Narkompros [the Department of Education] and the Chief Scientific Council, which had

TABLE I.

	Date	Title and duties
1 *)	October 1, 1925	Scientific worker first class
2 *)	March 1, 1928	Senior engineer/physicist
3 *)	September 16, 1930	Laboratory director (ferroelectrics)
4	September 18, 1930	Head of department of general physics
5	July 1, 1931	Leader of crystal physics group
6	May 1, 1933	Head of department (nuclear physics)
7	February 16, 1934	Head of department
8	November 16, 1936	Director of nuclear-reactions laboratory
9	April 1942	Director of LPTI Laboratory No. 3 (tank armor)
10 *)	August 1943	Director of Laboratory No. 2, with transfer to Moscow
11 *)	January 15, 1944	Placed on special payroll and removed from staff and payroll of LPTI

*Data taken from Kurchatov's work-book are indicated by an asterisk after the number in the first column. The remaining information was gathered from the archives of LPTI and IAÉ.

responsibility for LPTI, could not meet the growing requirements of the institute for staff, accommodations, and equipment. Meanwhile, though maintaining direct ties to the colleges (vuzy) and primarily to Leningrad Polytechnical Institute, LPTI began to interact more and more closely with industrial enterprises in Leningrad and elsewhere. A. F. Ioffe and his closest assistants at the Institute—A. A. Chernyshev, N. N. Semenov, D. A. Rozhanskiĭ, V. N. Glazanov, and others—accomplished this interaction through a special laboratory under the jurisdiction of the All-Union Economic Council (VSNKh), which had substantial financial resources at its disposal and, most importantly, was directly in charge of planning the development of Soviet industry. The Leningrad Physico-Technical Laboratory actually began its activities back at the end of 1924. The status and regulations of the Laboratory were taken up at the October 19, 1925 session of the Presidium of VSNKh. The mission of LPTL, according to these regulations, included conducting physico-technical research and applying it to the needs of industry, carrying out tasks for industry, training industrial engineers, organizing technical consultations, etc. On March 1, 1926, LPTL began its functions in a building at No. 1a Yashumov Lane (now Kurchatov Street), which had been expressly assigned to it. In the beginning the staff of LPTL numbered 45 persons, but their number rapidly grew, with some of the staff working simultaneously at LPTI.

The scientific interests and studies of A. F. Ioffe, which he had been pursuing since the previous decade, accorded remarkably with the problems in the development of Soviet industry. His work on the physics of x rays and their effect on the electrical properties of solids stimulated research at LPTI and LPTL on x-ray structural analysis and defectoscopy. The study of the elastic properties and mechanical strength of crystals dictated the development of contacts between the department at LPTL led by N. N. Davidenkov and the metallurgical plants and railroads.

2. Of particular interest to us is the wide range of the studies on the physics of dielectrics and the mechanism of their electrical conductivity, which also came up in the pre-Revolutionary works by A. F. Ioffe on this subject, which he began while still in Munich. The construction of power stations and communications lines and the needs of cable manufacturers required the active assistance of physicists. It was this area of applied research—the study of the conditions for breakdown in dielectrics, their insulating properties under extremely high voltages—that A. F. Ioffe headed up under the auspices of the Department of General Physics at LPTL. From the ranks of his colleagues at LPTI and the Physico-Mathematical Faculty at LPI he attracted A. F. Val'ter, B. M. Gokhberg, N. N. Semenov, and V. A. Fok to work in this area of research. But the Department of General Physics at LPTI needed still larger numbers of energetic staffers.

In September of 1924, at the time of the Fourth Congress of Russian Physicists (which was held in Leningrad), a young man named K. D. Sinel'nikov came to Ioffe's attention. Ioffe had heard of this young man from Prof. S. N. Usatyĭ, a colleage (and in-law) of Ioffe who was Sinel'nikov's teacher at Tavricheskiĭ (Simferopol) University and later his co-worker at the university in Baku. Although Sinel'nikov had not submitted beforehand a paper for the congress, he gave a report on his studies in the physics of dielectrics that impressed Ioffe. Ioffe invited Sinel'nikov to work with him at the Institute, and Sinel'nikov started work on December 1, 1924. With Usatyĭ's backing, Sinel'nikov recommended to Ioffe a classmate of his at Simferopol and Baku—Igor' Vasil'evich Kurchatov, who was also studying the physics of dielectrics. It was in this area that the technical-physics period of Kurchatov's activity began, and was to continue for about twenty years.

3. To "break the ice" at his new job the twenty-two year old Kurchatov (who had by then published 5 papers) undertook a methodical study (carried out with the collaboration of Sinel'nikov)[4] of the passage of slow electrons through thin metal foils. A problem of this kind arose in connection with attempts being made at the time to place photographic film on the outside of a Braun cathode-ray tube (a prototype of the modern cathode ray oscillograph). It was assumed that the electrons accelerated in the vacuum tube, after shooting through the thin metallic window, could interact directly with the film. This question had been the subject of a note by the American physicist H. E. Hartig,[5] who had obtained strange results implying that slow electrons with energies of several electron volts can pass through an aluminum film 3–9 μm thick. Hartig had been convinced of the integrity of the foil, i.e., of the absence of holes going through it, by simply examining it under transillumination. Sinel'nikov and Kurchatov on a similar apparatus first confirmed the results of the American investigator and then showed unambiguously that they were due to imperfections in the foil—small holes which could not be seen under light but whose existence was proved by the young Soviet physicists by observing air bubbles passing through them in a liquid. Afterwards, in working with a truly defectless foil, Kurchatov and Sinel'nikov[4] obtained results which were free of contradictions.

This study was not pursued further; it is of interest only because it was Kurchatov's first (and also as in indication of the unusual—from a modern point of view—problems faced by experimenters at that time: It is hard for us to imagine that in the mid-twenties the curves traced by the electron beam of the Braun tube were photographed from inside the tube).

4. The physics of dielectrics underwent a period of intensive development in the twenties. By the end of the last century it had been proved, primarily by the work of Warburg, that the transport of current in glass takes place by means of sodium ions. It was shown that Faraday's law is obeyed rather accurately in glasses and other solid dielectrics. There were, however, significant departures from Ohm's law. The temperature dependence of the electrical conductivity over a wide temperature range remained unclear.

Ioffe involved his co-workers at the Laboratory in this complex of questions which preoccupied him at the time.

Ioffe had already established that the main potential drop (discontinuity) in certain dielectrics occurs near the electrodes. It was thought that this effect occurred only at high temperatures. The work of Kurchatov and Sinel'nikov[6] refuted this idea. The focus of these studies was on the electrode sheath, on methods of measuring its thickness and determining the voltage drop across it. Kurchatov and his colleague were coming face to face with the problems of dielectric breakdown.

By that time the thermal theory of breakdown had gained wide currency. Under conditions such that the heat liberated by the passage of current through a dielectric is not completely removed from it, a heating of the sample occurs which leads to an increase in the current and a corresponding growth of the Joule heating: The process grows in an "avalanche" to breakdown (the thermal destruction of the dielectric). However, at low temperatures, and with a large enough electrode surface area and not too thick a sample, the breakdown occurs due to some mechanism other than thermal. Since electronic conductivity had not been observed in the investigated dielectrics, Ioffe, Sinel'nikov, Kurchatov, and P. P. Kobeko[7] came out with support for and an expansion of the somewhat earlier suggestion of Ioffe that breakdown results from the development of an ion avalanche, leading to impact ionization. The basic idea was that the mean free path of an ion in the crystal was sufficient for the ion to build up the necessary energy to knock out another ion. However, if the test sample was thin enough that the path of the ion to the electrode contained only a few mean free paths, an avalanche would not be able to form on a large enough scale to constitute a breakdown. This meant that the breakdown voltage should increase with decreasing sample thickness. Experiments seemed to confirm such a tendency. And that would give rise to a wide range of possible technical applications—the production of high-voltage insulators made from thin insulating layers (glass, mica, oil, etc.) and also high-voltage "storage batteries."

Further studies, both theoretical[8] and experimental,[9] showed that the concept of the ion avalanche and the corresponding experimental results should be reexamined, since systematic errors had crept into the experiments in the determination of the thickness of the investigated layers.

5. It is important, however, that in the course of these studies in Ioffe's laboratory, particularly those of Kurchatov and his colleagues, a number of positive results were obtained that are still important today: New insulating materials were discovered, precision experiments were done, the validity of Faraday's law was demonstrated,[10] a path was laid toward a picture of the resistance of crystals to electrical breakdown, a profound analogy was developed between the mechanical and electrical properties of crystals (the role of impurities and other lattice distortions in the processes related to mechanical and electrical destruction).

We note that the growth of the breakdown voltage with decreasing sample thickness was later reliably observed in a number of experiments with dielectrics, but the breakdown here arose on account of the development of an electron avalanche; it can thus be said that the idea of impact ionization in solids, although in a modified form, was developed and confirmed. Moreover, studies[11] of p-n junctions have demonstrated the existence of the same effect in semiconductors.[3)] Here the p-n junction, from which free carriers have been "extracted" by a strong electric field, is analogous to the thin electrode sheaths in a dielectric, which are responsible for the high-voltage polarization and other effects. Direct experiments in semiconductors (germanium and silicon) in the fifties, while Ioffe and Kurchatov were still alive, showed that the breakdown field on a p-n junction increases as the junction thickness decreases. Thus, in this case a carrier avalanche does arise and the "Ioffe effect" occurs, as was stressed by Vul and Shotov.[11]

6. An article related to this series of studies on the electrical breakdown strength of dielectrics was published by Kobeko and Kurchatov[12] in the journal *Fizika i Proizvodstvo* [Physics and Production]. This article is of definite interest as evidence of active contacts between these young technical physicists and industry, in this case with the Red Triangle Plant. The journal itself was intended to promote the development of contacts of this kind made by Ioffe and his colleagues. Its editorial board included G. V. Braude, A. F. Val'ter, D. A. Rozhanskiĭ, and I. V. Kurchatov.

The article by Kobeko and Kurchatov was entitled "Work at the Physico-Technical Institute in conjunction with production at the Red Triangle Plant." It was an analysis of the production of the ebonite works at the plant. It happened that no studies of the electrical insulating properties of their products were being done at the plant. One of the problems addressed by the studies at LPTL was to provide guidelines for the plant as to the necessary size of their laboratory and the possible scientific problems that could be studied there. We see that Kobeko and Kurchatov participated in a most direct way in promoting the organization of laboratories in plants. The importance of this practice, which was instituted on the initiative of the leadership of the LPTI, would be hard to overemphasize. As for the studies themselves, it was discovered that the breakdown voltages of the ebonite sheets displayed a large scatter. Kobeko and Kurchatov[12] showed that this was due to the existence of "weak" spots on the ebonite sheets which could be detected visually. These spots resulted from the imperfect technology of the vulcanizing process, which they proposed to correct. "We hope," wrote these authors in concluding their paper, "that the problems touched upon in this article can be solved right in the laboratory of the plant; we have developed plans for the laboratory and presented them for inspection to the administration of the Red Triangle Plant."

7. Kurchatov's studies on the electrical properties of dielectrics can be considered a prelude to a series of papers on ferroelectricity which was to bring world renown to him and his colleague and classmate Kobeko. Before reviewing these papers, however, let us turn to several other papers by Kurchatov on the physics of dielectrics and semiconductors.

