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INFORMATION REPORT INFORMATION REPORT

[redacted]

COUNTRY	USSR	REPORT NO.	CS DB-3,633,414
SUBJECT	The Problem of Uranium Isotope Separation by Means of Ultracentrifuge in the USSR	DATE DISTR.	8 October 1957
		NO. PAGES	61
		REQUIREMENT NO.	RD
DATE OF INFO.	1945 to September 1956	REFERENCES	
PLACE & DATE ACQ.	[redacted]		

[redacted] EVALUATIONS ARE DEFINITIVE. APPRAISAL OF CONTENT IS TENTATIVE.

SOURCE: [redacted] Source has not been graded. Appraisal of Content: 3.

[redacted]

[redacted]

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Approved for Release
Date 8/10/79

[redacted]

STATE	X	ARMY	X	[redacted]	X	AIR	X	FBI		AEC	EV	[redacted]

EG-180

(Note: Washington distribution indicated by "X"; Field distribution by "#".)

INFORMATION REPORT INFORMATION REPORT

C00010316

[redacted]

REPORT NO. EG-1802

Copy 1 of ___

COUNTRY USSR

DATE DISTR. 22 MAY 1957

SUBJECT The Problem of Uranium Isotope Separation by Means of Ultracentrifuge in the USSR

NO. OF PAGES 60

PLACE ACQUIRED [redacted]

NO. OF ENCLS. (LISTED BELOW)

DATE ACQUIRED BY SOURCE Mid-1946 to September 1956

SUPPLEMENT TO REPORT NO.

DATE OF INFORMATION 1945 to September 1956

THIS IS UNEVALUATED INFORMATION

SOURCE [redacted]

[redacted]

[redacted]

[redacted]

[redacted] EG-1802

[redacted]

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[redacted]

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TIMETABLE OF HIGHLIGHTS IN CENTRIFUGE DEVELOPMENT

1946	March 1946	Steenbeck group organized in Sinop.
	November 1946	Centrifuge project initiated in Sinop by source. Steenbeck leaves for Artsimovich's Laboratory 2 in Moscow.
1947	November 1947	Mechanical proof for centrifuge given by source. Steenbeck returns from Laboratory 2 in Moscow to Sinop.
1948	Spring 1948	Construction of separation apparatus with concentric rotors.
	June 1948	Steudel assigned the design and construction of separation apparatus with point bearing.
1949	March 1949	[redacted] assigned the design and construction of separation apparatus with hollow tube and point bearing.
	April 1949	Successful tests with Steudel's and source's centrifuges.
	June 1949	Steenbeck report to Beriya suggesting the use of centrifuge plant as topping for gaseous diffusion plant.
1950	September 1950	Beginning of five-meter long rotor centrifuge for topping plant. "Small Factory."
1951	January 1951	Steenbeck group obtains results with a three-meter rotor which they had promised for a five-meter rotor.
		Meeting of Technical Council in Moscow. Steenbeck suggests that centrifuge is ripe for semitechnical application and is rebuked by Malyshev.
		Steenbeck learns from Thiessen that Soviets have solved problem of gaseous diffusion plant without centrifuge.
		Endurance tests with centrifuge.

[redacted]

	September 1951*	Steenbeck and Scheffel and best qualified Soviet assistants, together with centrifuge equipment, transferred from Sinop to Kirov plant in Leningrad.
1952	January 1952*	[redacted] transferred to Kirov Plant in Leningrad. Steenbeck group ordered to build two centrifuge batteries of six long-tube units each.
	March 1952*	[redacted] refuses to work on long-rotor centrifuges and begins work on short-tube centrifuge. Development of scoops and molecular pump.
	June 1952*	Demise of long-rotor centrifuge project.
	August 1952* September 1952*	[redacted] completes construction of 25-cm centrifuge and obtains satisfactory separation results with low power consumption.
1953	March 1953*	Technical Council meeting at which it is intimated that Steenbeck group's centrifuge was selected over Kamenev's project.
	May 1953*	Separation tests on 45-cm centrifuge continued by Soviets who plan construction of three more 45-cm centrifuge units. Withdrawal of Steenbeck group from work in Kirov Plant. Beginning of quarantine period.
	September 1953*	Steenbeck group transferred from Leningrad to Kiev for work in Physical Institute of the Academy of Sciences. Quarantine period.
	26 July 1956	Departure of Steenbeck group from Soviet Union. Issuance of 50,000 rubles bonus to Steenbeck and 15,000 rubles to each of his assistants.

Note: All dates marked with an asterisk should probably be plus one year, as is indicated from the chronology obtained from [redacted] in STIB Report OLB/B056/189, dated 12 October 1956. In source's recollection of events after the beginning of 1951, he seems to have lost a year. Dates in the timetable above and throughout the body of the report, however, have been recorded as source gave them. It is hoped that the discrepancy in chronology can be cleared up in future interrogation of source.

[redacted]

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INTRODUCTION

1. Upon entering Berlin in April 1945, the Soviets immediately began to marshal German industrial equipment for dismantling and shipment to the USSR. Their booty included human resources also, for early they began to conscript leading German scientists for work in the USSR. It is difficult to discover whether any form of coercion was used by the Soviets in recruiting atomic specialists. It would appear that conditions existing in early postwar Germany favored this Soviet enterprise. There were men, like Gustav Ludwig Hertz, who had eagerly awaited the defeat of Nazi Germany and were willing to aid any power that had contributed to the defeat of the Nazis. There were others, like Prof. Peter Adolf Thiessen, who had been compromised by high-level association with the Nazi regime and feared that the Western allies would call them to account. Such men thought that they would be safer with the Soviets; whatever the Soviets' faults, uncompromising moralism was not one of them.

2. The Soviet office which had its headquarters in Berlin-Gruenau and was headed by Lt. Gen. Avraamiy Pavlovich Zavenyagin appears to have contacted only the leading German scientists who had remained in Berlin or in the Soviet Zone of Occupation. Those men served as focal points, as their associates then volunteered to accompany them to the USSR.[1]

3. It is of interest to note that several German groups, though they allegedly came to the Soviet Union to engage in free research, were incorporated into the native Soviet nuclear research program, although many of these Germans had no previous experience in the field. Not only did they fill an existing gap in the Soviet research program but, according to source, they constituted a complete autarky on the problem of isotope separation. The German groups by themselves covered the entire field, pursued

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1. An item of interest is the question whether the Germans who went to the USSR knew that they would work on nuclear research for military application. Returning Germans have claimed that the German specialists had been invited to engage in pure, free, and general scientific research in the USSR, and that it was only after Hiroshima that Minister of Internal Affairs Lavrentiy Pavlovich Beriya summoned leading Germans and pointed the direction for their research. It is evident, however, that most German returnees have a personal stake in this question: they are awaiting certification as political refugees in West Germany. This certification would be difficult to obtain if they admitted (1) that they volunteered for work in the USSR and (2) that they volunteered to do nuclear research. Since the present source was candid by nature and [redacted], it was hoped to obtain an objective clarification of this point. He did not join the atomic research group, however, until [redacted] and thus his information, which confirmed the previous accounts, was secondhand.

[redacted]

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[redacted]

every available avenue of approach, and were completely self-sufficient in respect to isotope separation.[2] Although source had little information on the activity of German groups outside of Sinop and Agudzeri he believes they did stop short of the problem of constructing the atomic bomb itself, which in the USSR was a Soviet monopoly. From this perspective, the selection of German scientists by Lt. Gen. Zavenyagin appears to be more than a mere coincidence.

4. The German groups of specialists that came to the USSR were augmented by PWs. Although most of these were mechanics and laboratory technicians, there were also scientists included, i.e., men who had their doctorates or Dipl. Ing. degrees. [redacted]

5. A third category of scientists joined the Soviets, in addition to the volunteers (or conscripts) and the PWs. Max Steenbeck had been arrested by the Soviets and, together with other leading Siemens company officials, had been incarcerated in a concentration camp in Poznan, Poland, where von Siemens (fnu) died and where Steenbeck was rapidly becoming a physical derelict. The latter sent to his warders a technical paper which somehow came to the attention of high-level Soviet scientists, including Artsimovich (fnu), who recognized Steenbeck's caliber. Steenbeck was released from prison to join the nuclear research institute in Sinop.

6. The research institutes set up at Sinop and Agudzeri were in the beginning, at least, fully German institutes. The bulk of the personnel was German, the directorship was German, and even the designation of the institutes was German. Thus, the institute in Sinop at which source worked was called Obyekt A, after its German director Manfred von Ardenne. Likewise, the institute in Agudzeri was called Obyekt G, after the Russification (Gerts) of Prof. Hertz's name.

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2. In Agudzeri the following were investigated:

Isotope separation by gaseous diffusion - by Reinhold Reichmann and Heinz Barwich.

Isotope separation by countercurrent gaseous diffusion - by Hertz.

In Sinop the following were investigated:

Isotope separation by electromagnetic separation - by Manfred von Ardenne.

Isotope separation by gaseous diffusion - by Thiessen.

Isotope separation by ultracentrifuge - by Max Steenbeck.

[redacted]

7. The limitations on the German director's authority stemmed either from the peculiar nature of security regulations or from the director's ignorance of Soviet administrative procedures in operating the nuclear research institute. In these aspects, the German director's authority was limited, and a Soviet administrator was his coadjutor. In technical or scientific matters the German director had unfettered control except that assignments for research were issued by the Soviets in Moscow.

ORGANIZATION OF SOVIET ATOMIC RESEARCH

8. Soviet research groups existed parallel to the German nuclear research groups. Source had little insight into these Soviet groups, but he believes that they were institutes operated by the USSR Academy of Sciences. While the Soviet groups worked as competitors to the Germans, there was no hostility between German and Soviet groups. Source identified the parallel groups for the following isotope separation projects:

Method	Soviet	German
Gaseous Diffusion	Probably [Isaak Konstantinovich]. Kikoin	Peter Adolf Thiessen Heinz Barwich
Electromagnetic	Artsimovich	von Ardenne
Ultracentrifuge	Lange[3] (until 1948?) Kamenev (fnu) (after 1950?)	Steenbeck

The division by nationality was not rigid. For example, Steenbeck spent a year and a half in 1946-1947 in Laboratory 2 with Artsimovich to assist the latter in his electromagnetic development.

9. The competition between the Soviet and German groups was one-sided, for while the Soviets received the technical reports of their German counterparts, the Germans received no reports from their Soviet counterparts and little or no information as to their actual progress and accomplishments.

10. In the early days, the German nuclear research groups were under the control of the MVD Ninth Directorate. Later this organization was superseded by the First Chief Directorate /of the Council of Ministers of the USSR/. Leading officials in the Ninth Directorate, and possibly also in the First Chief Directorate, were Lt. Gen. Zavenyagin and Maj. Gen. Zverev (fnu). To supervise the technical side of the work of the German groups, the Ninth Directorate and later the First Chief Directorate had a number of scientific Referenten (specialists); among these were /Vasiliy Semënovich/ Yemelyanov and /Aleksandr Ivanovich/ Novikov, the latter being

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3. [Possibly Prof. F.F. Lange.]

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especially concerned with centrifuge development. The administrative problems of the German specialists were handled by [Col.] [Mikhail Mikhaylovich] Kuznetsov, who dealt with the Germans on matters such as leave, salaries, working conditions, and the negotiation of employment contracts.

11. Source did not know the top organization to which the parallel Soviet groups were subordinate. He does not believe that this was the MVD Ninth Directorate, which he thinks was organized because of the security problems involved in engaging foreign nationals on highly classified projects. This, he said, would explain why the MVD was selected to organize the supervisory Ninth Directorate.

12. Directing the research work of both the Ninth Directorate (or later the First Chief Directorate) controlling the German nuclear research institutes in the USSR and of the Soviet counterpart was the Technical Council of the Council of Ministers of the USSR. It was the Technical Council which periodically or in ad hoc sessions reviewed the work of the Germans or issued new assignments. Since Germans and Soviets were working in parallel on similar problems, it was also the task of this body to evaluate the German version of a Soviet project and to decide which solution was preferred. Thus, sometime in 1948, the Technical Council selected the Thiessen barrier for mass production. The decision was not always clearly in favor of one or the other national group, but the better parts of two projects could be coupled. This happened with the electromagnetic method of isotope separation. In 1949-1950 the Technical Council found the separator of the Soviet Artsimovich to be on the whole superior, while the ion source developed by von Ardenne was preferred to that of Artsimovich. In source's opinion, the electromagnetic isotope separation work which continued past this date made use of the von Ardenne ion source.