The first of these papers[13] (which, like the majority of Kurchatov's papers from 1927–1933, was written with the collaboration of Kobeko) came about in the course of a critical review, as was so typical of Kurchatov's style, of the data of an experiment by the German physicist Trei on the unipolar conductivity of the salts AgI and Ag_2S. The rectifying properties of the samples, which were pressed from powders, did not depend on the geometry of the points of contact with sharp-point electrodes. (The question of whether such a selectivity might be possible was a natural one in those days of rectifier radio receivers!) The measurements of Kobeko and Kurchatov, which were made by means of Ioffe probes, showed that nonconducting layers of iodine (or sulfur) formed in the sample near the anode, and that the observed decrease in the current with time was due to the growth of the resistance of the samples. The unipolarity was thus determined by the geometry of both electrodes (sharp-point and plate) and the corresponding difference in the rate of decrease of the current with time. The latter circumstance was reflected in the time dependence of the rectification factor.

Kobeko and Kurchatov[13] pointed out in conclusion that the phenomenon of unipolar conductivity observed by Trei (which he had incorrectly interpreted in terms of the mobility of the ions taking part in the electrical conduction process at high temperatures) was typical not only of AgI and Ag_2S, which were studied by Trei, but also appeared at temperatures of the order of 300 °C in a variety of other materials—glass, mica, $Na_2B_4O_7$, $Li_2B_4O_7$, and $K_2B_4O_7$. In all these dielectrics the application of a voltage leads to the formation of insulating layers in the region adjacent to the electrodes—a process which Kobeko and Kurchatov called "forming" (as we mentioned earlier, it was first observed by Warburg in the case of glass). This sort of extension of the range in which an effect appears, a *sui generis* generalization of the effect, was very typical of Kurchatov's working style.

These same problems were dealt with in more detail in an article by Kobeko, Kurchatov, and Sinel'nikov[14] on the unipolar conductivity of powder samples of CuS and Cu_2S (these sulfides of copper are semiconductors; this paper was probably the first Soviet study in this area of physics). It was demonstrated first of all that the copper ions participated in the electrical conduction under the experimental conditions: Faraday's law was obeyed, though not exactly. The unipolarity effect did not depend on the electrode material if one of the electrodes was flat and the other sharp (in agreement with the results of the previous article). However, unipolarity was recorded even when both electrodes were flat, provided that one of them was aluminum. These authors suggested that in this case there is a marked difference in the areas of the contact surfaces of the electrodes with the sample under study.

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3) The article cited contains a detailed bibliography of the corresponding experimental and theoretical papers.

Another factor controlling the unipolarity was the formation of metallic filaments in the dielectric crystal. The precipitation of the metal during electrolytic conduction of certain solids did not occur uniformly over the entire surface, but in the form of thin filaments growing through the sample. These filaments (bridges) could not grow on a sharp-point anode because of the rapid growth of an insulating forming layer on such an electrode. But when the anode had a large surface area the filaments grew rapidly. So this effect also leads to unipolarity: An electron current flows along the metallic threads. Faraday's law does not come into play here, since the conduction almost immediately ceases to be electrolytic.

The set of metallic bridge filaments, which grow along specific directions in the crystal lattice, is reminiscent of a tree. Effects of this sort and the corresponding conductivity are therefore called "dendritic." In later studies at the Ukrainian Physico-Technical Institute in Kharkov, Kurchatov and his colleagues (Sinel'nikov, A. K. Val'ter, and O. N. Trapeznikova) made a careful study of this effect in its own right, and took impressive photographs of the metallic filaments permeating the crystal.

Kobeko, Kurchatov, and Sinel'nikov[14] set out to demonstrate that the "forming-dendritic" model described above gives a fitting account of the unipolarity effect. Additional experiments, including the study of thin sections of an aluminum electrode, showed that only 5% of the electrode surface was in contact with the CuS and Cu_2S samples (and the same holds true for several other metals, including copper, that were given special study). This effect was explained by the circumstance that most of the contact surface was covered by a thin oxide film.

On the basis of the electrical measurements it was established that in the case of a positive potential a layer of sulfur was deposited on both electrodes (the so-called "forming"); if the anode was aluminum, the deposition of the sulfur layer went rapidly to completion on account of the small effective contact area. The formation and decay of the sulfur insulating layer near the electrode and the growth of the dendrites were studied in detail using the technique of supplying a pulsed voltage to the samples.

While the rectification (unipolarity) factor was roughly 10^3 when steady voltages of different signs were applied on the aluminum anode, for an ordinary alternating current this factor was 15–20. It should be said, however, that "electrolytic" rectifiers, whose very unique physics was studied in detail at LPTI (primarily by Kobeko, Kurchatov, and Sinel'nikov), were inferior to selenium and cuprous-oxide rectifiers. It was just at this time that Soviet industry began to produce the latter type of rectifier, and so Kurchatov did not return to the series of physical and technical problems considered in Ref. 14, but in the final stage of his research on the electrical properties of solids did several studies on barrier-layer photocells based on Cu_2O, which are interesting mainly for their spectral characteristics.[15]

8. Kurchatov paid his last tribute to research on the electrical properties of solids in a paper[16] written in 1933 with N. A. Kovalev, T. Z. Kostina, and L. I. Rusinov (who would later become his co-worker at the Nuclear Laboratory of LPTI). This work was of an applied nature and was motivated by a problem in electrical engineering—the protection of electrical transmission lines from overvoltages caused by lightning strikes. A similar problem had been faced by LPTI in earlier years in connection with the overload protection of communication lines; here the overloads resulted from the proximity of the lines to power lines. This problem had been solved in its day by A. A. Chernyshev through the creation of special vacuum arc gaps. In the case of the high-voltage lines, protection was provided by solid-state dischargers with a self-adjusting resistance. The resistance was switched into the circuit in parallel with the protected object following the operation of the spark gap, with its suitably chosen electrode separation. The solid-state resistors, which were developed for this purpose abroad, were special ceramic composites (tyrite in the USA, ozelite in Germany). The technology for the fabrication of these composites was very complex and therefore undesirable to reproduce. It was thus necessary to develop an equivalent. Kurchatov and his co-workers chose for this purpose pressed carborundum (SiC) powders with a certain filler or binder. Powders such as this were being produced for other purposes by the Il'ich Plant in Leningrad. For such composites to work in the protective devices it was necessary to have gaps (transitional contacts) between the carborundum grains. The entire device had to meet a number of specific requirements, chiefly that the resistance decrease as the voltage rises (that the voltage-current characteristic be nonlinear), that the contacts be stable, and that the device have a low interia and a long service life, this last being governed by the aging of the carborundum and of the bonding of the composite when current is passed through it.

In Ref. 16 the authors developed a special technology for fabricating the carborundum compounds and a method of studying their electrical characteristics (under pulsed loads), so that the required operating parameters and geometry could be chosen. The (type S-100) compound that they developed had a projected service life of at least 5 years and was in every respect economical and competitive with the imported samples. The stability of the voltage-current characteristics was so high that A. Z. Shakirov, in a further development of these studies, proposed to use S-100 in devices for measuring high voltages.

To explain the operating principle of the carborundum self-adjusting compounds, Kurchatov and his co-workers, having critically examined the corresponding American papers (which attributed the "self-adjustment" to the occurrence of microscopic spark discharges between the carborundum grains), settled on a wave-mechanical tunneling mechanism for the passage of the current (electrons) between the narrow gaps between the grains, which implied a stepwise dependence of the current on the applied voltage. An experimental check of these ideas gave such good agreement that the authors themselves expressed surprise at how well the theory, which was derived for the pure case, applied to the carborundum composite. However, Kurchatov and Rusinov never took up the problem (which they had posed in Ref. 16) of refining the theory of self-adjusting resistances.

At the same time, of course, the physics and technology of "self-adjusting resistances" continued to develop; these devices were later named "varistors." Even today varistors are made from the same silicon carbide (carborundum); from this standpoint the work just described can be considered pioneering.

Today, in addition to the modern SiC-based composites tyrite and vylite, zinc oxide is coming into wider use in varistors. The mathematical description of the characteristics of these devices (the nonlinearity factor) is the same as the description used in Ref. 16. As for the actual physics of the self-adjustment of the resistance, our current understanding of this process is that it is due to a variety of mechanisms: electron field emission from the sharp points of the carborundum grains, the passage of current through p-n junctions formed in them during the technological processes of fabricating the varistors, and thermal effects at microcontacts. High-voltage varistors are still used to protect high-voltage transmission lines and high-voltage equipment. These devices are reliable in operation and have a working life measured in decades.

III. FERROELECTRICITY STUDIES

1. Kurchatov's studies of ferroelectricity are among his best-known work. As with many of his other studies in solid-state physics, he made these studies with the close cooperation of Kobeko and his other colleagues in Leningrad (V. I. Bernashevskiĭ, M. A. Eremeev, B. V. Kurchatov, and G. Ya. Shchepkin). After the separation of Kharkov Physico-Technical Institute from LPTI, Kurchatov transferred much of this work to the Ukraine, working there with Sinel'nikov and A. K. Val'ter. By examining their publications (and also archival documents), one can immediately see that the experimentalists in Kurchatov's group interacted closely with the theorists L. D. Landau and Ya. I. Frenkel'.[17] Moreover, it can be inferred that Landau's studies on phase transitions (1937) and Frenkel's studies on the physics of dielectrics and orientational fusion were influenced by discussions of ferroelectric effects and by reflections on the nature of these phenomena. In a number of articles and in the book *Ferroelectrics*,[18] Kurchatov mentions a theory of ferroelectric phenomena developed by Landau, but this theory does not appear in Landau's publications.

It is fitting to preface our discussion of Kurchatov's first studies on ferroelectricity with an excerpt from the article[19] by Ioffe entitled "I. V. Kurchatov—investigator of dielectrics." Ioffe wrote, "Kurchatov's talents were fully revealed in the discovery and study of ferroelectricity. Certain anomalies in the dielectric properties of Rochelle salt had been described previously [see below, auth.]. Kurchatov intuitively suspected that these anomalies were manifestations of new aspects of dielectric behavior." We might add that here again one can clearly see Kurchatov's ability to proceed from a few partial rules and relationships to an extension of their domain of application and to the establishment of a whole class of effects constituting a new field of physics.

2. The first published work of Kobeko and Kurchatov on ferroelectrics was completed in March of 1930 and came out in the third number of the journal ZhRFKhO (Journal of the Russian Physical and Chemical Society) in the same year.[20] In this article it was mentioned specifically that the existence of the anomalous properties of Rochelle salt had been pointed out to the authors by N. N. Andreev (1880–1970). Nikolaĭ Nikolaevich Andreev, who had arrive at LPTI at practially the same time as Kurchatov (but as an already mature scientist) and organized the Department of Acoustics, was naturally interested in the problems of piezoelectricity. At the time in question, the dielectric anomalies in the properties of Rochelle salt (potassium-sodium tartrate tetrahydrate—$NaKC_4H_4O_6 \cdot 4H_2O$) had already been discussed in the physics literature. In addition to the articles by J. A. Anderson[21] and W. G. Cady (which were published in 1918 in an American publication not readily available to Soviet physicists), reports on this subject by Joseph Valasek of the University of Minnesota began to appear in the journal *Physical Review* in 1920. These at first were short (about one page in length, without formulas or graphs) abstracts of reports given at meetings of the American Physical Society in April[23] and December[24] of 1920. Working from his own studies of the charging and discharging of a capacitor filled with a dielectric of Rochelle salt and from the nonlinear dependence of the charge on the applied potential difference observed by Anderson, Valasek reported that he had established the existence of a hysteresis in Rochelle salt that is analogous to the well-known hysteresis of ferromagnets. Valasek pointed out the possibility that a permanent polarization exists in the salt crystal in the absence of an external electric field. Valasek's next paper contained important information on the temperature dependence of the polarizability, which was unlike that of ordinary piezoelectrics. The piezoeffect (the magnitude of the piezoelectric modulus) was particularly high in the crystal at temperatures in the $-20°$ to $+50\,°C$ range, with the corresponding coefficient having two peaks in this region. Valasek found analogous peaks in measurements of the dielectric constant ε over the discharge of the capacitor. Both these effects occurred only in one of the crystallographic directions; the values of ε measured along the other two axes were normal.

All these deliberations and assertions, stated in a still more definite form and accompanied by a description of the corresponding experiments, were published by Valasek in a series of detailed articles which started coming out in 1922.[26] In the first article he reported electrical hysteresis (and showed a graph of it), spontaneous polarization, and the identical behavior of the piezoelectric modulus and dielectric constant as functions of the temperature. It was the piezoeffect in Rochelle salt, it is important to note, that primarily interested this American author, as is reflected in the titles of all the publications we have mentioned. Valasek also spoke of "Curie points" for Rochelle salt. He found the maximum value of ε in this crystal to be 1300. As can be inferred from the table given in that article, this value depended to a high degree on the electrode material of the capacitor used in the experiment. Valasek noted a scatter in the values of ε from one sample to another and in the values measured in the same crystal at different times; finally, the value of ε also depended on the conditions of the experiment. Valasek did not give a theoretical description of the phenomenon, but pointed out that the anomalously large values of ε observed in the experiments were probably due to a polarizational displacement under the action of a field of "semifree charges"—H^+ and O^- ions of the water of crystallization of the salt molecule.

3. In Ref. 20, Kobeko and Kurchatov attempted to explain the anomaly in ε by the high-voltage polarization of dielectrics. This point of view seemed completely natural to them, since neglecting the existence of polarization layers in dielectrics leads to an extraordinarily strong overstatement of the value of ε determined from the values of the capacitance, as they well knew from Ioffe's papers and their own studies. It is not surprising, therefore, that Kurchatov, in the preface to his book,[18] indicated that the studies on Rochelle salt "bordered directly on a number of studies by the author and P. P. Kobeko on the effects of high-voltage polarization, which were among a group of problems studied in the laboratory of Academician A. F. Ioffe."

Systematically developing and improving the technique of measuring the profile of the potential along a slab of Rochelle salt placed in a capacitor, Kobeko and Kurchatov showed in the course of further studies that the effect in question is not due to the high-voltage polarization. These authors developed techniques for obtaining large crystals of Rochelle salt that were free of impurities. Working from their experience in the study of the electrical properties of dielectrics, they replaced the thin-foil contacts cemented to the surface of the salt crystal by electrodes consisting of its saturated solution held against the sample by the pressure of a column of mercury. This approach enabled these Soviet physicists to obtain unambiguous and reproducible results in their measurements (as functions of the potential gradient) and to refine the values of ε, which in their first experiments reached 10^4. They also ruled out the "fatigue" effect which had been observed by Valasek—a decrease in the value of ε with the time over which the voltage was applied to the capacitor.

An important aspect of Ref. 20 was the direct indication of the role in the observed effects played by the orientation of the dipoles in the Rochelle salt crystal, which was deduced from their temperature dependence. It would seem that orienting of dipoles in this crystal, a solid object, is impossible. It was at this time, however, that the concept of a dielectric polarization due to the motion of molecules began to develop; Kobeko and Kurchatov were thoroughly familiar with Errera's experiments[27] (1924) on ice and dimethyl sulfate, in which this effect was established.[4)] This permitted them to speculate that analogous processes could occur in Rochelle salt as well.

In 1930, L. Pauling developed a theory describing the transition of the motion of the molecules of solids from oscillations about an equilibrium position to rotations. In light of this theory one could infer the existence of a jump in the dielectric constant of solid hydrochloric acid (HCl), which was in fact recorded in a study of Kurchatov and G. Ya. Shchepkin.[29]

4. A series of studies carried out in the twenties and thirties at LFTI investigated the correlation between the mechanical and electrical properties of crystals. These studies included a study by Kurchatov and V. I. Bernashevskiĭ[30] on the unipolar polarization of Rochelle salt, which was first mentioned back in the papers of Valasek. However, it was only in Ref. 30 that this effect received careful study. These authors advanced the hypothesis that the unipolarity is due to mechanical imperfections in the salt crystals. Working with a thin-strip electrode, they did a scan, as it were, of the surface of the transverse cross section of the sample, systematically "probing" it. It turned out that individual parts (layers) of the crystal typically had a strong unipolar polarization, which was hidden when measurements were taken over the whole sample (with electrodes covering the entire cross sectional area). After a heating of the crystal and a subsequent slow cooling (annealing), the "unipolarity factor" decreased substantially. Kurchatov mentioned that his student and co-worker, M. A. Eremeev, had studied the properties of crystals of Rochelle salt under uniaxial compression. These studies yielded data on the shift of the phase-transition point, which Kurchatov had estimated previously, though only taking into account one of the factors influencing this shift.

Kurchatov, Sinel'nikov, and A. K. Val'ter investigated the behavior of the dipoles in the Rochelle-salt structure in Ref. 31. To study the kinetics of the polarization in an external field, i.e., the reorientation of the dipoles (within a domain), these authors used the technique of applying a pulsed voltage to the sample. In those days there were no standard short-pulse generators, so the creation of an apparatus capable of delivering a voltage pulse 10^{-9} sec in length required great experimental skill. The description of the apparatus and of the tests used to prove it out took up most of the article.[31] It was found that for pulses of this length the dielectric constant remained rather high; it followed that the period of the oscillations of the dipoles was shorter than the duration of the pulse. The configuration of the domains (Kurchatov called them "colonies") in Rochelle salt, which are elongated in the direction of one of the crystal axes, was studied later.[18] In the spirit of the domain studies in ferromagnets, in Ref. 18 Kurchatov

[4)]Kurchatov met Errera in Leningrad at a conference on solid-state physics (in September of 1932), at which the Belgian physicist gave a report[28] entitled "Dielectric polarization in the solid state." This report, in particular, gave an account of Kobeko and Kurchatov's results on ferroelectricity and stressed the fundamental nature of these results.

undertook to find a relation between the dimensions of a domain and the linear dimensions of the whole sample (a dimensional effect), and to describe the processes of polarization and depolarization of Rochelle salt.

In a short study[32] which bordered upon Ref. 31, Kurchatov and Shchepkin studied the anisotropy of Rochelle salt—the existence of an "anomalously high" polarizability in only one of the crystallographic directions. In this study the value of ε was measured along all three axes over a wide range of temperatures (from -180 to $+40$ °C).

In studying ferroelectricity it was natural to ask which structural features of the crystal and which of the constituent chemical elements were responsible for the unique ferroelectric properties. It is worth mentioning here that even though crystal physics specifies the necessary conditions for ferroelectricity (the 10 polar symmetry classes), these conditions are by no means sufficient, and even today there is no clear answer to this fundamental question. In searching for this answer, Kurchatov, Kobeko, and their co-workers (Eremeev and B. V. Kurchatov) studied the properties of Rochelle salt crystals containing impurities—isomorphic crystals of Rochelle salt containing admixtures of rubidium-sodium ($NaRbC_4H_4O_6 \cdot 4H_2O$) and tantalum-sodium ($NaTlC_4H_4O_6 \cdot 4H_2O$) tartrate tetrahydrates.[33] It was found that a 0.25% admixture decreased ε (measured along the "polarization axis") by a factor of several times; a 0.5% admixture, by 10–20 times. The "orientation" of the polarization was thus due only to the dipoles contained specifically in Rochelle salt; in the ammonium-sodium salt ($NaNH_4C_4H_4O_6 \cdot 4H_2O$) there was no "ferroelectric effect" at all (see Refs. 34 and 35, wherein these ideas were explored further).

5. Thus, at the time Kurchatov began to study Rochelle salt (1930) a number of its anomalous properties had been established, and the similarity of this substance to ferromagnets had been pointed out. There was a large scatter in the results of the papers from the twenties and in measurements of the basic physical properties of Rochelle salt, measurements were irreproducible, and there was no theory of any kind for the observed effects. It was Kurchatov, Kobeko, and their co-workers who made the first systematic studies of Rochelle salt. Kurchatov also gave the first theory of ferroelectricity, which played a large role in the phenomenological theories that began to appear in the post-War years, which were based on general thermodynamic principles and concepts about phase transitions (V. L. Ginzburg). We stress that Kurchatov considered the phase transition to be the result of an ordering of electric dipole moments as the temperature was lowered (by analogy with ferromagnets) and, in the other direction, of the vanishing of the spontaneous polarization for $T > T_c$.[5)]

[5)]The lower Curie point in Rochelle salt, whose existence was reliably established by Kurchatov and his co-workers (following Valasek), is a very special and rare occurrence in a ferroelectric and has yet to be given a definitive theoretical description.

Kurchatov based his analysis on the well-known Clausius-Mossotti formula relating the dielectric constant ε to the polarizability α of the dielectric:

$$\frac{\varepsilon-1}{\varepsilon+2}=\frac{4\pi}{3}N_0\alpha; \tag{1}$$

where N_0 is the number of molecules in a unit volume, and the polarizability α is the sum of three terms: the temperature-independent electronic and ionic terms α_e and α_i, and the dipolar term α_d, which is due to the orientation of the permanent dipoles of Rochelle salt in the electric field. The value of α_d obtained in analogy with the Langevin treatment of magnetic moments (in a magnetic field) comes out to be $(4\pi/3)N\mu^2/kT$ (μ is the dipole moment of a molecule), i.e., an explicit function of T. Solving equation (1) for ε, we find

$$\varepsilon=\frac{1+(8\pi/3)N_0\alpha}{1-(4\pi/3)N_0\alpha}, \tag{2}$$

so that for $(4\pi/3)N_0\alpha = 1$ the dielectric constant ε goes to infinity. The dependence of α on T enables one to understand the experimentally established fact that the spontaneous polarization disappears at high temperatures.

The disappearance of the polarization for $T > T_c$ (in the case of Rochelle salt $T_c = 22.5$ °C, according to the data of Kurchatov and his co-workers) was understood, as it occurred by the usual mechanism: the thermal motion of the molecules prevented the dipoles from establishing a regular order (from lining up). However, the disappearance of such an orientation at temperatures below some critical temperature had no analog in ferromagnetism. Kurchatov assumed that this last effect was occasioned by a decrease in the density N of the dipole "gas" as the temperature fell and to the resulting decrease in the forces of interaction between dipoles. In Ref. 36, I. V. Kurchatov and B. V. Kurchatov made a special study of this low-temperature limit of the spontaneous polarization, which they called the "second Curie point." The measurements of ε put this point at -15 °C.[6)] This study also included crystals isomorphic to Rochelle salt. For example, a 1% admixture of $NaNH_4C_4H_4O_6 \cdot 4H_2O$ changed the upper Curie point to 4.3 °C and the lower Curie point to -9 °C. In this experiment 30 points were taken for the $\varepsilon(T)$ curve in the interval from -33 to $+33.6$ °C, and the classical double-peaked curve was obtained for ε.