13. Insofar as the Technical Council decided when a German project had been satisfactorily completed, it also influenced the point at which the Germans were to be withdrawn from a given project. Originally it was planned that the German groups were to work on nuclear development only up to the laboratory model stage. For reasons of security, they were to be removed from contact with any given project as soon as it entered what the Soviets called the semitechnical development stage. This plan, however, was not too carefully observed; possibly the urgency with which the Soviet sought a solution of the atomic bomb problem was such as to override strict security considerations. When [Thiessen] found the solution to the problem of his laboratory model barrier, he was not withdrawn but instead saw the barrier through the semitechnical stage and possibly even through the production stage in Elektrostal. The same applied to [Steenbeck.] When the latter negotiated with Minister of Internal Affairs Beriya in 1949 for a centrifuge that would carry the enrichment of U_{235} from 50 percent to 90 percent, the contract finally agreed on called for Steenbeck to remain with the project until the factory was built.

[redacted]

[redacted]

14. Source is convinced that the German atomic research groups in the USSR constituted a very important part of the Soviet nuclear research effort. He does not believe that theirs was a peripheral role, such as training Soviet scientists or preparing paper studies to which the Soviets might eventually turn whenever their own efforts had led to a cul-de-sac. For this reason, he is convinced that the Germans operated with real figures on concrete and immediate Soviet problems. For example, the external dimensions of the barrier, the pressure ranges, and the corrosion properties given to Thiessen by the Soviets were real ones, and so were the magnitudes with which Barwich operated in his calculations pertaining to the gaseous diffusion cascade. That the Germans were not working in a theoretical vacuum but were in the midst of the actual Soviet problem becomes eminently clear, according to source, from the circumstances surrounding the assignment to Steenbeck of the centrifuge "small factory" in 1949.[4] Steenbeck knew that the Soviets were willing to pay any price in order to obtain weapons-grade material, and that their gaseous diffusion plant was not producing U_{235} with an enrichment greater than 50 to 60 percent. He refused to negotiate with anyone but Beriya, for he wanted concessions (of a personal nature) which no one in the USSR with whom the Germans had dealt up to that time, i.e., Lt. Gen. Zavenyagin or the men of the Technical Council, could give. Beriya summoned Steenbeck and agreed to his demands. This, source repeated time and again, was evidence that the Germans were working not on a problem of secondary importance but were in the midst of the actual Soviet atomic effort, and that the leading Germans, i.e., Thiessen, Steenbeck, Hertz, Barwich, and possibly von Ardenne, knew at every stage of the game the exact nature of the Soviet program. There is only one limitation to this categorical statement. All Germans were operating with a specification for a yield of one kilogram of U_{235} metal per day. Although the Soviets did not, and could not, operate with an unknown factor on the technical specifications, such as pressure ranges, corrosion properties, etc., they could and most likely did operate with such a factor in respect to production. Source is convinced that one kilogram per day was merely nominal, and that this factor, at least in the early period, was less rather than more than one kilogram.

ORGANIZATION OF THE TECHNICAL DEPARTMENTS IN SINOP

15. The first German group to arrive in Sinop was the one headed by Manfred von Ardenne. As early as May or June 1945, he had been invited by the Soviets to the USSR for consultations, and once there either was not permitted to return or was inveigled into remaining. Von Ardenne had left behind in Germany his assistant, Fritz Bernhardt, who was now instructed by von Ardenne to prepare the equipment of the institute in Berlin-Lichterfelde for shipment to the USSR and to collect whatever old hands he could find to work under von Ardenne in the USSR. Among the German scientists that joined von Ardenne were Bernhardt, Herbert Reibedanz, and Gerhard Jaeger.

16. Later in 1945 Prof. Thiessen arrived with his group which included Werner Wittstadt, Hans Bartel, and Ludwig Ziehl.[5] Though von Ardenne had the idea of enrolling all new arrivals on his own project and under his supervision, Thiessen refused to subordinate himself to von Ardenne.

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4. For details, see page 25.

5. Ludwig Albert Wilhelm Ziehl is EEG-353.

[redacted]

17. In January 1946, Steenbeck arrived. (The vicissitudes of Steenbeck from his Poznan prison cell until he finally reached Sinop were not remembered by source.) Steenbeck, too, refused to subordinate himself to von Ardenne. Self-centered and aware of his own ability, he answered the questions of subordination with, "I am the physicist of us two," alluding to von Ardenne.

18. While both Thiessen and Steenbeck were willing that von Ardenne carry the official title of director and handle the administrative side of the institute, in scientific matters they would brook no interference, accepting for themselves the titles of subdirectors.

19. One more small German group, composed of biologists and headed by Wilhelm Menke,[6] had come to Sinop with von Ardenne's group. The group of biologists specialized in nuclear radiation problems, and shortly after Steenbeck's arrival they were transferred from Sinop to Sungul to join another group of Germans under Nikolay Vladimir Timofeyev-Resovskiy.

20. The liaison between the Ninth Directorate and the Sinop Institute and Obyekt G in nearby Agudzeri was maintained by Gen. Aleksandr Ivanovich Kochlavashvili. As, in the early days, little equipment or supplies were available, Gen. Kochlavashvili took a personal and very active part in the procurement of these. While in the early years it was difficult to obtain supplies, it was, nevertheless, astounding that the Soviets were able to procure as much as they did in the face of the general destruction of the Soviet economy. This was additional evidence of the significance attached by the Soviets to the work of the German nuclear research institutes.

21. While the institute was being set up, the first assignments were being issued. In no case, however, was a specific project forced upon a German group. Once Beriya had pointed the way to atomic research, each group leader more or less suggested to the Soviets which isotope separation method he thought he wanted to investigate. That few Germans chose the same method does not mean that the Soviets directed the division, but simply that a German scientist preferred to investigate a method not yet selected rather than to enter into competition with another German scientist.

22. The three main German groups in Sinop began work around mid-1946 on the following projects:

a. Von Ardenne - Electromagnetic Separation Method

The electromagnetic separation project of von Ardenne was completed in 1949. The Technical Council opted in favor of Artsimovich's separator but found von Ardenne's ion source to be superior. Around 1950, von Ardenne and part of his staff went to Elektrosila for one and one-half years to build the ion source on a semitechnical scale. Afterwards von Ardenne engaged in electron microscopy. In this connection he attempted to reassemble the electron microscope which had been dismantled in Siemens and shipped to Sinop. For a long time he was unsuccessful, because he was unable to procure the missing spare parts. Still later, von Ardenne constructed a mass spectrograph which was successful and earned him the enmity of Werner Schuetze,[7] the German mass spectroscopy specialist in Agudzeri.

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6. Wilhelm Menke is a STIB source.
7. Werner Schuetze is REG-314.

[redacted]

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b. <u>Thiessen - Gaseous Diffusion Method</u>

In connection with gaseous diffusion isotope separation we encounter the exception to the rule stated earlier as to "no competition among the Germans in the USSR," for Reinhold Reichmann (died in 1948) working in Agudzeri evidently had also chosen this as his topic. Thiessen's barrier was completed sometime around 1949.[8] In the opinion of source, the gaseous diffusion method, especially Thiessen's barrier, was the first of the various isotope separation methods in the USSR which yielded satisfactory results. From the Smyth Report it was known that this was also the method which was used in the US; the Soviets, who had previously regarded the isotope separation race as an open one, decided henceforth to put all available resources into the gaseous diffusion method. Disregarding security principles, they permitted the Germans to continue with the project past the laboratory-model stage. After 1949, Thiessen was frequently absent from Sinop and probably supervised the introduction of the barrier into mass production. Sometime in 1950, the Thiessen group in Sinop began experimenting on corrosion and passivation. These experiments were, in source's opinion, connected with corrosion problems encountered in the Soviets' gaseous diffusion cascade in Kefirstadt /Verkh-Neyvinskiy, N 57-15, E 60-15/. For this purpose they were supplied with "a pump as used in the cascade....the pump came from the cascade." Without being able to recall any technical details, source knows that the final corrosion experiment of Ziehl and Helmut Hepp was successful. This experiment, according to source, showed "that the cascade could now operate." Source does not know, however, whether this successful experiment was based on original work of the Thiessen group in Sinop or was simply a check performed by the Thiessen group on experimental findings obtained elsewhere by a Soviet group working on the problem of corrosion. Source learned from Thiessen much about the latter's work, although during the very brief interrogation he was unable to recall any detailed specifications. Barwich collaborated very closely with Thiessen, and very early and for a long time occupied himself with calculations for a gaseous diffusion cascade. Source had only hearsay information about these calculations, but he believes that he heard from Barwich or Thiessen that the Soviet cascade operated with a pressure between 10^{-2} to two millimeters of mercury.

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8. According to source, the decision was made by the Technical Council in 1949 "plus or minus one year." From Ziehl's Report No. EG-1758 it is evident that the Thiessen barrier was introduced into Zavod 12, Elektrostal in mid-1948, which means that the Technical Council's decision came earlier than mid-1948.

9. [redacted]

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e. Steenbeck Group

The manner in which Steenbeck joined the Soviets has been discussed earlier. The theoretical paper which he prepared in prison for the purpose of effecting his release dealt with a Wirbelrohr (whirling tube), a rotating gas discharge. When he began work in Sinop, however, he did not take up this project which evidently had little competitive chance as an isotope separation method. Many years later this method was made a topic for a Kandidat nauk dissertation by one of Steenbeck's best but unidentified Soviet assistants.[10] The Steenbeck group was formed during June 1946, when its principal members had been sifted from Soviet PW camps.

(1) The leading German personnel of the Steenbeck group were:

Steenbeck;

Herbert Emil Becker, who was transferred to Sungul [N 55-54, E 50-45];

(Dr) Eberhard Melchior, who left in 1949/1950;

[redacted]

(Dr) Steudel who left in 1949/1950; Steudel had earlier been with von Ardenne's group, but in 1948 was incorporated into Steenbeck's group after differences had arisen between Steudel and von Ardenne.

(2) The Design Section of the Steenbeck group was composed of the following:

[redacted]

10. Source could not recall the Soviet's name and referred to him by the nickname Triefauge used by the Germans because of the Soviet's thick eye glasses.

11. [redacted]
12.
13.
14.

[redacted]

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[redacted]

Ferdinand Kafka, who left in 1949 or 1950

(3) The German group was augmented by unidentified German PWs, most of whom were replaced by Soviets during 1948.

(4) The Soviet element of the Steenbeck group was originally very small. When Germans were removed, the Soviet element was increased, and by 1950 the Steenbeck group numbered about 40 Soviets. Of these, source identified the following scientists:

Andreyev (fnu);

/Shaley Savovich/ Burdiashvili;

Gogichayshvili (fnu);

Grigoryan (fnu);

Kirvalidze (fnu);

Morinzon (fnu);

Triefeuge;

plus one or two unidentified mathematicians.

ORGANIZATION AFTER 1950

23. Source could add no additional information to the now familiar picture of how in 1950 the institutes in Sinop and Agudzeri were combined under the directorship of /Vladimir Vasilyevich/ Migulin, and how gradually the Soviets assumed even greater control over the institutes. Source, however, was able to confirm that the combined institutes were henceforth known under the title "NII-5."[16] Even though the Steenbeck group transferred from Sinop to Leningrad in September 1951 and January 1952, for purposes of administration the group continued to correspond for some time with Migulin, and whenever reference was made in Leningrad to the institutes near Sukhumi, the term NII-5 was used.

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15. [redacted]

16. The designation NII-5 /Nauchno-issledovatelskiy institut-5 = Scientific Research Institute No. 5/ was reported by [redacted] in Report No. EG-1685.

[redacted]

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EARLY NONCENTRIFUGE ACTIVITY OF THE STEENBECK GROUP

Troepfchen Methode

24. The first project which Steenbeck selected and worked on in Sinop was an isotope separation method of a countercurrent diffusion type which he referred to as the Troepfchen Methode (droplet method). A thin vertical liquid stream enters a tube in which it breaks up into drops. These drops evaporate and the heavy and light fractions of the fluid evaporate at different rates. The model gases which were used for this method were either a chlorine or a bromine compound. Dr. Becker and the Soviet assistant Andreyev were assigned by Steenbeck to work out this method. By the end of 1946 Becker was transferred away from Sinop, but the project continued and became more and more a personal domain of Andreyev. In fact, by 1948 Steenbeck was ordered to leave the project completely in the hands of Andreyev, who then continued on this work in Sinop until 1949-1950, when he, together with his equipment, was transferred to an unidentified installation in Leningrad.

25. That the Soviets made this Troepfchen Methode for isotope separation a Soviet monopoly does not, in source's view, imply any serious Soviet interest in this method, not even as a potential. Instead, this move involved a bit of local patriotism coupled with an inferiority complex, from which the Soviets in Sinop are supposed to have suffered at this time. All constructive work was being done by the Germans, and Gen. Kochlavashvili thought that here perhaps was one project of which it might be said that a Soviet had performed the work. Source is convinced that even at the time of Andreyev's transfer to Leningrad in 1950, he had not as yet produced any positive experimental results but was artificially "held just above water" and, furthermore, that as long as men of the caliber of Andreyev work on this method it will never amount to anything.