Kurchatov wrote equation (1) in a somewhat different form, separating the electronic and ionic polarizability on the right-hand side and substituting for the dipolar polarizability the expression given above. In this case

$$\frac{\varepsilon-1}{\varepsilon+2}=\frac{4\pi}{3}P_0+\frac{4\pi}{9}\frac{N\mu^2}{kT},$$

where P_0 is the sum of the electronic and ionic polarizations of a unit volume of Rochelle salt. In this formula the quantity N, which was usually defined as the number of molecules per unit volume, was taken by Kurchatov to mean the number of dipoles per unit volume that are capable of orientation. Having constructed from the experimental data the dependence of

[6)]According to recent data, the upper and lower Curie points of Rochelle salt are at 24° and -18 °C, respectively.

$(\varepsilon - 1/\varepsilon - 2)T$ on T, Kurchatov found $N\mu^2$ as a function of temperature, i.e., as he wrote, he became able to follow the change in the density of the dipole "gas" with changing temperature.

6. In 1920, Herwig developed a theory describing orientational effects in solid dielectrics, and the final formula involved the product $N\mu^4$. In Refs. 34 and 35, Kurchtov compared the values of $N\mu^2$ which he had found earlier with this quantity. The value of μ thus obtained came to $\sim 3 \cdot 10^{-18}$ cgs esu, which exceeded by several times the value obtained using other data. In these papers one notices Kurchatov's already somewhat critical attitude toward the dipole theory of ferroelectricity, which in fact treats a dipole gas. In his monograph[18] he writes even more definitely: "It must be noted that although the basic features of ferroelectricity are correctly described by the Debye theory, a complete quantitative description of the phenomenon in terms of this theory has nevertheless failed to be obtained" (pp. 38–39).

Kurchatov gave a summary of his research on ferroelectricity in a short summary included in his little-known book, *Problems of Contemporary Physics in the Works of the A. F. Ioffe Physico-Technical Institute*.[37]

At the end of 1932, Kurchatov began to move away from studies in the physics of ferroelectrics, which had been a veritable "gold mine" into which he had stumbled in doing research on dielectrics, and switched to the study of atomic nuclei. Already heavily involved with nuclear physics, he finished the aforementioned monograph[37] and prepared an abridged version, which was later published in France.[38]

The fourth volume of the first edition of the Physics Dictionary came out in 1938. Kurchatov was asked to write two adjacent articles:[39] "Segnetova sol" [Rochelle salt] and "Segnetoelektriki" [Ferroelectrics]. These laconic and pithy notes were his final salute to the research that had made the name of Kurchatov widely known not only in the USSR, but also far beyond its borders.

Further progress in the study of ferroelectric phenomena came in the immediately accepted studies of B. M. Vul and his co-workers on barium titanate (1944). These studies, first of all, resulted in a sharp increase in interest in possible technical applications of ferroelectrics and, second, stimulated the work of V. L. Ginzburg on the phenomenological (thermodynamic) theory of ferroelectricity, which was closely related to Landau's general theory of phase transitions. It is noteworthy that research on ferroelectricity still continues at LPTI—the "birthplace of ferroelectricity"—under the direction of G. A. Smolenskiĭ. The reader can become familiar with the current state of research on ferroelectrics from the well-known monographs of Refs. 40–44.

7. Kurchatov's first 10 years at LPTI were also marked by his exceptional book-writing activity. Perhaps his two best-known books were *Ferroelectricity*[18] (1933), which we have already discussed in several places, and *Splitting the Atomic Nucleus*[45] (1935), which is mentioned in the following pages. It is less commonly known that in 1930, Kurchatov came out with a booklet entitled *The Electrical Breakdown Resistance of Materials*[46] that was published in the "Nauka i Tekhnika" [Science and Engineering] series in Moscow. In this booklet Kurchatov gave a popular account of the principles involved in studying the passage of a current through a dielectric.

In 1935, a course entitled *Electronic Phenomena*,[47] which was accepted by Narkompros as a textbook for universities, was published in Leningrad. This course was a completely revised edition of a monograph of the same name written in 1928 by Semenov. The team of authors included Semenov, Kurchatov, D. N. Nasledov, and Yu. B. Khariton. Five of the 17 chapters in this were written by Kurchatov: "Experimental proof of the wave nature of the electron," "The wave picture of matter according to de Broglie and Schrödinger" (this chapter was written jointly by Kurchatov and Nasledov), "Electrons in metals," "Electron emission by metals," and a supplement, "Induced radioactivity." This work was apparently all related to Kurchatov's pedagogical activity, first (1927–1929) in the Physico-Mathematical Faculty at LPI and then (1935–1943) at the M. N. Pokrovskiĭ Pedagogical Institute. Kurchatov was Head of Scientific Research for the Chair of General Physics there in 1935, became Director of the Chair of Theoretical Physics in 1937, and in December of 1938 was named Director of the Chair of Experimental Physics.

In 1935, the same year that *Electronic Phenomena* appeared in print, one of the volumes of a course in general physics under the general editorship of A. F. Ioffe came out in Ukrainian. The names of three authors appeared on the jacket of this book—I. V. Kurchatov, N. N. Semenov, and Yu. B. Khariton.[48] It can be supposed that Kurchatov was responsible for, at minimum, the chapters entitled "Gases in a force field" (pp. 99–129) and "Electrical conductivity" (pp. 164–198). A direct comparison of the Ukrainian and Russian editions (both the first[49] and second[50] editions) shows that they are practically identical. Thus, counting reprints and translations, one finds 8 books that were authored or coauthored by Kurchatov published in the 5 years between 1930 and 1935!

This is all an indication that Kurchatov kept abreast of all the current theoretical and experimental research on electron theory, gas discharges, quantum mechanics, the physics of semiconductors and dielectrics, and nuclear physics. In the early thirties, when there were no special journals of abstracts in physics published in the Soviet Union, there were special abstracts sections in the pages of *Zhurnal Tekhnicheskoi Fiziki* [Journal of Technical Physics] and the journal *Physikalische Zeitschrift der Sowjetunion*, which was published in Khar'kov and printed articles in German, English, and French; the Editor-in-Chief of both these journals was A. F. Ioffe. Ioffe interested Kurchatov in working on this section of the journals. Starting with their first volumes, there were several—sometimes up to 5 or 6—abstracts written by Kurchatov in every issue (this is particularly true of ZhTF). They touched upon questions in the physics and technology of solid-state rectifiers and photocells, and also of gas discharges. In addition, Kurchatov wrote large review articles devoted to the proceedings of various (mostly Soviet) conferences, reviewing both the reports and discussions.[51,52]

IV. WORKS ON NUCLEAR PHYSICS

a) Preparatory period

1. For many years nuclear physics was represented at LPTI only by the work of D. V. Skobel'tsyn. Back in 1924, in the physics laboratory of Petrograd Polytechnical Institute, he was the first in the Soviet Union[53] to apply the important refinement to the Wilson cloud chamber of placing it in a magnetic field.[7)]

From 1925 Skobel'tsyn also worked at LPTI. Using the Wilson cloud chamber, he carried out a number of fundamental studies in the period from 1924 to 1929 on the interaction of gamma rays with matter. One of the early studies in this cycle was a measurement of the gamma-ray spectrum of RaC.[55]

In the scientific programs at LPTI before 1933, it is extremely rare to encounter research that might be related to nuclear physics; this research was clearly not of a systematic nature.

2. 1932 is often called the "golden year" of nuclear physics. Chadwick discovered the neutron and Anderson the positron. Fermi suggested an answer to the riddle of the continuous spectrum of beta-decay electrons, pointing out the possibility that the electron is created simultaneously with a neutrino in every beta-decay event. In Rutherford's laboratory Cockroft and Walton built a high-voltage accelerator and realized the first nuclear reaction (the disintegration of lithium by protons) induced by artificially accelerated particles. Lawrence and Livingston published the first detailed article on the construction of the recently invented Lawrence accelerator—the cyclotron. Urey discovered heavy water—deuterium.

It seems probable that this glittering sequence of events played a large role in Kurchtov's decision to abruptly change his field of research to nuclear physics. It must be assumed also that Ioffe had an influence in this decision. Finally, we note that even before 1932 Kurchatov began to familiarize himself with developments bearing upon nuclear research and even participated directly in them. We are speaking of the construction of high-voltage charged-particle accelerators at the Ukrainian Physico-Technical Institute in Khar'kov, where the program was directed by Sinel'nikov.

Kurchatov's direct switch to the problems of nuclear research came at the end of 1932. In November of that year a nuclear seminar started up at LPTI under his leadership. The secretary of the seminar was D. D. Ivanenko. Sessions were held 5 times a month. They were attended by up to 35 physicists (not all from LPTI). In December an order was circulated through the institute calling for the creation of a "special group on the nucleus." Ioffe took upon himself the duties of group leader, and Kurchatov was appointed his deputy (Ref. 56, p. 13). The group was made up of 10 physicists in all, including Skobel'tsyn, Eremeev, Ivanenko, and I. P. Selinov. G. A. Gamow and L. V. Mysovskiĭ were appointed as consultants to the group.

In 1933 this "special group" was reorganized into the Department of Nuclear Physics. Kurchatov became its head. He also directed the Nuclear Reactions Laboratory in this department. Kurchatov spent much time studying the literature on nuclear physics. However, in that same year (1933) he was already heavily occupied in preparing a technical base for the planned nuclear studies. It was clear that without a modern high-power accelerator there would be no possibility of rapid progress in nuclear studies at the Institute.

3. Having examined the various types of accelerators that had been developed to date, Kurchatov and A. I. Alikhanov (who had also transferred to the nuclear program with several of his co-workers) settled on the cyclotron. From roughly 1933 on, they discussed plans for constructing a large cyclotron at LPTI.

To get an idea of the ways of controlling such an accelerator and the features of its operation, Kurchatov decided to build a small cyclotron in his laboratory—a device of the same size as that described in the first detailed article by Lawrence and Livingston.[57] Their cyclotron, which was constructed at the University of California, Berkeley, had an electromagnet pole diameter of 28 cm; it was started up at the end of 1931, and by the beginning of the next year had produced protons with an energy of 1.22 MeV, a record in its day, at a beam current of 10^{-9}A.

Kurchatov entrusted the construction of the small cyclotron to Eremeev at the end of 1932. It was proposed that this cyclotron would be capable of accelerating protons to about 1 MeV. The diameter of the poles of the magnet was chosen to be 28 cm, as in the prototype in Lawrence's laboratory. The total mass of the magnet (with copper windings) was around 2 metric tons. The high stability required of the guiding magnetic field was arranged by supplying the windings of the magnet from

G. Ya. Shchepkin, I. V. Kurchatov, and M. A. Eremeev (from left to right; photo taken in 1933 or possibly 1934).

[7)]The Wilson cloud chamber was first placed in a magnetic field by P. L. Kapitsa[54] of the LPTI staff while he was working in Rutherford's laboratory. A distinguishing feature of Kapitsa's apparatus was the use of an intense pulsed magnetic field ($B \sim 80$ kG), which made it possible to obtain a rather high curvature of the tracks of α particles in the chamber.

the Institute storage battery. The calculated magnetic field strength in a 4-cm gap was 12 kOe. The vacuum chamber of the cyclotron was square in shape and contained one dee. The chamber was evacuated by a mercury diffusion pump. The rf generator supplying the dee was built using a single water-cooled tube. Its output power was 10 kW at $\lambda \sim 15$ m.