[redacted]

Radiation Enrichment Measurement Method

26. Dr. Trattner [redacted] began in mid-1946 to develop a method for measuring the enrichment obtained by Andreyev and Dr. Becker with the Troepfchen Methode. The principle applied was Neutronenbestrahlung (activation analysis). Both the normal and enriched material were irradiated with a strong neutron source, and became radioactive; thus the difference in the degree of radioactivity acquired by the isotopes was a measure of their enrichment. The work was successfully completed in 1949, and a calibrated apparatus built on this principle was used by Andreyev henceforth in his work.

27. Dr. Trattner [redacted] continued to work on the process until 1948 when he left Sinop, and the finishing touches were put on it by the Soviet Grigoryan. Though this method can be used for any enrichment measurement of the kind of model gases used by Andreyev, it is likely that the meter was not mass produced and received no application outside the institute in Sinop.

Centrifuge Enrichment Measurement Method

28. At the same time that [redacted] Dr. Trattner began to work on the above project, Dr. Dames worked on another enrichment measuring method using the density centrifuge. The principle applied was as follows: The pressure rise of a liquid in a centrifuge was utilized to compare the comparative enrichment of droplets or particles. Depending on their density, these droplets or particles would come to an equilibrium at definite (given) radius inside the centrifuge. Before reaching successful completion, this development was dropped in 1949 when Dr. Dames was transferred from Sinop.

Steenbeck's Consultantship with Artsimovich

29. Steenbeck had been in Sinop just long enough to introduce these topics when, in the beginning of 1947, he was transferred to Laboratory 2 in Moscow. There he worked for one to one and one-half years as consultant to Artsimovich. The latter knew of Steenbeck's reputation as a world-renowned specialist in the field of gas discharges, and thought Steenbeck might aid him in his electromagnetic isotope-separation project. Though Steenbeck told source of his work with Artsimovich, source was unable to recall at the time of the interview any details except that Steenbeck's contribution to the Artsimovich electromagnetic separator was a net or cage to be inserted in the path of the ions, so as to concentrate the ion beam. Source never met Artsimovich nor his Soviet assistants, but from Steenbeck he knows that Artsimovich was a physicist of great stature, whom even the self-assured Steenbeck accepted as his superior. Source also recalls that Artsimovich, according to Steenbeck, was a man with whom it was

possible to debate sensibly on political topics. Steenbeck returned to Sinop in the first half of 1948. Artsimovich continued with his electromagnetic isotope-separation work and, as has been mentioned earlier, the Technical Council selected his separator in the competition with the von Ardenne apparatus sometime in 1950, plus or minus one year.

CENTRIFUGE DEVELOPMENT WORK IN SINOP

Mechanical Proof of the Centrifuge

30. In November 1946, [redacted] turned to centrifuge development. The very first experiments, source recalls, began before official authority for the project had been received from the Ninth Directorate in Moscow. The imprimatur came in February 1947. It must be noted that a centrifuge for isotope separation had been Steenbeck's idée fixe ever since he came to Sinop. Even before he came to Sinop, he seems to have suggested to the Soviet a centrifuge with extremely long rotors, anywhere from five to ten meters, but evidently the Soviets had little faith in the centrifuge, and Steenbeck himself could not quite decide whether to do centrifuge research or to concentrate on the Troepfchen Methode; at any rate, he did not start work on the project before departing for Moscow to aid Artsimovich.

31. The reason for the Soviet reluctance may have been that their own centrifuge research program conducted by the German expatriate Lange was not successful. Source knew that Lange had been working on centrifuges with long rotors for about ten years, and his project was called off sometime in 1948 or 1949. Source did not know details of the Lange apparatus but had the impression that Lange used a long rotor with a device of some kind inside the rotor.

32. Evidently the Soviets, though skeptical, relented and permitted the Steenbeck group this side-line activity on centrifuges only as a concession to Steenbeck while he worked on Artsimovich's project. [redacted]

33. The first year's activity [redacted] was simply experimental work of a mechanical nature with the aim of proving that it was possible to get long tubes to spin at a reasonably high speed. It was hoped that once this experimental proof was given, the Soviets would take the centrifuge seriously and not just tolerate it as an eccentricity on the part of Steenbeck. By the end of 1947, [redacted] report which concluded that it was possible to spin rotors at high speed. He had taken a tube of 90 cm length with a diameter of 58 mm and had operated it at circumferential speeds of about 300 meters per second.

[redacted]

34. At about the same time [redacted] which gave the impetus for Soviet interest and which released the resources in terms of manpower which the Soviets had earlier withheld or only grudgingly given to the centrifuge project, Steenbeck returned from Moscow, having been reassigned to Agudzeri. It is tempting to assign a causal relationship between Soviet recognition of the centrifuge's possibilities and the release of Steenbeck to concentrate on this work. Source is sure, however, that Steenbeck came even earlier [redacted] There is no doubt, however, that the Soviets knew of the progress [redacted] as Gen. Kochlavashvili visited [redacted] almost daily during this period.

35. When Steenbeck returned to Sinop, the Troepfchen Methode was abruptly taken out of his hands and turned over to Andreyev. Steenbeck was furious, as at this time he still did not have full confidence in the centrifuge and would have liked to keep the two methods. This would have given him the opportunity to turn from one method to the other, should one of the methods prove impossible of solution. As time went on and the centrifuge process made rapid strides, Steenbeck, of course, was glad that he did not need to concern himself with the Troepfchen Methode.

36. Source did not know specifically what Soviet office authorized the Steenbeck group to concentrate on the centrifuge. He cannot recall that a special Soviet commission visited Sinop in this connection. Zavenyagin and Zverev were in Sinop repeatedly during the early years, but source doubts whether their visits were specifically related to the centrifuge project. Source did know from discussion with Steenbeck, however, that Zavenyagin at a relatively early date became the Soviet patron of Steenbeck's centrifuge project and fought for it against considerable opposition in high circles. Source had no details to support this assertion except for a remark made by Zavenyagin at a much later visit to the effect, "I was always convinced that something could be done with the centrifuge." In the final analysis, the Soviets probably felt that the centrifuge was a gap in their isotope separation program which could now be filled.

Isotope Separation by Centrifuge

37. It was only of academic interest to know that it was possible to spin rotors. The Soviets were not so much concerned whether a rotor could be spun; they wanted separation results. Thus, when Steenbeck returned from Moscow, he supervised the design and construction of a separation apparatus. [redacted] Henceforth, the division within the Steenbeck group which lasted until the end of the centrifuge

[redacted]

[redacted]

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project in 1953 was as follows:

a. Theoretical work performed by Steenbeck with mathematical assistance from Dr. Melchior until 1949-1950.

b. [redacted]

c. Auxiliary equipment by Scheffel.

Soviets joined these groups in ever increasing numbers. It must be noted that Steenbeck's theoretical work did not always or even normally precede the experimental work [redacted] As often as not, the experimental findings were ahead and the theoretical explanations followed.

38. The separation centrifuge which was built at this time had a short rotor which was fabricated from Duralumin.. and consisted of two concentric tubes. The over-all diameter of the tubular system was 60 mm. The gap between the tubes was approximately 6 mm. The overall length was 30 to 40 cm. The operating speed was 1100 cycles per second, which corresponds to a peripheral speed of 200 meters per second. The rotor operated in a high vacuum. A sketch of the centrifuge appears on page 20. NO

39. The countercurrent flow which took place between the walls of the cylinders was produced by a temperature gradient provided by two spiral water-tube systems (one for hot, one for cold) wound about the housing. At the beginning of the experiment, the unit was filled with UF_6. At the end of the experiment, the upper receptacle was cooled with liquid air and the enriched material was transported from the top by a system of tubing which is not depicted. Finally the tubing below the receptacle was clamped off, and the receptacle was sent to the measuring laboratory for analysis.

40. The reason for the experiments with this concentric-tube rotor was that at this point Steenbeck's theory had not progressed to a point at which he was able to predict the performance of a tube without a concentric cylinder on the inside. He was still assuming a flow between infinitely long planes rather than between cylinders. From the very beginning, the material used for the centrifuge was Duralumin, which was found to be satisfactorily corrosion stable in respect to UF_6. Passivation studies were not made by the Steenbeck group, as it was felt that, contrary to the gaseous diffusion method, corrosion was a relatively minor problem in centrifuges, especially as the UF_6 used in the apparatus was confined inside the rotor and did not come in contract with the bearing. Another reason for using Duralumin was that they wanted to build as lightly as possible.

[redacted]

[redacted]

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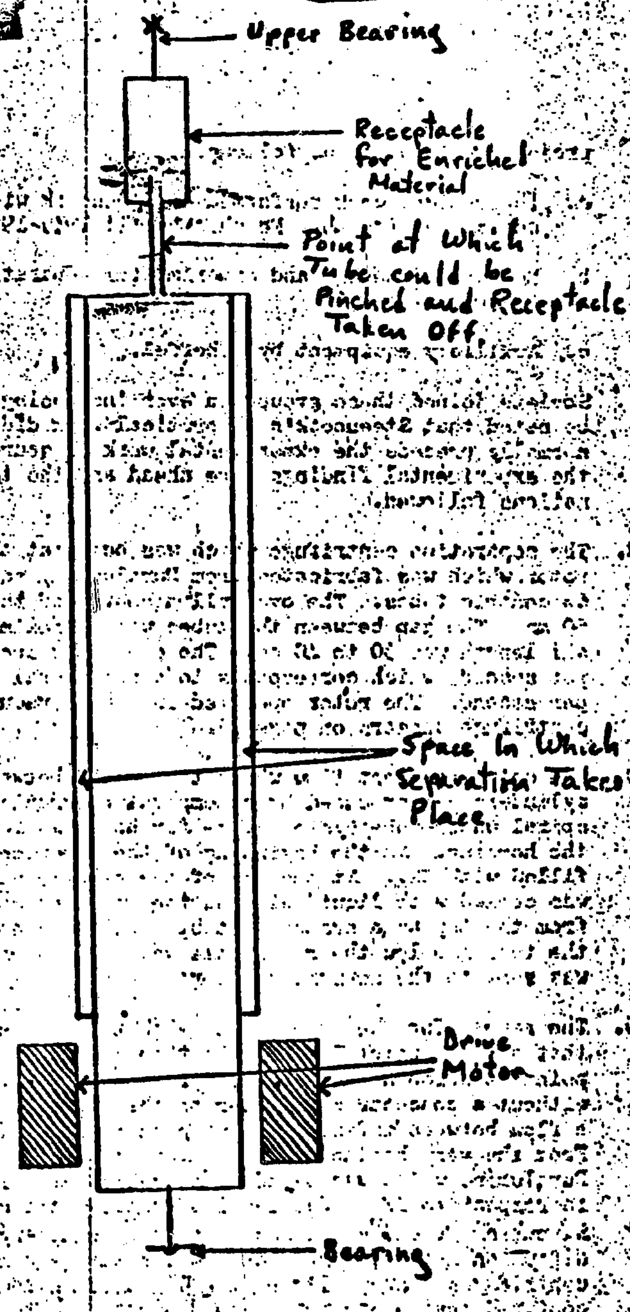


CONCENTRIC TUBE CENTRIFUGE
MEMORY SKETCH

[redacted]

41. Work on the first separation apparatus lasted about one year until 1949. For the first six months, the experimental results were unsatisfactory. Sometimes the machine separated UF_6 isotopes and sometimes it did not, and it was not possible to establish any regular or predictable behavior. The percentage which was occasionally obtained was just barely high enough to be measurable. During this period of "sometimes yes and sometimes no," the Soviets still doubted that it was possible to separate UF_6 with a centrifuge and began to put pressure on Steenbeck. No one in the USSR had yet been able to show that it was possible to separate UF_6 with the centrifuge. Even Lange, in source's opinion, had only separated model gases.

42. After about six months, in summer 1948, Steenbeck suddenly discovered a fundamental error: a temperature difference existed between the exterior and the interior tube; the exterior tube was warmer. This temperature difference gave rise to a mixing of the material which had already been separated inside the centrifuge. The error was corrected by making the inner tube warmer than the outside tube, and separation results were then obtained which corresponded to the expected enrichment. Above all, the centrifuge separated each time. The enrichment was measured by means of the alpha counter designed by Dr. Werner Hartmann and the mass spectrometer developed by Dr. Schuetze, both of Agudzeri. The best enrichment obtained was about six percent, wherein this figure is defined as $\alpha -1$ (separation factor minus one), expressed in percentage, and is the difference between the isotopic ratio of the heavy end and the light end. As, however, the quantity of the fraction enriched in the light isotope was very small as compared to the heavy gas in these early experiments, it was assumed for the sake of simplification that the gas fed into the machine had the same concentration as the depleted fraction.