Protons were accelerated in this cyclotron to 530 keV. The beam current did not exceed a few 10^{-10} A. Kurchatov apparently did not do any physical experiments on this cyclotron, probably because of the low beam current. Eremeev wrote in his autobiography that this work was not pursued further because of a lack of funds. However, the annual report of LPTI for 1934 (PTI Archives, f. 3, op. 1, ed. khr. 32, l. 7) puts a different construction on the matter: "Further development was halted in view of the fact that the State Radium Institute was putting together a much more powerful 10-MeV facility."

b) Nuclear reactions with protons

In 1933 Kurchatov was devoting much of his attention to the construction of a high-voltage proton accelerator. His chief assistant in this work was G. Ya. Shchepkin. The main accelerator was a 500-keV cascade generator (of the Cockroft-Walton type). It was notably compact, an attribute achieved by clever placement of the separate elements and the use of kenotrons with a very small heater current, which were built on special order at the Svetlana Plant. For this apparatus Kurchatov and Shchepkin assembled a 350-keV sectional disk vacuum tube. The tube was designed at the Ukrainian Physico-Technical Institute (UPTI) by A. K. Val'ter and Sinel'nikov, together with Kurchatov and Schepkin (Ref. 45, pp. 98–99). With this apparatus Kurchatov and his co-workers made two studies.[58,59]

Kurchatov's very first study in the area of nuclear physics[58] was published in March of 1934. The goal of this study was to obtain certain data on the energy balance of the nuclear reaction B + p. To do this, Kurchatov and his co-workers studied the gamma rays produced in the reaction. They came to the conclusion that the new data were consistent with the earlier conjecture that the boron is split into three alpha particles.

The second study with the proton beam was also published[59] in 1934. Before turning to this study, let us examine the content of a related article by Kurchatov and Sinel'nikov[60] in which they analyzed all the available published data on the mechanism of the reaction Li + p. Many data had accumulated, some of them contradictory. In Ref. 60 these authors showed convincingly that there were many arguments in favor of the earlier, hastily proposed hypothesis of the Rutherford group that the reaction $^6Li + {}^1H \rightarrow {}^4He + {}^3He$ takes place (the isotope ^6Li is present at a level of ~7% in a natural lithium target). However, since the light isotope of helium (^3He) was not among the known stable nuclides, there arose the question of whether it was radioactive. The answer to this question was the subject of Ref. 59. An attempt was made to find the positron activity arising as a result of bombarding lithium by protons. A target of Li_2O was bombarded by protons at energies up to 350 keV. The beta activity of the target was measured in two versions of the experiment: at the time of bombardment—an early example of looking for radioactivity "in the beam"—and ~15 sec after the end of the bombardment. The authors concluded that the nucleus ^3He was either stable or was long-lived, with a half-life with respect to positron activity of more than 3 years. We note that the existence of the stable nuclide ^3He in nature was firmly established in 1939. The abundance of this isotope in natural (atmospheric) helium, according to modern data, is $\sim 1.3 \cdot 10^{-4}\%$.

c) Neutron-induced artificial radioactivity

In the spring of 1934, the Roman group led by E. Fermi began to publish a series of notes in the Italian journal *Ricerca Scientifica* reporting the discovery that artificial radioactivity is excited in many elements by neutron bombardment.[61-63]

The possibility of this phenomenon, we might note, was conjectured in the pioneering work of F. Joliot and I. Curie on artificial radioactivity.[64] This news was of great interest to Kurchatov. He dropped his proton-beam studies and energetically began to pursue research on the "Fermi effect." A collaboration with the Physics Department of the State Radium Institute (SRI) in Leningrad was quickly organized. This department was headed by L. V. Mysovskiĭ, who long before had designed an emanation apparatus that was in operation at SRI. Ampoules and capillaries containing radon were prepared on this apparatus for distribution to many medical institutes. It was not difficult to arrange the preparation of neutron sources of the type used by Fermi, which were in the form of glass ampoules filled with radon and beryllium powder.

Professor G. K. Trabacci, the Director of the Physical Laboratory of the Ministry of Public Health in Rome, who had supplied Fermi's group with the radon-beryllium neutron sources, was called "Divine Providence" by this group. Professor Mysovskiĭ was just such a "Divine Providence" to Kurchatov's group. The simple (but weak) radon-beryllium neutron sources served physicists faithfully for several years, until the advent of strong neutron sources based on ion accelerators.

The first two papers on neutron-induced radioactivity to come out of Kurchatov's laboratory were sent to press only three months after the appearance of the first reports from the Roman group. One of these papers[65] reported a study of artificial radioactivity in phosphorus. Some interesting new results were established that went beyond the findings of the Fermi group. In addition to the β^- activity with a half-life of ~3 h, which Fermi's group, using a radiochemical method, had assigned to silicon (so that it had to be assumed that the reaction $^{31}P(n,p)^{31}Si$, $^{31}Si \xrightarrow{\beta^-} {}^{31}P$ occurs), Kurchatov's group found an activity with a half-life of ~3 min. Naturally, they addressed the question[65] of the "isotope identification" (in modern terminology, the "nuclide identification") of the new activity. The problem was simplified by the fact that phosphorus has only one isotope (^{31}P). It was assumed that the new activity belonged to the ^{28}Al nucleus, produced in the reaction ^{31}P(n, α)^{28}Al; the basis for such an identification was the known fact that ^{28}Al has a half-life of ~3 min; it is converted to ^{28}Si by β^- decay. Kurchatov and his coauthors came to the conclusion that branching was observed in the nuclear reactions: A fraction of the phosphorus nuclei under neutron bombardment underwent a reaction of the (n, p) type, while another fraction underwent an (n, α) reaction. Branching of a nuclear reaction had also been observed earlier—in cases involving the bombardment of a target by charged particles. For example, the reaction ^{27}Al(α, p)^{30} had been known since 1929 (Rutherford and Chadwick), and in 1934 Curie and Joliot[66] established that the reaction ^{27}Al(α, n)^{30}P also occurs. However, the branching of a reaction during *neutron* bombardment was observed for the first time in Ref. 65.[8)]

In the second of the papers mentioned the Kurchatov group studied the reaction Al + n (aluminum also consists of only a single isotope—^{27}Al). Here they also obtained new data: In addition to the activity reported by the Fermi group, with a half-life of 12 min, they found an activity with a half-life >12 h. They also established that the β^- decay of the nuclide with the 12-min half-life is accompanied by γ emission at the rate of ~1 photon per decay. The energy of the γ rays was estimated by taking absorption curves in lead. This was apparently the first measurement of the parameters (relative intensity and photon energy) of γ radiation ever made for an artificial radionuclide. A radiochemical study showed that the activity with the 12-min half-life was due to magnesium, which must therefore be obtained in the reaction ^{27}Al(n, p)^{27}Mg. The activity with the half-life >12 h was most likely due to sodium, and so the reaction $^{27}\mathrm{Al}(n, \alpha)\ ^{24}\mathrm{Na} \xrightarrow{\beta^-} {}^{24}\mathrm{Mg}$ occurs. This means that here, as in the case of P + n, a branching of the neutron-stimulated reaction into two channels, (n, p) and (n, α), is observed.

Shortly thereafter, Kurchatov and his co-workers published another article,[69] refining the results of Ref. 68. It was confirmed radiochemically that the long-lived activity in the Al + n reaction was due to Na. The half-life of this activity was measured from the γ rays and found to be 12–13 h (the modern value is around 15 h).

In October of 1934, Fermi and his co-workers discovered that by placing a sufficiently thick layer of hydrogenous material—paraffin or water—between the neutron source and the target, they could increase the induced radioactivity of the target in some cases by hundreds of times [we are speaking only of reactions of the (n, γ) type]. This effect was attributed to a slowing (moderation) of the neutrons to thermal velocities as a result of multiple collisions with protons and an increase in the cross section for the interaction of the neutrons with the target nuclei with decreasing neutron velocity. This novel effect was of great practical importance for everyone working in the field of neutron-stimulated artificial radioactivity, since it made possible the study of elements having very small cross sections. It was immediately used by the group of A. I. Alikhanov at LPTI, which was conducting studies of the β spectra of artificially radioactive nuclides.

In Kurchatov's laboratory, studies of radioactivity induced by slow neutrons continued through 1935–1936. We shall not give an account here of the content of these studies.[9)] In the short period from July of 1934 through February of 1936, Kurchatov and his co-workers published a total of 17 articles on artificial radioactivity. We might add that by the end of 1934, Kurchatov had finished a monograph entitled *Splitting the Atomic Nucleus*,[45] in which he reviewed the basic experimental data obtained in the past two or three years in the field of nuclear physics.

d) Studies of nuclear isomerism

The artificial-radioactivity research of the Kurchatov group, which continued into 1935, soon spilled over in a natural way into a broader range of subjects. Kurchatov became interested in the mechanism of neutron-stimulated nuclear reactions and in questions relating to the absorption, moderation, and scattering of neutrons in matter, i.e., neutron physics as a whole. This became the primary thrust of Kurchatov's scientific studies.

Before turning to an account of the results of several of the studies from this period, we must discuss several articles which, along with studies made in other countries, opened up a very rich field in nuclear physics, in terms of both the number of publications and the scientific interest of the results. We are speaking of the study of nuclear isomerism.

Early in the year 1935, Kurchatov's laboratory turned to bromine as a target material for bombardment by slow neutrons. This element had been studied previously by Fermi's group. Kurchatov's group, however, measured not the β^- activity, as the Fermi group had done, but rather the γ activity of the resulting nuclides.[70] As in the other cases we have discussed, Kurchatov's group succeeded in obtaining new data in addition to those obtained in Rome: Besides the known activities with half-lives of 18 min and 4.2 h (chemically identified earlier with bromine), they found an activity, also due to bromine, with a half-life of 36 h. This case, however, differed from the one we mentioned earlier in that the interpretation of the result was extremely difficult. In fact, faced with the presence of only two stable isotopes of bromine—^{79}Br and ^{81}Br—and assuming that only reactions of the type (n, γ) are possible, one can easily explain the formation of two radiobromines, but the formation of a third is incomprehensible. In this respect the situation was similar to the case of two other radionuclides which had been produced at that time. These were the products obtained in neutron bombardment of indium[71] and rhodium.[72] This

[8)]Independently of the Kurchatov group, the Joliot-Curie group (Ref. 67) also discovered the phenomenon of braching of a nuclear reaction, also for the case P + n.

[9)]The most complete list of Kurchatov's publications is given in the special brochure of Ref. 1.

riddle might be called "the problem of too many half-lives." In the first papers either nothing was said to indicate that there was a problem here[72] or else it was mentioned that there were unexplained facts, but no solutions of any kind were offered. In contrast, the Kurchatov group's paper on radiobromine[70] consisted mainly of the exposition on the "problem of too many half-lives" and attempts to resolve this question. The possibility of explaining the data by the existence of nuclear isomerism was mentioned but rejected. However, by the end of the year (1935) Kurchatov had returned to this explanation.[73,74] In analyzing the problem of radiobromine in the lecture of Ref. 73, Kurchatov states: "One must apparently assume the existence of nuclear isomers—*two isotopes with the same mass number but with different structures*" (emphasis in the original).

Thus, Kurchatov first advanced the hypothesis that one of the nuclei—^{80}Br or ^{82}Br—is isomeric, and that *two of the three observed half-lives should be attributed* to this one nucleus (which particular two half-lives was also unknown at the time). In 1937 it was shown independently in two different studies[75,76] that the two half-lives ~17 min and 4.4 h must be assigned to a single nucleus—bromine 80—which is consequently isomeric; the 36-h half-life belongs to bromine 82. Kurchatov's hypothesis had thus proved to be correct.