Steudel's Separation-Test Centrifuge

43. One result of the Soviets' pressure on Steenbeck to produce predictable separation results was that Dr. Steudel, too, was given the assignment to build another centrifuge for separation tests. The mechanical parts for the apparatus were supplied by source and the electrical parts by Steudel, who also carried out the separation experiments by himself. The apparatus was of a short-tube type. The novel feature introduced by Steudel was a magnetically suspended centrifuge, wherein the suspension was regulated by means of a photoelectric cell. The entire centrifuge system was enclosed in a glass container.

44. The rotor used by Steudel was not made of concentric tubes but was a single hollow tube. Steudel required nine months for this development which, source commented, "was a very long time."

[redacted]

45. A rough memory sketch of the Steudel centrifuge appears on page 23. The following points on the drawing should be noted:

a. Housing

The rotor system was enclosed in an evacuated glass tube. The over-all dimensions of the apparatus were alike, or possibly somewhat smaller than, the dimensions given for the centrifuge on page 25.

b. Bearing

Source was unable to recall whether this bearing was still of the type in which the spiral was rigidly attached to a wire or was of the type in which the spiral slightly pressed against the wire.

c. Dampers

A piano wire passed through the entire length of the centrifuge. Along this wire were attached dampers to prevent oscillation.

d. Extraction Mechanism

There was no mechanical contact between the rotor and the extraction mechanism or the feed-in. Liquid-air cold fingers led into the top of the assembly near the point at which the gas emerged from the rotor. The light UF_6 fraction was immediately condensed at the cold parts. A similar system for the extraction of the heavy isotope fraction was probably provided at the bottom of the apparatus. The enrichment obtained with the Steudel centrifuge was about six percent.

e. Drive

A larger motor was located near the top and outside the housing of the assembly and drove the rotor.

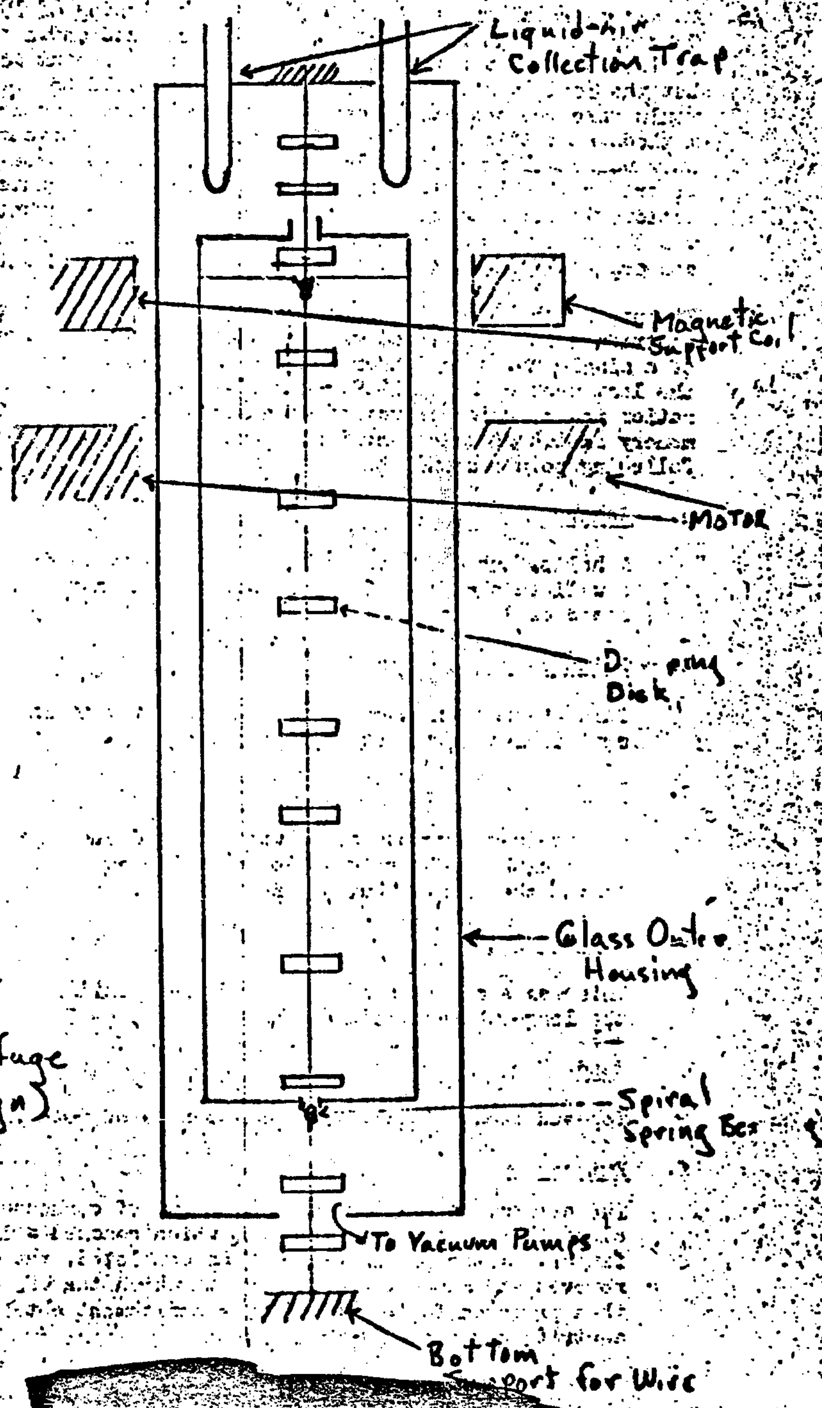
f. Rotor

The rotor was a hollow tube with dimensions similar to those given for the centrifuge on page 25 below but probably a bit smaller. The circumferential speed was about 200 meters per second.

g. Magnetic Support Coil

The rotor was supported by this coil.

[redacted] EI-1802



Early Magnetically Suspended Centrifuge (Steudel Design)

[redacted]

The Point-Bearing Centrifuge

46. As source recalled, the time around the beginning of 1949 was a very critical one for Steenbeck. Steenbeck had shown that he could spin rotors and that he could separate uranium isotopes, but the Soviets still doubted that results could be obtained which would make the centrifuge competitive with other isotope separation methods. At this time the Soviet position was, "Either you can show us something reasonable, or we will shut the book on the centrifuge." They did not, however, set up any technical specifications in terms of separation factor or power consumption. This was the time when Steudel was still assembling his machine, and source, since mid-1948, was making mechanical tests with long rotors.

47. Steenbeck [redacted] build a centrifuge in competition with Steudel. This centrifuge made use of a single hollow cylinder and ran on a needle-point bearing. [redacted] A rough memory sketch of this centrifuge appears on page 25 . The following points should be noted as pertinent to this sketch.

47-1. a. Rotor

A hollow tube, 300 mm in length and 58 mm in diameter, with a wall thickness of 0.3 mm, operating at a circumferential speed of 1100 cps.

b. Point Bearing Bottom

This bearing was similar to the bearing used in the later centrifuges described on page 38.

c. Drive

A large asynchronous motor was attached somewhere along the length of the rotor. Source believes it was located inside the housing but was not certain.

d. Top Bearing

This was a spring bearing with damper similar to that used in the long-tube centrifuge described on page 38.

e. Feed

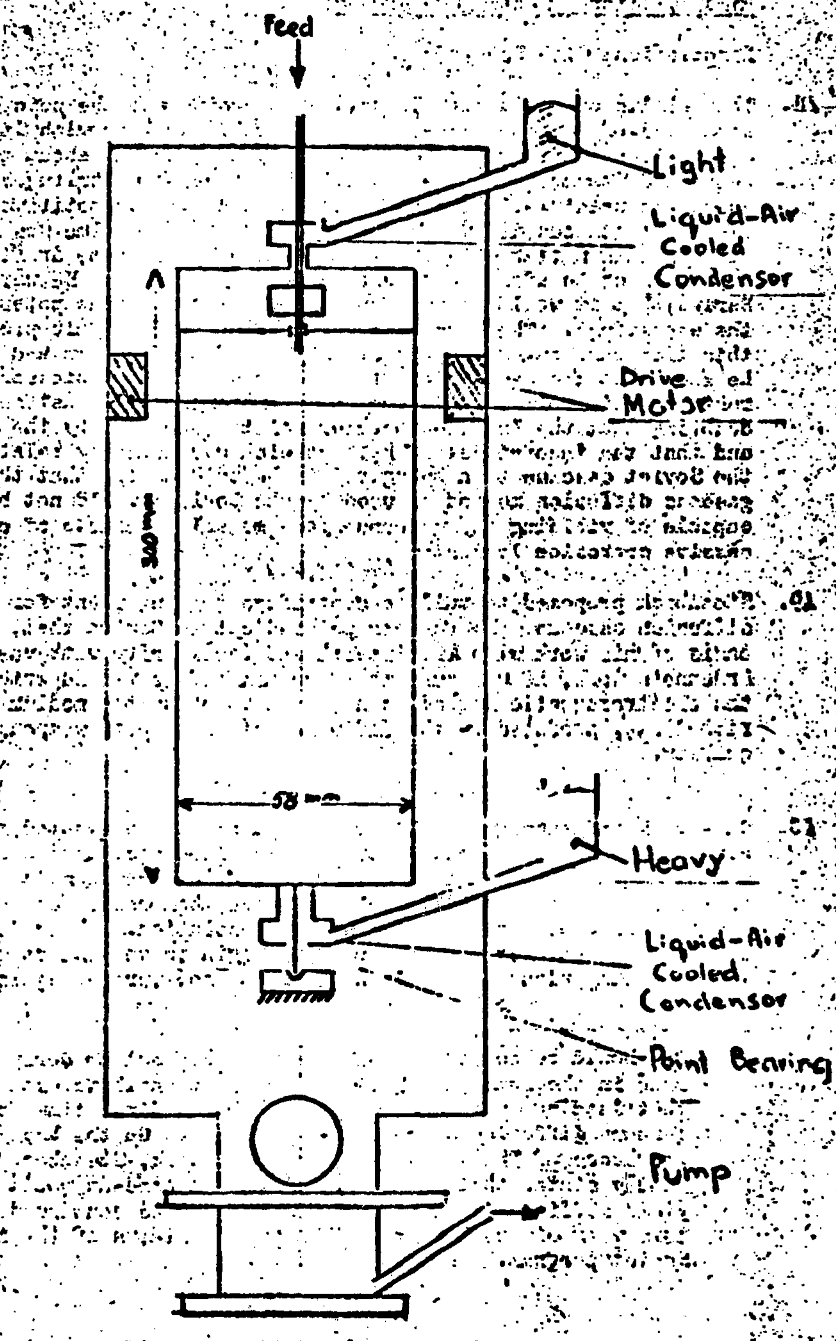
Top-end feed directly into hollow axis of rotor.

f. Extraction

The extraction was performed by means of condensation directly along the top of the rotor into which reached a liquid-air cold finger. After the experiment was completed, the cold fingers were removed and submerged in water, in which the UF_6 dissolved and was then prepared for analysis. The enrichment obtained with this centrifuge was about eight percent.

[redacted]

[redacted]



HOLLOW-TUBE POINT-BEARING CENTRIFUGE
(Rough Memory Sketch)

[redacted]

"Small Factory" Centrifuge Project

Negotiations with Beriya

48. The results obtained with Steudel's apparatus and the point-bearing apparatus [redacted] together with Steenbeck's calculations on long rotors which were completed at about this time, convinced Steenbeck of the feasibility of using centrifuges as a means for uranium-isotope separation and of its competitive status vis-a-vis gaseous diffusion. He thought that the time was now ripe to push this matter with the Soviets. He, therefore, drafted a report which he addressed directly to L. P. Beriya. He warned Beriya that it would be indefensible to disregard the potential of the centrifuge. The circumstances in the Soviet atomic program at this time were favorable for Steenbeck. From Thiessen and Barwich he knew that the cascade built on the basis of the Thiessen barrier must, at this point, be encountering extremely high wastage. Never doubting that the Thiessen barrier was the one used by the Soviets and that the theoretical work of Barwich was actually related to the Soviet cascade then being built, he told Beriya that the gaseous diffusion method as used by the Soviets would not be capable of yielding any weapons-grade material because of excessive corrosion losses.[17]

49. Steenbeck proposed to build a centrifuge topping plant for the diffusion cascade. He further pointed out to Beriya that, on the basis of his work with Artsimovich and familiarity with von Ardenne's work, he was sure that the centrifuge was superior to the electromagnetic method as an agent to bring the medium enriched U_{235} produced by the diffusion cascade up to weapons-grade quality.