In subsequent years the group under the general leadership of Kurchatov (the "foreman" of the group was L. I. Rusinov) did a well-thought-out series of studies of the isomerism of radiobromine.[77-80] These papers brought complete clarity to the question of the decay scheme of the isomers of bromine and confirmed the theoretical ideas on the nature of the phenomenon that were developed by Gamow and Weizsäcker.[10)]

e) Studies of nuclear reactions involving neutrons. The absorption and scattering of neutrons in matter

1. The Kurchatov group's research on neutron physics in the years 1934–1936 included a number of studies on the nature of the nuclear reactions which occur when light elements are bombarded by neutrons. The main apparatus used was the Wilson cloud chamber (one was located at the State Radium Institute, another at the Ukrainian Physico-Technical Institute, and a third at the M. N. Pokrovskiĭ Pedagogical Institute in Leningrad, constructed under the direct supervision of Kurchatov). In Ref. 82 it was reported that when lithium is bombarded by slow neutrons, the tracks of two heavy particles flying apart at 180° are observed. The authors of Ref. 82 measured the range of these particles and concluded that the reaction $^6\mathrm{Li} + \mathrm{n} \to {}^4\mathrm{He} + {}^3\mathrm{H}$ hypothesized in previous studies almost certainly does occur.

It was known that boron has an unusually large neutron-absorption cross section. A study of the reaction B + n was undertaken[83] to elucidate the nature of the interaction. It had earlier been suggested that the reaction $^{10}\mathrm{B} + \mathrm{n} \to {}^4\mathrm{He} + {}^4\mathrm{He} + {}^3\mathrm{H}$ occurs; Kurchatov and his co-workers showed that only two particles emerge in the reaction. They suggested that the reaction is $^{10}\mathrm{B}(\mathrm{n}, \alpha)^7\mathrm{Li}$. The question of the mechanism of the B + n reaction was subsequently taken up in many laboratories. Kurchatov's group returned to this question in 1938, and published a detailed article.[84] They suggested the following interpretation of the presence of two groups of α particles with different ranges: In a fraction of the cases the product nucleus (^{7}Li) is formed in an excited state, with an energy equal to 420 keV. It is now known that these conjectures about the reaction B + n were correct. (The energy of the first excited level of the ^{7}Li nucleus, according to more precise measurements, is 478 keV.)

The paper of Ref. 85 addressed an extremely important question in nuclear physics—the study of the neutron–proton interaction and the problem of estimating the cross section for the capture of a slow neutron by a proton to form a deuteron. It was noted in this paper that the experimental and theoretical data on the size of this cross section were in sharp disagreement (0.1–0.01 b and 10^{-3} b, respectively). An estimate of the same cross section based on the data on the "inverse reaction" $^2\mathrm{H}(\gamma, \mathrm{n})^1\mathrm{H}$ also yielded $\sigma \sim 10^{-3}$ b. The Kurchatov group had determined the cross section for the reaction $^1\mathrm{H}(\mathrm{n}, \gamma)^2\mathrm{H}$ by measuring the intensity of the γ rays produced in the reaction. Their result ($\sigma \sim 0.25$ b) showed that the theory of the deuteron proposed by Bethe and Peierls was in need of radical revision. This paper[85] stimulated a number of experimental studies of the n-p interaction at different energies, resulting in the discovery that the nuclear forces depend on the relative directions of the neutron and proton spins (E. Wigner, 1935–1936).

2. The Kurchatov group's papers on the absorption and scattering of neutrons in matter began to appear in print in 1935. It soon became clear that there were a number of anomalies that could not be explained in terms of the then-accepted "single-particle model" for the interaction of neutrons with nuclei. In particular, the so-called "$1/v$ law," according to which the cross section for the capture of a neutron by a nucleus should be inversely proportional to the neutron velocity v when the latter is very small, was not obeyed. Further, it was established that the measured coefficient of absorption of slow neutrons by the nuclei of various elements depended on the element used as a radioactivation detector. For a given element the largest value of this coefficient was obtained when the intensity of the neutron flux passing through the absorber was determined from the measured artificial radioactivity induced in a detector made from the same element.

The paper by L. A. Artsimovich, Kurchatov, *et al.*[86] was one of the first reports of the observation of this effect (it was published in May of 1935). In this paper it was first pointed out that among the possible explanations for these results is the hypothesis that the activation detector is selectively sensitive to neutrons of different energies—that "the absorption of slow neutrons is of a resonant character, i.e., $\sigma(E)$ has a sharply de-

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[10)] Here we give only a brief account of the study of nuclear isomerism, since a detailed review of this subject was recently published in Uspekhi (Ref. 81).

fined peak in a certain energy interval." This hypothesis was also discussed in other articles published by the Kurchatov group in the same year.[87,88]

Not long afterward, Szilard[89] and Amaldi and Fermi[90,91] published papers which presented new data on the selective absorption of slow neutrons. The existence of the effect was no longer in doubt. After a number of puzzling results were explained on the basis of this effect, and especially after the appearance in early 1936 of the fundamental paper by Bohr,[93] in which he proposed a new model of nuclear reactions and convincingly explained the physical nature of the selective absorption of neutrons, a great number of papers were published with the aim of proving and refining the new theoretical ideas (at this same time a quantitative theory of the resonance absorption of neutrons had been developed by Breit and Wigner.[93]). Kurchatov, Rusinov, and Shchepkin, working mainly at UPTI in the laboratory of A. I. Leipunskiĭ (in the years 1936–1937), made their contribution to these studies. We shall mention here a detailed study[94] of the absorption of C neutrons in silver, cadmium, and boron at various temperatures. This paper was presented at the Second All-Union Conference on the Atomic Nucleus (Moscow, September 20–26, 1937). This study was notable for the wide range of temperatures used (20.4–463 K), made possible by the liquid-hydrogen apparatus operating in the cryogenics laboratory at UPTI, which was directed by L. V. Shubnikov (this apparatus, the first of its kind in the Soviet Union, went into service in 1931).

f) Works in the Cyclotron Laboratory of the Radium Institute. The design and construction of the cyclotron at LPTI

1. In the early thirties the construction of a large Soviet cyclotron was already underway in Leningrad. In 1932, on the initiative of L. V. Mysovskiĭ, the head of the Physics Department of the State Radium Institute (SRI), The Academic Council of the SRI adopted a resolution to construct a cyclotron at the Institute. Construction began the same year. For those days it was a large device: The electromagnet had pole tips 1 m in diameter and a mass of around 31 metric tons.[96] By the end of 1935 the equipment was all in place, but the adjustment of the apparatus was proceeding very slowly. All attempts to obtain a beam of accelerated ions in the first vacuum chamber prepared at SRI (a square chamber with a single dee, having a final-turn radius r_f = 115 mm) ended in failure. The second chamber, which was circular, with an inside diameter of 600 mm, having two dees with r_f = 256 mm, was mounted on the magnet in early 1936. It was only in February of 1937, however, that a proton beam with an energy ~500 keV was first obtained; in July of that year protons were accelerated to ~3.2 MeV. However, the operation of the device was extremely unstable.

Kurchatov, naturally, was very interested in the progress of the work on the commissioning of the SRI cyclotron. He had counted on being able to use this cyclotron for work in his laboratory at LPTI, at the very least for bombarding targets with neutrons. In addition, he wanted to obtain some solid practical experience on the SRI cyclotron in order to use this experience in the forthcoming construction and operation of the LPTI cyclotron. It was difficult for him to restrain his impatience seeing how far the SRI cyclotron was from regular operation. Perhaps he thought that he might have been able to help Mysovskiĭ and his group in commissioning the cyclotron if he were participating in the organization of the group. Be that as it may, starting roughly in the spring of 1937, Kurchatov worked once a week in the SRI Cyclotron Laboratory.[11)] He gradually came to lead the work. He proposed to replace vacuum chamber No. 2 by an improved one. The new chamber was designed by Kurchatov, Alikhanov, and D. G. Alkhazov. It was constructed with a great deal of help from the machine shop at LPTI, particularly from the machinist K. N. Petrov. Chamber No. 3 had a diameter of 450 mm and was the first at SRI to be equipped with an internal target (test probe), which was inserted through a vacuum lock. The first beam to a Faraday cup placed beyond the deflector was obtained in this chamber in October-November of 1938.

In early August of 1937, Kurchatov was named Director of the Cyclotron Laboratory at SRI. The putting in operation of the new chamber was progressing well, and from early 1939 Kurchatov, as he wrote in a letter to his wife, Marina Dmitrievna, was in this laboratory from ten in the morning to eleven at night. His principal assistants at this time were D. G. Alkhazov and V. P. Dzhelepov, the latter being a co-worker at LPTI who was assigned to the SRI Cyclotron Laboratory in mid-January of 1939. Through almost all of 1939 the laboratory was engaged in a detailed study of a peculiar operating regime of the cyclotron (the deuterium glow-discharge regime), in which the cyclotron chamber was converted into an intense sources of neutrons.[96,97] It was in this period that the SRI cyclotron began to be used for scientific work. For example, uranium was bombarded with neutrons in order to study its fission and to search for transuranic elements, and ethyl bromide was bombarded for a group led by Rusinov at LPTI, which was studying the nuclear isomerism of bromine.

Although Kurchatov remarked that in cases where the cyclotron was used as a neutron source, the simpler glow-discharge operation is to be preferred to the ordinary operation with an outside ion source,[97] it was clear that this mode of operation of the device could only be considered an intermediate stage in its commissioning; it would be necessary to learn how to obtain an ion beam with the full design every (5–6 MeV for deuterons, 11–12 MeV for α particles) at a satisfactory beam current, and to bring the beam out to a target placed beyond a deflector.[12)]

On April 1, 1939, because of Mysovskiĭ's illness, Kurchatov was appointed to the post of head of the Phys-

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11) In 1937, Kurchatov and Alikhanov were engaged as consultants by the Physics Department at SRI.

12) The SRI cyclotron began to work in normal operation only at the end of 1940, with chamber No. 4 (which had an inside diameter of 800 mm).

ics Department at the Radium Institute, which was then known as RIAN,[13)] but continued as director of its Cyclotron Laboratory. He remained in this capacity until October 5, 1940.

2. Let us turn now to the plans for the construction of the large cyclotron at LPTI. As we have already mentioned, these plans came into being shortly after Kurchatov and Alikhanov switched to the field of nuclear studies. However, several years were to pass before any concrete steps were taken in this direction.

In seems very likely that near the end of 1937, the Department of Heavy Industry of the USSR adopted a resolution to allot funds and materials for the construction of a cyclotron at LPTI. We note, however, that long before the adoption of this resolution Kurchatov had done a great deal of preliminary work. He had already decided in 1936 that it was necessary to progress from approximate estimates made in the laboratory to design work, which should be handed over to specialists. The theoretical aspects of the cyclotron design and the design of the electromagnet involved many of the scientists at the M. I. Kalinin Industrial (Polytechnical) Institute—G. A. Grinberg, M. I. Kontorovich, and N. N. Lebedev from the Chair of Theoretical Physics of the Engineering-Physics Faculty, and S. M. Gokhberg, L. R. Neĭman, A. I. Blokhin, and E. M. Kel'zon from the Chair of Electrical Machinery of the Electro-Mechanical Faculty. The LPTI had specified that the cyclotron was to be capable of accelerating protons and deuterons to a final energy of 10 MeV. The working plans for the electromagnet of the cyclotron were taken over by V. K. Fedorov, the Chief of the Bureau for the Design of Direct-Current Machinery of the Elektrosila Plant; at the end of 1937 he entered into a service agreement. The proposed plans included a provision for smoothly adjusting the upper beam of the yoke of the magnet together with the upper pole and its windings to permit installation of vacuum chambers of different heights in the pole gap.[14)] Fedorov met repeatedly with Gokhberg and L. M. Nemenov to coordinate the project. In early 1938 he presented a complete set of working drawings for a magnet with a pole diameter of 1.2 m. The mass of the magnet, according to the plans, would be ~75 metric tons.