50. Steenbeck had turned directly to Beriya because he wanted to have concessions of a personal nature and realized that Beriya alone could make these. He gambled that the Soviet predicament with the diffusion cascade was such as to make them ready for any concessions in order to get a satisfactory solution. While Steenbeck ostensibly went over the head of Zavenyagin in connection with this topping plant proposal, he did not, however, go against the

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17. It should be noted that Steenbeck's proposal to Beriya was made in the summer of 1949. It was not until November 1949 that leading Germans were shown, for the first time, the gaseous diffusion cascade in Kefirstadt. On the basis of his knowledge of Thiessen's and Barwich's work, Steenbeck confidently stated that the Soviet diffusion cascade would not yield sufficiently high enrichment. He was convinced that this work of the Germans reflected the status of the native Soviet program.

[redacted]

[redacted] EG-1802

wishes of Zavenyagin. The report, although addressed to Beriya, went through the regular channels via the Ninth Directorate, or the First Chief Directorate. Zavenyagin, from the beginning the patron of the centrifuge development, deemed Steenbeck's proposal sufficiently important to forward the matter to Beriya. It was Gen. Kochlavashvili, the government plenipotentiary in Sinop, and evidently a close friend of Beriya, who advised Steenbeck to turn directly to Beriya.

51. Steenbeck was summoned to Moscow and, after a two months waiting period, was summoned to Beriya in the Kremlin. This meeting took place towards the end of July or the beginning of August 1949. Though source was told of the meeting, he had forgotten many details; thus, he did not recall who was present except for Gen. Kochlavashvili. He stated that if Generals Zavenyagin and Zverev were present at the meeting, they were "very small fish" in this assembly of MVD notables. There were no discussions of technical problems; it was clear that decisions relating to the centrifuge had long ago been made by the Soviets and that Steenbeck was called only to get the decisions of the Soviets and to present his personal demands. In this connection Steenbeck had been authorized by his principle German assistants to negotiate on their behalf. Steenbeck offered to build for Beriya a five-stage cascade of centrifuges with rotors of approximately five meters length to enrich the U_{235} material from 50 percent to 90 percent. This project the Germans called the "small factory." Source did not recall the planned concentration of the waste from this topping plant, but said it would be fed back into the gaseous-diffusion plant at the appropriate point. The Soviets did not set a time limit or any other specifications. It is evident, according to source, that while the Kefirstadt cascade was not producing 90 percent U_{235},[18] this does not imply that they obtained even 50 percent enrichment. It may well have been 60 percent or 70 percent, but in that case the Soviets would have reduced the number of stages of the centrifuge topping plant.

52. In return, Steenbeck wanted to have a concrete commitment on his own and his assistants' return to Germany. Specifically he wanted to have his daughter returned to Germany to attend school. Furthermore, he wanted a definite date for the return of the remainder of his family, and he wanted contracts which clearly stipulated the duration of his assistants' service in the USSR.

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18. Early, source had stated 96 percent as final enrichment, but later thought that in 1949 the Soviets still spoke in terms of 90 percent and that 96 percent was a figure that came in conjunction with later work.

[redacted]

53. Beriya agreed on all points and, to Steenbeck's surprise, offered to return his wife and the rest of his family at the same time that his daughter left for Germany.[19]

54. In the period that followed, all resources required by Steenbeck were put at his disposal. The best measure of this can be obtained in terms of the man hours made available and the total monetary cost of the project. Whereas earlier Steenbeck received workshop time at the Sinop institute only grudgingly (about 500 hours per month), after the Beriya meeting he was given 3,000 hours per month of workshop time and actually used as much as 5,000 hours per month. Also in the period up to Steptember 1951, 7,000,000 rubles in all were expended on the centrifuge group. It is true that Steenbeck lost all German assistants [redacted] but as mentioned earlier these were replaced by Soviets. The group was organised as follows:

Theory

Steenbeck with two Soviet mathematicians [redacted]

Separation Tests

Kirvalidze

[redacted] The centrifuge [redacted] turned over to Kirvalidze to determine optimal operating conditions.

55. Starting with tubes of 60 cm, they moved to 90 cm, 1.50 m, and finally three-meter rotors. This was the maximum length with which separation tests were carried out by the Steenbeck group in connection with this project.

Description of the Long-Rotor Centrifuge

56. There is, in the opinion of source, a basic difference in the philosophy of design of long-tube centrifuges as opposed to short-tube ones. The long-tubes must go through a number of critical speeds in coming up to their operating frequency, while the short tubes go through no critical speeds. For this reason, a long rotor cannot be fabricated from one tube but must be made of interconnected short pieces separated by elastic joints. The use of these elastic joints keeps the critical speeds low and permits the rotor to be successfully accelerated through them.

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19. The actual contracts were signed in November 1949. [redacted]

57. A rough memory sketch of the long-tube centrifuge is given on page 30. The length of the long rotor was 300 centimeters and its diameter was 58 millimeters. The rotor was driven by a synchronous rotating electromagnetic field which acted on a steel impeller.

58. The rotor itself was made of Duralumin. The rotor rested on a needle bearing, similar to that developed by Beams, which operated entirely within the hard vacuum. The bearing will be described on page 40, Point 3.

59. The rotor itself operated in a high vacuum, of about 10^{-5} millimeters of mercury, provided by a diffusion pump located on the bottom of the machine. The top bearing consisted of a damper with a spring-steel wire, which operated within a three-millimeter opening in the top cover of the rotor. A countercurrent flow was obtained by a temperature gradient maintained by an external warm and cold water system.

60. The depleted fraction was removed near the bottom (warm) end, and the enriched fraction from the other end. This material was condensed on copper pins cooled with liquid air and was drawn off into ampules for enrichment tests. Multiple ampules were provided which permitted continuous operation.

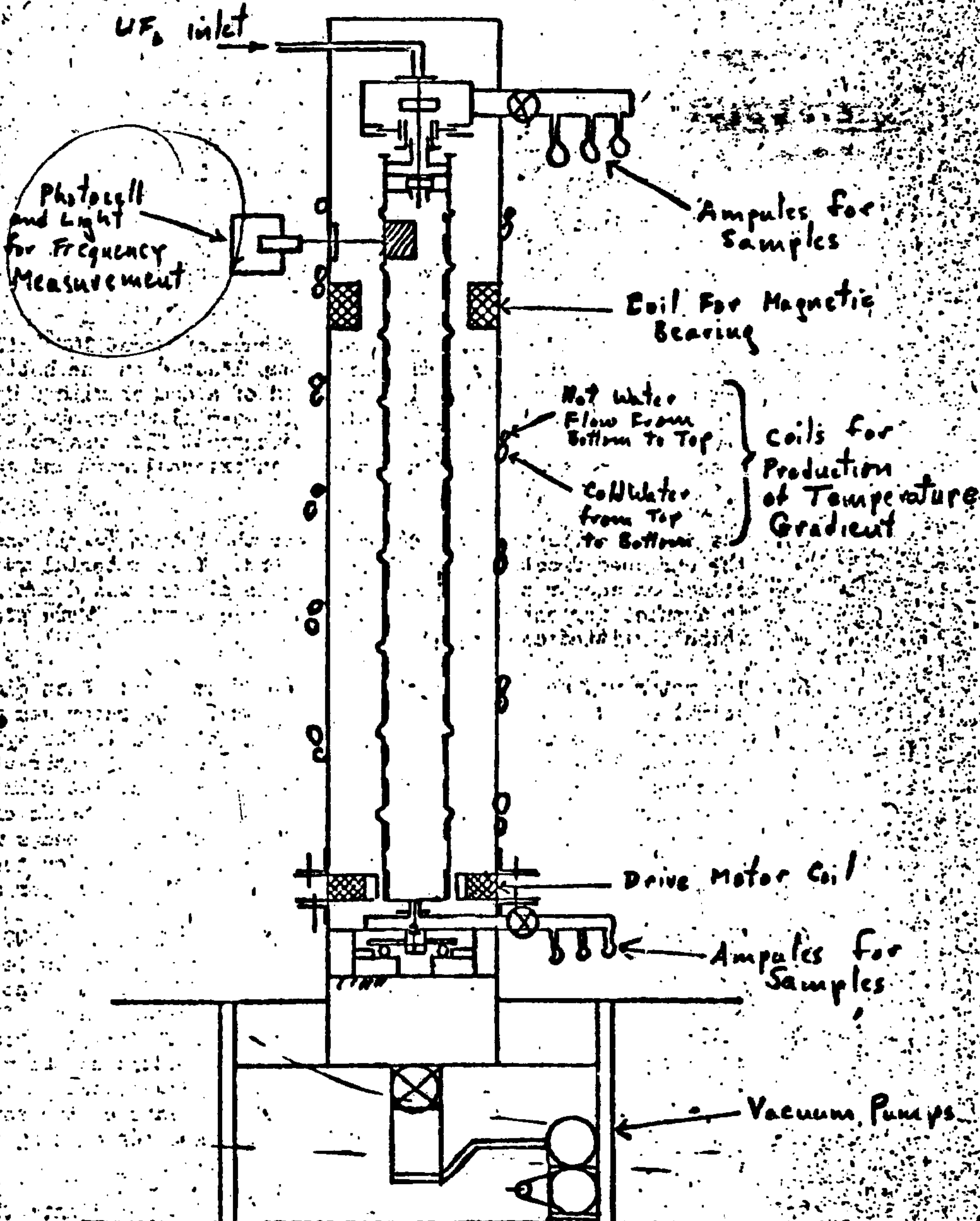
61. The rotor was made of ten pieces, each 28 cm long. Each elastic joint, or sylphon, was two centimeters long. The rotor was made of extruded Duralumin with a 0.3-mm wall thickness. The sylphons had a wall thickness of 0.15 mm, and their outer diameter at the point of maximum bulge was 75 mm. The rotor's outside diameter was about 58 mm, and it was operated at peripheral speeds of 220 to 250 meters per second. The outer parts of the sylphons were operated at speeds of 340 meters per second. Separation tests were made, however, only at rotor speeds of 220 meters per second.

62. The long rotor was tested for a lifetime operation of 3,000 hours. This was two times the required lifetime fixed by the Soviets. A check was made at each 1,000 hours during this test. The wear on the needle of the bottom bearing was a few hundredths of a millimeter during the first 1,000 hours; but, after 1,200 hours of operation, further wear was so small that it could not be measured. Source said that their measurement capability was accurate within a few microns. No wear was detected on the plate of the bottom rotor which was made of steel of the type used for cutting tools (the Soviet term was <u>babetit</u>).

63. The long rotor went through one critical speed per joint; the highest critical speed was about 400 cps. The critical speeds depended upon the stiffness of the joint and on the mass of the intermediate pieces.

[redacted]

[redacted] ID-1802



LONG-ROTOR CENTRIFUGE
Not to Scale
Rough Memory Sketch

64. The sylphons were made in the machine shop by pressing rubber into an aluminum tube mounted within a special jig. This operation was repeated five times to obtain the desired distention of the tubes. The wall thickness of the sylphons was 0.15 mm. The outer diameter of the bulges was up to 74 mm, while the outer diameter of the cylindrical portion was 58 mm. The later sylphons were made for a sleeve fit into the rotor tubes, while the earlier ones were threaded.

Assembly and Test of Long-Tube Centrifuge

65. The assembly and particularly the adjustment of the long-tube centrifuges were incomparably more difficult than those of the short-tube models. The sylphons, tubes, and covers of the long-tube centrifuges were manufactured in the workshop and delivered to the laboratory. Individual rotating tests and wall-thickness machining were done on special jigs designed for the purpose, and then the assembly of the rotor began. First two tubes were joined by a sylphon and rotated in air at fairly low speeds to check for alignment. Errors were corrected by bending the joints by hand until the two short pieces satisfied the requirements. This process was repeated with another pair of tubes, and then with two other sets of three tubes each. Then one of the two-tube sections was attached to one of the three-tube sections and the aligning process done once more at the new joint. This was repeated to make a second five-tube section, and then the two five-tube sections were joined and tested to make the completed rotor. Caps were applied as for the short-tube model, and the rotor was inserted into the housing. A window was located near the bottom of the housing to aid the insertion of the needle into the bottom bearing.

66. Countercurrent flow was maintained by a longitudinal temperature gradient of 50° C distributed over the 300-cm length of the rotor. This temperature gradient was maintained by two water streams flowing outside the centrifuge in tubes perhaps about 0.8 cm in diameter. The maximum temperature used at the top (warm) end was about 70° C.

Operation of Long-Tube Centrifuge

67. The maximum separation factor α achieved with the long-tube centrifuge was 3.0. The optimum value for the separation factor (separation factor at maximum separating power)[20] was found to be

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20. Source defined separating power L as proportional to $(\alpha-1)^2$ times $\frac{dM}{dt}$. A more complete discussion of this term is given in cascade theory (see page 43).