The results of the group at the Chair of Theoretical Physics were reported in Ref. 98, and those of the group at the Chair of Electrical Machinery were reported in Ref. 99 (see also Ref. 100).

Near the end of 1937, the Chief Architect of LPTI, Ya. D. Glinkin was commissioned to consider plans for the cyclotron building (Ref. 101, p. 195).

By the time plans were begun for the LPTI cyclotron (1935–1936), cyclotrons were being planned in a number of countries of Western Europe. However, in none of the scientific centers of Europe was so large a cyclotron planned as the one at LPTI. For example, plans were begun in 1934 at the College de France in Paris for a cyclotron with a pole diameter D = 850 mm (on line in 1938), in 1936 at the Institute of Theoretical physics in Copenhagen for a cyclotron with D = 900 mm (on line in 1938), and in 1936 at Cambridge University in England for a cyclotron with D = 940 mm (on line in 1939). Thus the choice of parameters for the LPTI cyclotron shows the perspicacity of the nuclear physicists at LPTI, and the boldness and sweep of their plans.

Even as the technical project of the LPTI cyclotron was reaching completion, only one cyclotron of a larger size was under construction in Europe—the one at Birmingham University, which had a pole diameter D = 1560 mm (1938–1950). In 1939 there were two cyclotrons at Lawrence's laboratory in Berkeley, with D = 940 and 1524 mm.

The workers in Kurchatov's laboratory who were occupied primarily with the cyclotron were L. M. Nemenov, an experimental physicist, Ya. L. Khurgin, a theoretical physicist, and P. Ya. Glazunov, and electrical engineer.[15)] Kurchatov arranged regular meetings of the various groups. They considered the problems of the design and construction of the electromagnet, the theory of the cyclotron, and the topography and adjustment of the magnetic field. In addition to the persons already mentioned from LII (LPI) and LPTI, the meetings were attended by D. G. Alkhazov and A. N. Murin (a graduate student of Kurchatov's) from the Radium Institute.

The Economic Council of The Council of People's Commissars of the USSR (Sovnarkom) adopted a resolution on June 7, 1939 to allocate funds for the construction of a building to house the LPTI cyclotron. By this time, plans had already been prepared for the apparatus and building, and preliminary agreements had been reached with factories for production of the equipment. The magnet for the cyclotron was ordered from the Elektrosila plant in 1939. The special "Armco" iron for this magnet was produced at the Bol'shevik Plant. In February of 1941 the magnet was finished and its bench testing begun.

On September 22, 1939, the cornerstone was laid for the LPTI Cyclotron Laboratory building.[16)] The building was designed by Ya. D. Glinkin, the working drawings made by The Department of Capital Construction at LPTI, which was headed by Ya. I. Lapkovskiĭ, and the Chief Engineer was A. F. Zhigulev. This resourceful, energetic, and highly qualified specialist played a major role in the successful construction of the Cyclotron Laboratory building. The construction work was done by the First Construction Branch of Lenmashstroĭ.

In October of 1940, Kurchatov stopped working at the Radium Institute and with characteristic vigor devoted

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[13)] In February of 1938 the Radium institute was brought into the system of the Academy of Sciences of the USSR and came to be called RIAN for short.

[14)] This feature of the design of the electromagnet of the LPTI cyclotron proved to be very convenient in studies which required rolling the vacuum chamber out of the magnet.

[15)] On July 29, 1940, Ya. L. Khurgin defended his dissertation on "The Theory of the Cyclotron."

[16)] The construction work was actually begun much earlier, so that by the time of the ceremony part of the walls were already laid.

all his energy to the supervison of the construction of the LPTI cyclotron. By the summer of 1941 the Cyclotron Laboratory building was almost finished.[102] An rf generator with a power of ~20 kW had been delivered from the group at LII. A vacuum chamber with "Pyrex" insulators (this had been mainly Nemenov's project) had successfully undergone vacuum testing. A date was set for the start-up of the cyclotron—January 1, 1942.

The war intervened...

In 1943, Kurchatov, who was then the Director of Laboratory No. 2 of the Academy of Sciences of the USSR (which is now called the I. V. Kurchatov Institute of Atomic Energy), instructed Nemenov and Glazunov to be flown into the besieged city of Leningrad to move the rf generator, the dee insulators, and sheets of brass and copper dug up out of "storage" from LPTI to Moscow. This equipment and material, which had been prepared for the LPTI cyclotron, was used in Laboratory No. 2 to build the first Moscow cyclotron, having a pole diameter of 73 cm.[103] This cyclotron went into service in 1944. It was at the time the only working cyclotron in the Soviet Union.

Kurchatov considered this cyclotron to be insufficient for general studies on the "uranium problem," an effort which he led. Therefore, the State Defense Committee adopted a resolution in 1945 calling for the urgent completion of the LPTI cyclotron. This work was directed by Alkhazov. The LPTI cyclotron went on line on November 27, 1946, and a beam of 6-MeV deuterons was obtained, with a beam current at the test probe of 250 μA.

The turn of events was such that Kurchatov, who had spent so much time and effort on its construction, never did see the LPTI cyclotron in assembled and working form.

Kurchatov's laboratory at LPTI had developed one other cyclic accelerator, which, unlike the cyclotron, was intended to accelerate electrons. This device, which was called the "quadratron" (Ref. 104, p. 13), was proposed by Ya. L. Khurgin, who had presented the idea at the nuclear seminar in 1938. Since Kurchatov himself was practically not interested in the work with fast electrons, we shall not describe this accelerator here or give an account of its construction, except to say that it, too, was interrupted by the war.

g) Fission of uranium

1. The search for artificial radioactive transuranic elements, which were supposed to be produced when uranium was bombarded by neutrons, was begun by the Fermi group back in 1934. Many nuclear laboratories soon became involved in this field of research. The unexpectedly large number of different activities that were obtained in the reactions U + n and Th + n gave rise to a complicated and tangled situation which had long sought explanation. In late 1938, Hahn and Strassmann sent their famous article[105] to press, and in early 1939 Frisch and Meitner published their no less famous letters.[106,107] A surprising—and simple—explanation had been found for this puzzle: The action of slow neutrons causes the uranium to split into two fragments of roughly equal mass. The engrossing story of the discovery of the fission of the uranium nucleus is described in detail in many papers (see, for example, the article by Gerlach.[108]).

In 1939 came a succession of very important experimental and theoretical papers elucidating the details of the fission process and revealing new facts. First of all came experimental proof of the fission hypothesis and direct measurements of the energy of fission of uranium. It was predicted, and then demonstrated, that bombardment by slow neutrons causes only the extremely rare isotope, uranium 235, to undergo fission.

In early 1939 it had already been established in several independent studies that secondary neutrons are released in the fission of uranium. It became clear that the new discoveries in the field of uranium fission could be of fundamental practical importance: They gave the first indication that the dream of exploiting the enormous store of intranuclear energy could pass from the realm of fantasy to the world of reality and, in principle, realizable processes. The words "nuclear chain reaction" flashed in the newspapers, and it was explained that such a reaction would be possible if it turned out that several secondary neutrons were released in each uranium fission event.

Immediately after the discovery of the fission of uranium, Kurchatov made an outline of which studies would have to be done first. Rusinov and G. N. Flerov pursued the problem of measuring the average number ν of neutrons emitted in each uranium fission event. They reported their result at the nuetron seminar at LPTI on April 10, 1939 (this fact was mentioned in their report[109] to the Fourth Conference on Nuclear Physics at Khar'kov in November of 1939). Since the calculation of the experimental results involved the published data on the reaction cross sections, which were not known to high accuracy, the value of ν obtained by Rusinov and Flerov had a large relative uncertainty: $\nu = 2 \pm 1$.

It is not surprising that this question—the determination of ν—was studied independently and almost simultaneously by a number of groups in different countries, using a variety of methods.

To some extent even before reliable values of ν were obtained, the conditions under which a fission chain reaction would be possible in uranium were calculated, again independently, in many scientific centers. One of the first such calculations was given in a paper[110] by Ya. B. Zel'dovich and Yu. B. Khariton of the Leningrad Institute of Chemical Physics (which had "branched off" from LPTI in 1931 and maintained close scientific ties with it). The authors, who were students of Academician N. N. Semenov, were experts on the theory of chemical chain reactions. Their paper was the most detailed and reasoned of the papers that were published. Khariton soon gave a thorough report[111] on his results to the Fourth Conference on Nuclear Physics in 1939. At the same conference a survey of the fission of uranium and thorium was given by A. I. Leĭpunskiĭ.[112] Much attention was also devoted to analyzing the feasibility of a nuclear chain reaction.

Thereafter, in 1940 and 1941, Zel'dovich and Khariton returned repeatedly to various aspects of the calculation of the nuclear chain reaction.[113-116] This set of papers constitutes a veritable encyclopedia on the subject of the fission of heavy nuclei and the theory of the nuclear chain reaction. The deep understanding of neutron physics that runs through these works do doubt stems in large measure from the participation of the authors in the animated discussions at the meetings of Kurchatov's neutron seminar at LPTI.

Initially the chain-reaction theory which had been worked out in greatest detail was that of the fission of uranium 235 by slow neutrons. The possibility of a chain reaction involving the fission of uranium 238 by fast neutrons remained an open question. To resolve this question would require, among other things, experimental data on the energy threshold for fission, on the behavior of the fission cross section as a function of the neutron energy, and on the cross section for the inelastic scattering of fast neutrons by uranium, since this process, which could shift the neutron energies to below the fission threshold, could be the main obstacle to the development of a fission chain reaction.

Kurchatov, of course, had a lively interest in these questions. Under the guidance of Flerov, T. I. Nikitinskaya, a graduate student of Kurchatov's at the M. P. Pokrovskiĭ Peadagogical Institute, measured the cross sections for the inelastic scattering of fast neutrons by several elements.

In this paper an estimate of $\sigma = 1.6$ b was obtained for the inelastic neutron-scattering cross section of uranium.[17)] On the basis of this result it was concluded that a chain reaction with fast neutrons in uranium 238 is impossible.

2. In 1939, Kurchatov assigned K. A. Petrzhak, his graduate student in the Physics Department at the Radium Institute (RIAN), to join with Flerov in devising an apparatus suitable for solving a number of problems in connection with the fission of uranium 238 by fast neutrons. For the projected studies it would be necessary to design a highly sensitive fast-neutron counter. As an initial model they took Frisch's fission chamber; they increased the sensitivity by a factor of 30–40 over that of the prototype by increasing the area of the uranium-oxide coated electrodes by a corresponding amount. A multilayered chamber was developed, in which the total area of the 15 uranium-oxide layers was 1000 cm^2.