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2.5. The rate of extraction of material enriched in the light isotope was 0.5 milligram per second for this case. A recalled graph of separating power plotted against transport of the light stream is given on page 33. The experimental data yielded approximately the separating power predicted by theory.

68. By the beginning of 1951, it was found that the three-meter tube centrifuge yielded the enrichment which Steenbeck had earlier promised to supply with five-meter length rotors. It should be noted that those Soviets who understood the centrifuge project (source mentioned only Novikov and Yemelyanov) were well satisfied with the project by late 1950 when the rotor was three meters long and even earlier when it was only 1.5 meters long. Nevertheless, pressure still came for Steenbeck to continue to increase the rotor's length. Source explained that this was a classical example of bureaucratic inertia. Steenbeck had promised five meters, and this is what the Soviets insisted he produce. Steenbeck, to whom long tubes had become an idée fixe, never seriously demanded calling a halt when the three-meter length was reached. Another reason for the Soviet's insistence on five meters, even though three meters were performing what had been promised for five meters, was that at about this time (beginning of 1951) the centrifuge project had ceased to be of great importance. The Soviets had solved the loss or corrosion problem in the gaseous-diffusion cascade. Source pointed out that the fact that the contracts with Steenbeck's assistants [21] were signed in November 1950 indicates that at that time the centrifuge topping plant was still of interest, and that the solution to the diffusion cascade problem must have come in the following two or three months. The three-meter length tube, however, was not ready in November 1950.

69. In the beginning of 1951, Steenbeck learned from Thiessen that the diffusion-cascade problem had been solved. In retrospect, Steenbeck should have had an inkling that this was the case, as a result of the Technical Council meeting which occurred probably in December 1950. Steenbeck was at that time immoderately taken to task by V.A. Malyshev, who evidently headed this council meeting, after Steenbeck had declared that the centrifuge

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21. The negotiations along the general lines approved by Beriya were made between the Germans and Col. Kuznetsov of the Ninth Directorate. Source specified that he would work on the centrifuge for the topping plant until the laboratory scale was completed. Thereupon he would enter into a six months quarantine period. Scheffel and Steenbeck contracted to remain until the plant was built. The actual signing was performed in Sinop by Gen. Kochlavashvili.

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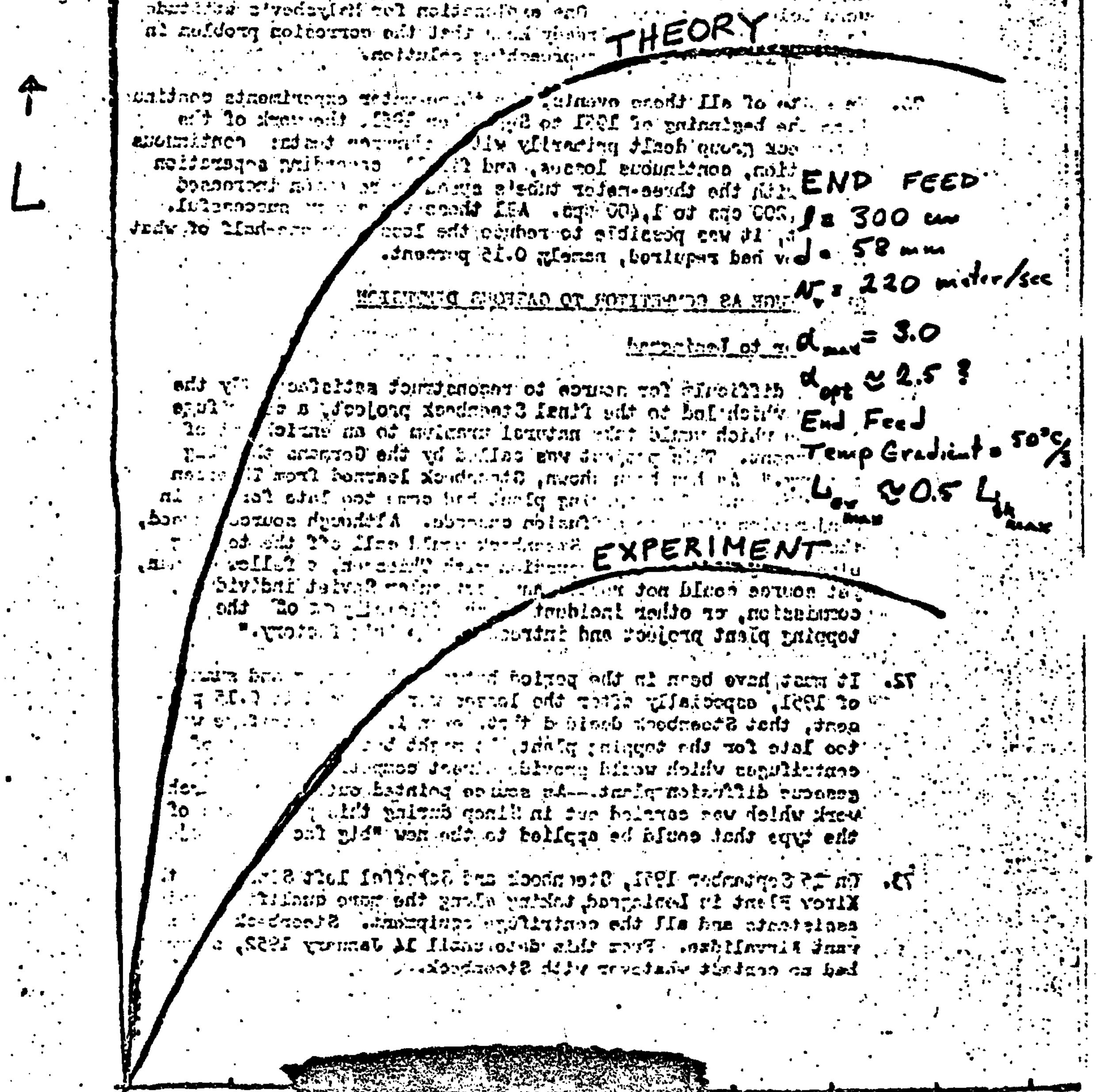
SEPARATING "POWER" = L

LONG-TUBE CENTRIFUGE



[redacted] sec⁻¹]

was ready to be taken out of the laboratory and translated into semitechnical scale. Malyshev asked him how he dared make such a suggestion without first having made endurance tests, and Malyshev insisted further that Steenbeck show that the losses were below 0.3 percent. One explanation for Malyshev's attitude is that he apparently already knew that the corrosion problem in the cascade was rapidly approaching solution.

70. In spite of all these events, the three-meter experiments continued. From the beginning of 1951 to September 1951, the work of the Steenbeck group dealt primarily with endurance tests: continuous separation, continuous losses, and finally cascading separation tests with the three-meter tube's speed or rotation increased from 1,200 cps to 1,400 cps. All these tests were successful. In fact, it was possible to reduce the losses to one-half of what Malyshev had required, namely 0.15 percent.

CENTRIFUGE AS COMPETITOR TO GASEOUS DIFFUSION

Transfer to Leningrad

71. It was difficult for source to reconstruct satisfactorily the events which led to the final Steenbeck project, a centrifuge cascade which would take natural uranium to an enrichment of 96 percent. This project was called by the Germans the "big factory." As has been shown, Steenbeck learned from Thiessen that the centrifuge topping plant had come too late for use in conjunction with the diffusion cascade. Although source agreed, that it is unlikely that Steenbeck would call off the topping plant project after a discussion with Thiessen, a fellow German, yet source could not recall any particular Soviet individual, commission, or other incident which officially cut off the topping plant project and introduced the "big factory."

72. It must have been in the period between the spring and summer of 1951, especially after the losses were reduced to 0.15 percent, that Steenbeck decided that, even if the centrifuge was too late for the topping plant, he might build a cascade of centrifuges which would provide direct competition with the gaseous diffusion plant. As source pointed out, the research work which was carried out in Sinop during this period was of the type that could be applied to the new "big factory" project.

73. On 15 September 1951, Steenbeck and Scheffel left Sinop for the Kirov Plant in Leningrad, taking along the more qualified Soviet assistants and all the centrifuge equipment. Steenbeck did not want Kirvalidze. [redacted]

[redacted]

[redacted] EQ-1802

[redacted]

[redacted]

Organization in Leningrad

75. [redacted] Kirov plant in Leningrad, no centrifuge work as yet had been started. The work was to be conducted in OKB-133 (or 33), which occupied a separate building in the Kirov plant. This OKB /Opytno-konstruktorskoye byuro = Experimental Design Bureau/ had earlier developed the compressors for the diffusion cascade, and work on this continued henceforth side-by-side with the centrifuge development. A site layout of the Kirov Plant and OKB-133 is shown on pages 53 and 58.

76. The organizational structure of the centrifuge group in OKB-133 was as follows:

Chief of OKB-133	- Sinëv (fnu), responsible for compressor and centrifuge development
Deputy	- Arkin (fnu)
Supplies	- Minin (fnu), an engineer
Centrifuge Development	- Steenbeck as German consultant to the Director

Theory: German Consultant: Steenbeck

Soviet Chief Elect: Morinson (fnu)

A staff of mathematicians was available for calculations for centrifuge and compressor development

[redacted]

[redacted]

Technical Activity in Leningrad

77. The new project began in March-April 1952 with the order for the construction of two batteries of six long-tube centrifuges each. Some of the design work for these batteries had been done by Steenbeck and Scheffel in Sinop. The centrifuges were of the same type that had been contemplated for the topping plant, as Steenbeck was convinced that he could use the same centrifuge in a cascade that was to take the enrichment from natural uranium to weapons-grade material. Steenbeck planned at this time simply to transport the material from one stage to the next higher stage by means of centrifugal pumps, and believed that it was just a question of enlarging the scope of the operation. [redacted] for the "big factory," however, the energy consumption required for the transport would become enormously important. Using centrifugal compressors as pumps, the energy requirement would not have been much smaller than the power requirements of the diffusion cascade. This, according to source, was a basic error made by Steenbeck.

78. The design and construction work on these batteries was, in source's opinion, excessively delayed and was not completed until the [redacted] The controversy, short rotors versus long rotors, had been raging for a long while. As early as 1948, the Steenbeck group had obtained favorable results with a short rotor, and yet had gone on to long tubes because Steenbeck had promised the Soviets tubes from five to ten meters long. In connection with the topping plant, Steenbeck, too, had committed himself to five-meter rotors. In Leningrad it was learned by the Germans that Kamenev, Steenbeck's Soviet competitor, was working with short rotors. [redacted] In May 1952, [redacted] the future was by way of short rotors. Slowly most of the Soviet scientists in OKB-133 and even the Ninth (or the First Chief) Directorate agreed that the short rotors were more promising; yet no one had the initiative to challenge the order to build the battery of six long-tube centrifuges.

79. [redacted] During this period the magnetic bearing was developed, the scoops which solved the problem of extraction at low expenditure of energy were devised, and the molecular pump

22. [redacted]

for evacuation of the space outside the rotor was invented. A separation apparatus which incorporated these discoveries and used a 25-cm long rotor was completed at the beginning of August 1952, [redacted]

Description of the Short-Tube Centrifuge

80. A drawing of the short-tube centrifuge appears on page 39. /Unlike the rest of the drawings in this report which are a draughtsman's renderings of very rough sketches made by source, this drawing is reproduced photographically from the original sketch made by source/.22

81. Like the long-tube model, the short rotor operated in a 10^{-5} vacuum, and it rotated on a needle bearing identical to that used in the long-tube model. The upper bearing was a magnetic bearing of novel design. The extraction of both the enriched and the depleted output streams was accomplished by means of scoops at the periphery of the rotor, and both streams were in this way provided with the motive power to reach the next stage in a cascade arrangement. The countercurrent flow in the rotor was also induced mechanically by the action of the upper scoops, and consequently no thermal gradient was required for this purpose.

82. The following points are depicted on the drawing on page 39.

Point 1 — Rotor

After extrusion and heat treatment, the rotors had to be turned on a mandrel in order to correct them to the required tolerance. The extruded rotors always required this treatment, while the machined rotors were finished off with the same equipment. The rotors were made by Soviets in the workshop, but the testing and final wall-thickness machining was done by the Germans. Dynamic tests were then conducted in air at about 100 cps, which resulted in about 10 percent discard of the manufactured rotors. The rotor wall was 0.3 mm thick.

Point 2 — Magnetic Bearings

The magnetic bearing used on the top of the "short tube" consisted of a small tubular permanent magnet made of Soviet magnetic alloy (similar to Alnico) and an unmagnetised cylinder of a stainless magnetic steel, which was mounted

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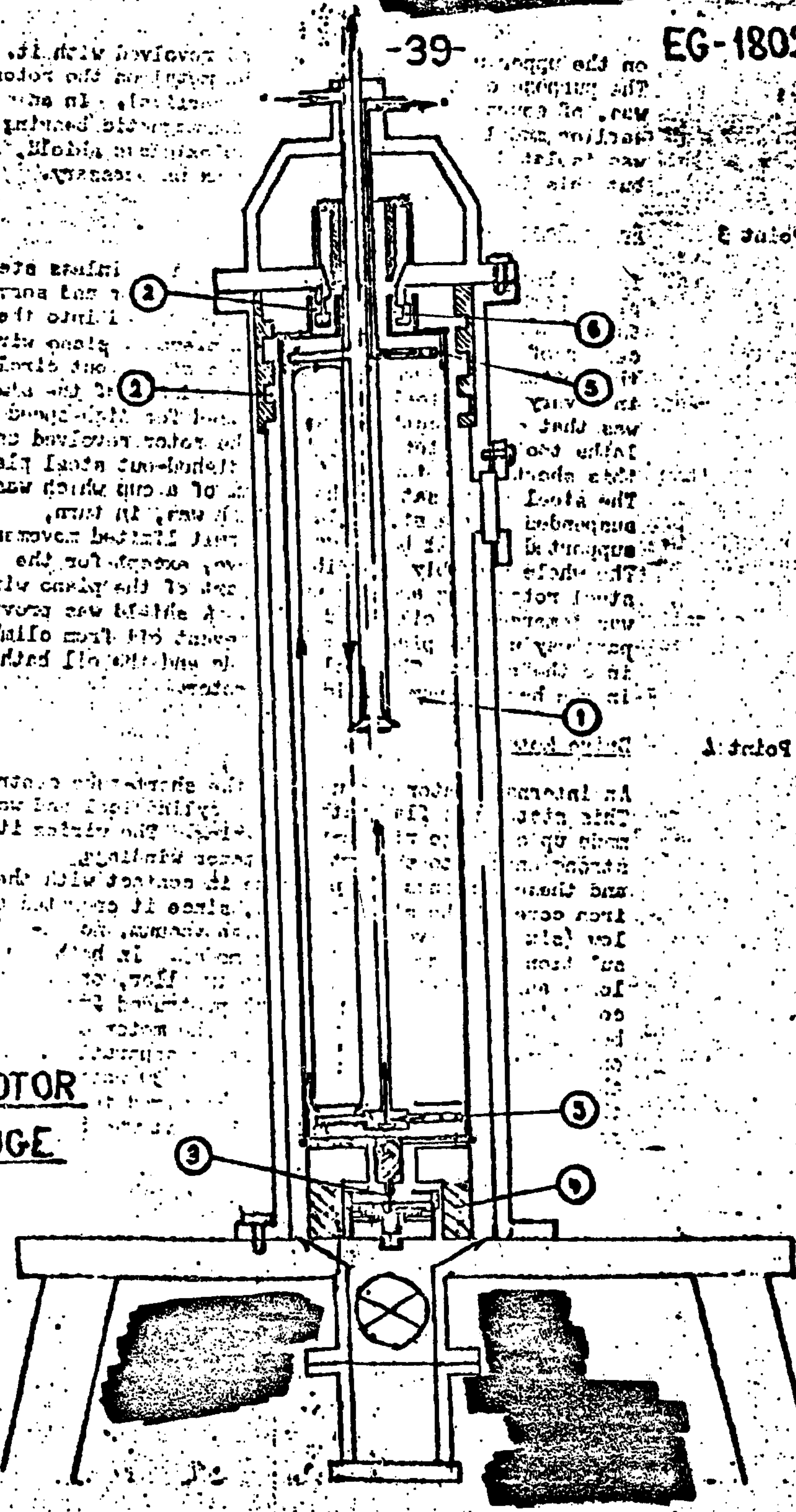
22. The sketch depicts a 45-cm tube centrifuge. It is in principle similar to the 25-cm tube described in the text.

[redacted]

[redacted]

[redacted]

EG-1802



SHORT-ROTOR CENTRIFUGE

[redacted]

on the upper neck of the rotor and revolved with it. The purpose of this magnet and the metal on the rotor was, of course, to hold the rotor vertical. In an earlier model, the upper part of the magnetic bearing was isolated from the lower by a plexiglass shield, but this idea was later abandoned as unnecessary.

Point 3 Point Bearing

In the bottom of the rotor there was a stainless steel plug which protruded below the rotor proper and served as the armature of the drive rotor. Fitted into the center of this plug was an 0.8-mm piece of piano wire, the bottom end of which rested in a dished-out circle in a very hard steel plate. The hardness of the steel was that of the cutting surface used for high-speed lathe tools (Soviet pobetit). The rotor revolved on this short wire, turning in the dished-out steel plate. The steel plate sat in the bottom of a cup which was suspended from a steel plate which was, in turn, supported on ball bearings to permit limited movement. The whole assembly described above, except for the steel rotor plug and the upper part of the piano wire, was immersed in oil for damping. A shield was provided part way up the piano wire to prevent oil from climbing into the rotor. The whole bearing and the oil bath were in the hard vacuum outside the rotor.

Point 4 Drive Motor

An internal stator was used in the short-tube centrifuge. This stator was flat rather than cylindrical and was made up of large ribbon-type wiring. The wiring itself was strong enough to support the stator windings, and these windings did not come in contact with the iron core of the stator. Thus, since it operated at low (six volts) voltage in a high vacuum, no insulation was necessary in this model. In both the long- and short-tube models the impeller, or armature, consisted of a steel plug which protruded from the bottom of the centrifuge rotor. The motor operated on the induction principle. During separation tests, the maximum power required was about 30 watts to drive the motor. About 40 watts were required to bring the rotor up to full speed before the test was begun.

[redacted]

Point 5 Extraction Scoops

Extraction from the short-tube rotor was done by means of fixed scoops mounted within the rotating gas. The scoop at the top took off the heavy fraction at a pressure of about five millimeters of mercury. The scoop at the bottom was also arranged to take off the product at the periphery, and thus had about the same pressure output. The countercurrent flow, however, developed by a fixed screen arrangement at the bottom of the rotor, which operated as a sort of molecular pump, brought the center or light stream out toward the periphery at the point where the light-stream pickup scoops operated. This made the elimination of interstage pumping possible on both streams. Source remarked that no effort was made to apply good aerodynamic design to these scoops, although the possibility of doing so and thus raising the separating power of the centrifuge was recognized. He pointed out that the Germans were abruptly taken off work on the centrifuge problem after the successful separation tests on the first model of the short-tube rotor were run, and that they consequently had no opportunity to refine the design. Source felt that the main reason for the relatively low separation factor obtained with the short-tube model lay in the poor aerodynamic properties of the scoops. He felt that with aerodynamic improvements the separation factor of the short-tube centrifuge could be raised up to that of the long-tube machine, that is, to 50 percent of theoretical.

Point 6 Molecular Pumps

The vacuum-maintaining molecular pumps consisted simply of fixed threaded insert mounted on the housing of the centrifuge in such a way that the revolving tube just cleared the top of the threads. The threads were like those of a nut and spiraled upward (looking at it from the direction of rotation of the rotor). Gas which became trapped in the threads was given a velocity in the direction of the spiral by molecular impact with the walls of the spinning rotor and was thus pumped away from the evacuated region. Exactly this same design was repeated in the second molecular pump which was mounted on a sleeve, at a smaller radius, within which the upper neck of the tube rotated. With

[redacted]

this arrangement a pressure of 10^{-5} mm of mercury was maintained in the space around the rotor, a pressure of 10^{-3} mm of mercury outside the neck of the rotor, and a pressure of 10^{-2} mm of mercury inside the rotor at its center.

Assembly and Test of Short-Tube Centrifuge

83. The needle for the lower (point) bearing of the short tube was matched to a hardened steel dish and the two were ground together to assure proper operation. The workshop supplied to the laboratory the rotor, the rotor covers, the bearings, the outside jacket, and the head piece of the centrifuge.

84. The first step of assembly was to insert the scoops and screen into the rotor. After this, the bottom cap, which included the impeller plug and the needle part of the bottom bearing, was attached to the bottom of the rotor tube. The cap was beveled so that it joined the tube smoothly, and the joint was made airtight by the application of BF-4 (Soviet designation) lacquer to the joint on the outside surface of the rotor.

85. The upper cap of the rotor was similarly applied and the rotor was slid down into the housing. The funnel-shaped opening in the bottom bearing made the insertion of the needle into the bearing easy. Then the top portion of the housing was attached, with its plumbing for feed and two output streams. This was bolted to the main housing.

86. Before assembly started the various required rotation tests of the rotor and caps had been made on special equipment designed for the purpose.

87. After the centrifuge was assembled, which required about a half hour for an experienced crew, the diffusion pump in the bottom of the housing (100 liter capacity) was turned on, and the centrifuge was entirely evacuated, taking care not to boil the oil in the bottom bearing in the process. When evacuation to 10^{-5} millimeters was completed, the rotor was turned on and allowed to come up to speed. This required about fifteen minutes. When the rotor had reached operating speed, the diffusion pump was turned off and isolated from the system, since the molecular pumps would then maintain the vacuum.

88. By means of a needle valve, UF_6 at natural isotopic concentrations was allowed to enter the rotor through the feed tube which led to the geometric center of the rotor. The entering material was measured by a flow meter. Only tenths of a second were required for the centrifuge to reach equilibrium; so the separation test was begun immediately.

[redacted]

89. The separation tests took varying amounts of time, depending on the quantity of output desired for analysis and on the conditions (e.g., mass transport rate) app[illegible] to the particular test. The shortest test remembered by [illegible] took about 20 minutes, while the longest required about two hours. He thought that the samples usually obtained in separation tests amounted to approximately two grams. Source estimated that during separation tests the bottom bearing consumed only about 0.5 watt of power, while the total energy requirement for the entire unit was about 20 watts, exclusive of motor losses. He estimated that the motor efficiency was about 30 to 50 percent. Thus something like 80 watts of electrical energy was required for the centrifuge described below, independent of the centrifuge dimensions if the separative power is considered constant.

90. The output streams were condensed into ampules, using liquid air, and were turned over to the Soviets for testing. The highest separating power reached with this model of the short-tube centrifuge was 32 percent of theoretical, versus the Soviet requirement for at least 15 percent of theoretical.

91. Enrichment and depletion of the output of the centrifuge separation tests was measured with both a mass spectrograph and an alpha counter. Since these measurements always checked with each other fairly well, source presumes that they were accurate.

Costs

92. Source was told by one of the Soviets that the cost of the laboratory version of the short-tube centrifuge, excluding the diffusion pump and supporting table, was 2,000 rubles. For comparison, source remarked that an audio oscillator cost 75,000 rubles.

Operation of the Short-Tube Centrifuge

93. [redacted] following results [redacted] with a centrifuge 25 cm long, 58 mm in diameter, and operating at a peripheral velocity of 340 meters per second.

a. Maximum separation factor 1.2

b. Optimum separation factor 1.14

[redacted]

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94. The Steenbeck group used a concept, used also in cascade theory, which he termed separating power (Trennleistung) and defined as

$$L = K\ (\alpha - 1)^2 \cdot \frac{dM}{dt}$$

where K is a constant of unrecalled dimensions, α is the separation factor and $\frac{dM}{dt}$ is the transport of the enriched stream out of the rotor. Source recalled that this quantity is proportional to the fourth power of the peripheral velocity. He also recalled that theory indicates that for very large peripheral speeds (higher than 340 meters per second) this dependence changes to a second power.

95. A graph depicting the dependence of the separating power on the flow of the light stream is reproduced on page 45. Source stated that the optimum conditions indicated corresponded to 32 percent of the separating power predicted by theory. Source said that this curve was found to be in accord with a requirement of 20,000 meters of rotor for a plant which will produce one kilogram of uranium 235 per day in material enriched to 96 percent. This corresponds to a power expenditure of less than 3,000 kilowatts. Source knew that it was planned to use natural uranium as feed, but did not recall what waste concentration was considered.

96. [redacted] in fall 1952 the long-rotor centrifuge project died.

97. [redacted] The Germans were gradually being eased out of the project.