As we know, a fission chamber can be made totally background-free, since for the proper parameters (gain and discrimination) of the amplifier the pulses from the α particles emitted by uranium and its daughters will not be registered even if several α particles are emitted almost simultaneously. The chamber thus registers only uranium fission events.

[17)]In the published version of this paper (Ref. 117) the cross section was given incorrectly as a result of a misprint.

However, in the adjustment of the apparatus Petrzhak and Flerov found that in the absence of a neutron source the chamber registered ~6 large ("fragment") pulses per hour. The experiments were done first at RIAN, on Roentgen street, and then at LPTI, which is, on the average, quieter in terms of radioactive background. Many different control experiments were done. The large pulses invariably appeared at the same average rate. Then Kurchatov, Petrzhak, and Flerov decided to announce to the scientific world the presumed discovery of a new phenomenon—the spontaneous fission of the uranium nucleus. Kurchatov first reported this at a meeting of the Division of Physico-Mathematical Sciences of the Academy of Sciences of the USSR in late May of 1940,[118] and V. G. Khlopin reported it at a meeting of the Division of Chemical Sciences at the same time. In June of that year, Petrzhak and Flerov sent a brief communication to *Doklady Akad. Nauk SSSR.*[119] Which of the uranium isotopes was responsible for the observed spontaneous-fission events remained an open question. In a more detailed article, which was sent to press in July of 1940,[120] it was suggested that the most likely candidate was uranium 235. It later turned out that it was the fission of uranium 238 that had been observed, with a fission half-life somewhat shorter than the original estimate given in Ref. 120 [$0.8 \cdot 10^{16}$ years rather than $(4 \pm 1) \cdot 10^{16}$ years].

Although Kurchatov was the supervisor of this study, had outlined "all the main control experiments and participated most directly in the discussion of the results of the research" (Ref. 120), he refused to include his name in the list of coauthors. According to Petrzhak,[121] he was afraid that in the future those who had directly carried out the experiment would be forgotten, and that only his name would remain.

Subsequently, with the aid of a multilayer fission chamber with a larger area of the working layer (5000 cm^2 of ThO_2 on 15 electrodes), Flerov and I. S. Panasyuk attempted to discover spontaneous fission of thorium.[122] They obtained only an estimated upper bound on the spontaneous-fission half-life: $T_{1/2} > 10^{19}$ years.

The discovery of the spontaneous fission of nuclei was of great importance in many fields of science. First of all, it represented a discovery of a fundamental property of nuclei—their ability to undergo a previously unknown type of radioactive decay. For heavy nuclei ($Z > 100$), spontaneous fission turns out to be the main process governing their stability. Therefore, the questions of their fission half-life and the limit of the periodic system of elements are intimately related.[123] The degree of difficulty and the very possibility of synthesizing far transuranic elements are directly governed by the spontaneous-fission half-lives of the corresponding nuclei. The ability of heavy nuclei to undergo spontaneous fission has important consequences in astrophysics and geophysics.[123]

3. At the Fifth Conference on Nuclear Physics (Moscow, November 1940) Kurchatov gave a report entitled "The Fission of Heavy Nuclei," in which he analyzed the most important papers that had been published in the year since the Fourth Conference.[124] It happened that at

the very time the foreign journals stopped publishing papers on the fission of heavy nuclei: The Joliot-Curie group had stopped publishing its papers after the beginning of the war with Germany in September of 1939, and the American physicists decided in the summer of 1940 to refrain from further publication in order to avoid giving any scientific information in this field of research to Nazi Germany. Therefore, in the section of his report that dealt with the question of the feasibility of a fission chain reaction in uranium, Kurchatov relied mainly on the calculations of Zel'dovich and Khariton and on several other Soviet papers. He indicated clearly that the answer to the question of whether a fission chain reaction was feasible was, in principle, affirmative: He saw two ways of achieving this—either by using ordinary uranium mixed with a neutron moderator of heavy water, or by using ^{235}U-enriched uranium mixed with ordinary water or possibly some other light materials (e.g., carbon) if it turned out that their neutron-capture cross sections were sufficiently small. Of course, "the road to its practical realization in the systems now under study will be fraught with enormous difficulties." After all, what was involved was the extraction, on a commerical scale, of heavy water or of enriched uranium with the isotopic content of ^{235}U increased, for example, by a factor of two (1.4% rather than 0.7%).

As we have seen, in his last report before the war, Kurchatov failed to mention only one very important idea for the construction of a nuclear reactor, which would not come up until later (leaving aside the foreign papers, which were not being published). This was the suggestion of using a "lattice" of uranium and moderator instead of their homogeneous mixture, i.e., to surround separate chunks (blocks) of uranium with the moderator. This would permit a substantial reduction of the role of resonant capture of the neutrons by the uranium 238 and thereby enable one to create a nuclear reactor using natural uranium. [In the Soviet Union, as Yu. V. Sivintsev points out on p. 36 of Ref. 125, the idea of a heterogeneous reactor was expressed in one of Kurchatov's seminars (in Moscow in 1943) by the prominant theorist I. Ya. Pomeranchuk. He and I. I. Gurevich worked out a general theory for such a system in 1943 (Ref. 2, pp. 63–64)].

4. The first scientist in the Soviet Union to devote considerable attention to questions of scientific organization in connection with the study of the fission of uranium and with the enormous potential economic significance of the results was Academician V. I. Vernadskiĭ. He considered it imperative first of all to ascertain in the shortest possible time whether there were sufficient reserves of uranium ore in the USSR. He also regarded the separation of uranium 235 from natural uranium to be a very important question.

On June 25, 1940, the Department of Geological and Geographical Sciences of the Academy of Sciences of the USSR, on Vernadskiĭ's initiative, named a "troika" with him as chairman to work out a plan for the measures that would need to be taken in regard to the possible use of intra-atomic energy.

On July 12, 1940, the "troika" returned with a special memorandum to Sovnarkom containing a number of proposals in regard to the uranium problem. A memorandum with the same date was sent by Vernadskiĭ and Khlopin to the Presidium of the Academy of Sciences of the USSR; this memorandum gives a detailed plan for the organizational measures that would be necessary in the years 1941–1942.[18]

In its July 30, 1940 session, the Presidium of the Academy of Sciences of the USSR adopted a resolution calling for the creation of a "Commission on the Uranium Problem under the Presidium of the Academy of Sciences of the USSR" (called the Uranium Commision for short). Khlopin was named chairman of this commission, Vernadskiĭ and Ioffe were vice-chairmen, and the membership included Kurchatov, Khariton, and P. L. Kapitsa (Ref. 126, p. 336).

Kurchatov, who was energetic and accustomed to strenuous work, was apparently unsatisfied with the pace at which the work on the uranium problem was progressing. Did he become acquainted with the details of the program of top-priority studies outlined by Vernadskiĭ and Khlopin in their memorandum? It is hard to say at this time. Even if he became acquainted, then evidently he and his colleagues considered it imperative to stress once again to the leadership of the Academy of Sciences the urgency of this series of studies and to augment the program in several areas. On August 29, 1940, Kurchatov, Khariton, Rusinov, and Flerov sent a letter to the Presidium of the Academy of Sciences of the USSR, addressed to its permanent secretary P. A. Svetlov. This letter was captioned: "On the use of the energy of uranium in a chain reaction" (Ref. 127, p. 199). The headings of the various points proposed in this letter for a "program of studies for the immediate future" were: 1. To determine the branching conditions for the chain in a mass of metallic uranium. 2. To elucidate the effects of the neutrons produced in the fission of uranium 238 on the behavior of a chain reaction in a mixture of uranium and water. 3. To determine the effective transverse cross sections for the capture of slow neutrons by heavy water, helium, carbon, oxygen, and other light elements. 4. To determine the conditions under which a chain reaction will occur in a mixture of uranium and heavy water. 5. To explore the question of extracting heavy water in large amounts (it was explained farther on that the amount in question would be several tons). 6. To explore the question of enriching uranium in the isotope with mass number 235.

It is easy to see that the proposed areas of research in the plan devised by the Kurchatov group had the specific goal of creating the theoretical and experimental prerequisites for the construction of a nuclear reactor.

Kurchatov was persistent and did not miss an opportunity to try to speed things up. On his initiative, at the time of the Fifth Conference on Nuclear Physics (Moscow, November 1940), still another memorandum to the

[18]The reader may learn of the details of Vernadskiĭ's activities in regard to the uranium problem in the book by I. I. Mochalov (Ref. 126, pp. 330–356).

leadership was composed, stressing the importance of the fission of uranium and the necessity of organizing extensive research in this field (Ref. 176, p. 39). The resolutions of the conference were discussed and approved by the Uranium Commission on November 30, 1940 (Ref. 126, p. 338).

The outbreak of second world war led to the complete cessation of all nuclear research in the Soviet Union.

V. CONCLUSION

From the recollections of his fellows at LPTI, it is known that in the first days following the outbreak of the war Kurchatov went to the laboratory of A. P. Aleksandrov, which was occupied with the problems of defending ships against magnetic mines, and, putting aside his work on nuclear physics until peacetime, switched over with his characteristic energy to the work of this laboratory. An account of these truly heroic efforts is given in the recent book by Tkachenko,[128] who has meticulously gathered an enormous mass of material which, in particular, permits one to follow Kurchatov's "naval odyssey." On August 9, 1941, Kurchatov and Aleksandrov arrived in Sevastopol', where they would spend three strenuous months, from late August supervising the demagnetization of ships. In November and December this work was continued at Poti and on the Caspian, and so Kurchatov arrived in Kazan', where LPTI was stationed, in January of 1942.

Some time later, the director of the tank-armor laboratory, V. L. Kuprienko, fell ill with typhus and died while on a mission to the target range where tests were being done on tank armor developed by LPTI and on methods of defending tanks from direct shell hits. Kurchatov succeeded him in this post.

The rest of Kurchatov's life story is well known: He was called to Moscow at the end of 1942[19)] and offered the leadership of the work on the uranium problem—the very problem for whose solution he and several of his colleagues at the Institute and at the Academy of Sciences had so persistently called.

Let us conclude this article with an exerpt from a remarkable document—an evaluation of Kurchatov written in 1940 and signed by Kuprienko, the Vice-Director of LPTI, and V. M. Tuchkevich, the academic secretary of the Institute:

Both in his personal scientific work and in the leadership of young scientific co-workers, I. V. Kurchatov has created a special style of work characterized by the ability to concentrate all his energies on the solution of the problem at hand and to complete rapidly the research he has undertaken. This ability of his to work with a great moral enthusiasm and at a rapid pace infects his co-workers as well, ensuring the rapid completion of assigned tasks. The results of this method of working are reflected in the fact that in his 15 years of activity, Kurchatov has done an enormous amount of work in various areas of physics (IAÉ Archives, f. 2, op. 1, ed. khr. 169, p. 33).

The characteristic features of Kurchatov's working style that were mentioned in this evaluation stemmed from his exceptional talent as an organizer. These traits, strengthened in the 15 years of working with the excellent staff of LPTI, together with his talent as a physicist and his outstanding personal qualities, ensured the rapid and successful completion of the work which was so vital to his country—the work on the uranium problem.

Igor' Vasil'evich Kurchatov was the pride of the Leningrad school of physics, the pride of Soviet physics, and a notable scientist and citizen of the Soviet Union.

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[19)]The exact date of Kurchatov's call to Moscow is not found in any of the biographies mentioned. We are relying on information on Kurchatov's missions to Moscow which is preserved in the orders to LPTI in regard to missions of the institute staff.

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