98. During the period January to March 1953, OKB-133 was planning the construction of three centrifuges of the 45-cm rotor type. These were to be placed in the same room in which the first 45-cm centrifuge was in place. At the same time, OKB-133 officials were discussing (and sometimes asked the Germans about) the construction of 40 centrifuges of the same type, which were to be set up in the

[redacted]

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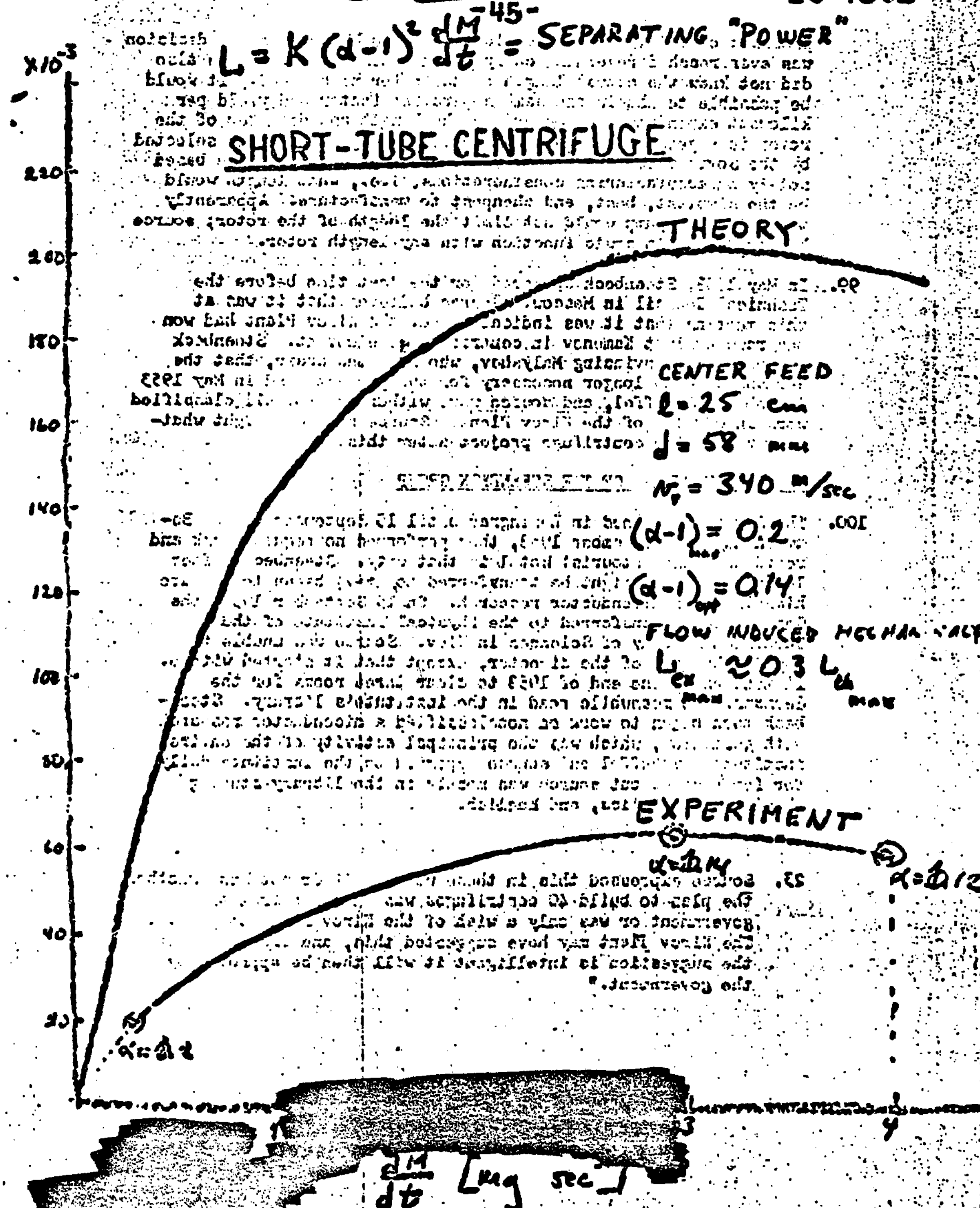

premises of OKB-133. Source did not know whether or not a decision was ever reached regarding carrying out this project.[23] He also did not know the actual length of the rotor to be used. It would be possible to obtain the same separation factor and yield per kilowatt expenditure by enlarging the length and diameter of the rotor in a geometrical ratio. The final length of tubing selected by the Soviets for the individual centrifuge unit would be based
107. solely on manufacturing considerations, i.e., what length would be the simplest, best, and cheapest to manufacture. Apparently the molecular pump would not limit the length of the rotor; source believed that it could function with any length rotor.

99. In May 1953, Steenbeck appeared for the last time before the Technical Council in Moscow. Source believes that it was at this meeting that it was indicated that the Kirov Plant had won the race against Kamenev in centrifuge development. Steenbeck succeeded in convincing Malyshev, who held the chair, that the
108. Germans were no longer necessary for the project. [redacted] Source had no insight whatsoever into the centrifuge project after this date.

QUARANTINE PERIOD OF THE STEENBECK GROUP

100. The Germans remained in Leningrad until 15 September 1953. Between May and September 1953, they performed no regular work and resided at the Intourist Hotel in that city. Steenbeck, after learning that he might be transferred to Kiev, began to prepare himself for semiconductor research. On 15 September 1953, the Germans were transferred to the Physical Institute of the Ukrainian Academy of Sciences in Kiev. [redacted] It took until the end of 1953 to clear three rooms for the Germans, who meanwhile read in the institute's library. Steen-
110. beck then began to work on nonclassified semiconductor research with germanium, which was the principal activity of the entire institute. [redacted]

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23. Source expressed this in these words: "I do not know whether the plan to build 40 centrifuges was an order from the government or was only a wish of the Kirov Plant people . . . The Kirov Plant may have suggested this, and ordinarily if the suggestion is intelligent it will then be approved by the government."

[redacted]

101. [redacted] 26 July 1956, when the Germans left the Soviet Union.

102. [redacted] A day before leaving the Soviet Union, the Germans were given bonuses or premiums which, in source's opinion, were of the order of magnitude of Stalin Prizes then out of fashion; [redacted] Source was of the opinion that this was related to the indications given to Steenbeck by the Technical Council in March 1953 that his centrifuge had been preferred to Kamenev's.

CHRONOLOGY OF STEENBECK'S THEORETICAL WORK

103. Source [redacted] supplied the following chronological listing of Steenbeck's work.

104. Initially Steenbeck calculated the separation for a plane model which he called Baendchen Model (strip model). He assumed a gas flow between two planes from top to bottom, in which the gravitational force is effective from the inside to the outside. The gravitational force separates the light and the heavy isotopes by transporting the heavy to the outside while the light remains inside. Against this centrifugal force operates the diffusion. It was the equilibrium between these flows that Steenbeck first investigated. This study served as basis for Steenbeck's proposal to use the centrifuge as a means of uranium isotope separation, and it was this study that led to the construction of the double-walled rotor /see page 19/.

105. Thereafter for about one and a half years Steenbeck was occupied with translating his calculations to a cylindrical rotor which did not contain another concentric tube on the inside. This is more generic, as it is assumed that the interior rotor has a diameter of zero.

106. Another study of Steenbeck which was important for self-cascading separation was an analysis of the internal flow. That is, there is a separation of light and heavy isotopes because of the gravitational and centrifugal fields; secondly, a cascading of this separation is caused by the countercurrent flow resulting from the temperature gradient. For years Steenbeck attempted

[redacted]

to discover the true nature of this flow and to discover, in particular, the location of the so-called dead zone. He solved this sometime in 1950. Subsequently Steenbeck made a study of losses involved in the centrifuge. His last theoretical investigations dealt with the countercurrent flow at the ends of the rotor (end-effect studies).

107. These theoretical studies of Steenbeck were not so clearly differentiated as they are listed; many overlapped, or were made at the same time. Steenbeck throughout these years also made many studies of the mechanical parts of the centrifuge especially in connection with long rotors. A comprehensive report on mechanical problems and separation theory was published by Steenbeck during the summer of 1951 shortly before he left for Leningrad. Before that date and afterwards he published small topical reports.

108. Steenbeck always rejected working on cascade theory. He was quite familiar with Barwich's theory of the diffusion cascade, and he felt that if necessary he could, with some relatively minor alterations, apply it to the centrifuge cascade.

ACTIVITIES OF KAMENEV

109. Source had very little knowledge of the work of Kamenev, the Soviet physicist ~~Kandidat nauk~~ who was a competitor of Steenbeck. [redacted] Kamenev occasionally visited Steenbeck and seemed to be fully informed of the group's work performed in Sinop and Leningrad. Kamenev had a laboratory in Moscow, which was most probably of the same magnitude as that of Steenbeck. Steenbeck once visited Kamenev's laboratory.

110. Kamenev began work on centrifuges at a relatively late date, and by 1953 he was still separating with model gases and not with UF_6. Source knew only that he worked with short rotors and that his centrifuge was of the type designed in Sinop during the early days when they, too, designed a short rotor.

111. Source considered Kamenev, aged approximately 45 years, extremely capable and intelligent. He stated that there is a good possibility that Kamenev took over the centrifuge project in the Kirov Plant after the departure of Steenbeck, but this was only a conjecture. Source argued that while the Kirov Plant management might prefer to place one of their own men in this job, they would realize that, if they entered into competition with Kamenev on the centrifuge development which was still unfinished at the time of Steenbeck's departure, they would certainly loose out to Kamenev.

[redacted]

CONJECTURES ON SOVIETS' FUTURE PLANS IN CONNECTION WITH THE CENTRIFUGE

112. That the Soviets were aroused by Steenbeck's centrifuge development was dramatically demonstrated by the 1949 negotiations between Beriya and Steenbeck. The Soviets continued to be interested in the possibilities of the centrifuge even after they found a solution to their gaseous diffusion cascade without recourse to the centrifuge. This was proved by the transfer of the Steenbeck group from Sinop to the Kirov Plant in Leningrad in September 1951 and January 1952. This transfer occurred at a time when the Sinop and Agudzeri institutes were reverting to nonclassified work. As late as March 1953, Steenbeck was still dealing with the Technical Council, which source considered one of the top-level scientific policy making groups in the USSR.

113. The short-rotor centrifuge which the Germans developed in OKB-133 was able to produce one kilogram of U_{235} metal per day, enriched to 96 percent, using 20 kilometers of rotor tubing, at less than one-tenth the power expenditure of the gaseous diffusion plant. Source remembers that the Steenbeck group had been operating with a target figure of 50,000 kilowatts for the diffusion cascade: He stated: "I don't know exactly, but it was a very high figure. It was certainly in the neighborhood of 50,000 kilowatts for one kilogram per day." Answering the question whether this was a realistic figure, source stated: "It is not completely taken out of the air, but it is not exact either. It is a figure with which they somehow operated. You see, the energy requirement is the thing which makes the centrifuge so very attractive, and for this reason we had to know at what point we actually would become competitive.....Sure, the figure is not accurate.....it may be off by a factor of two.... whether it is correct I don't know, but that is the figure which was given to us." Source could give no details as to who specifically issued this target figure, but it was used during their work in the Kirov Plant from March 1952 to March 1953. According to source, the centrifuge which earlier had been only of academic interest now was competitive with the gaseous diffusion process.

114. An attempt was made to discover any fragments which source might have gleaned that would indicate the future course the Soviets would take in connection with the centrifuge. [redacted] it was not clear [redacted] whether the Soviets would build the centrifuge on a vast scale or would put it on ice....This question could not be answered from any of the statements or remarks made by the Soviets."

[redacted]

115. Source was then asked to analyze the problem, taking into consideration political and economic as well as scientific aspects. Source stated firmly that there exists no "absolute necessity [for the Soviets] to build the centrifuge cascade as a competitor to the gaseous diffusion plant. They have a process by means of which they can separate as much as they want." That the gaseous diffusion process was capable of producing weapons-grade material is evident, since they shelved the use of the centrifuge as a topping plant: "They no longer were interested in the centrifuge at a certain date in 1951. Therefore, the gaseous diffusion plant was operating at weapons grade as of that time."

116. Source avoided giving a definite answer to the question whether the Soviets would build a centrifuge cascade. Stressing the many intangibles involved, he decided that it would be impossible to make an evaluation. It was the impression of the interrogators, however, that source held it unlikely that the Soviets would build such a cascade. Nevertheless, he believed that, at the very least, they considered it interesting enough to continue it as a development project. Because the energy consumption is relatively low and because small amounts of material can be enriched to a high degree, they would not completely drop the project.

117. Source's arguments against the Soviets building a centrifuge cascade may be summarized as follows:

a. While power requirements may be a very important consideration in the economy of the US, power requirements were not considered to be a critical aspect in the Soviet Union. The Soviet government would deprive the civilian sector of the economy of electric power as long as it would be required for the atomic project. The Soviets would consider the power consumption factor only if a point were reached at which the rest of the armament program (i.e., nonnuclear armaments) would be endangered by excessive power demands of the atomic program.

b. Should the Soviets desire to increase their stockpile of fissionable material, they would do so, not by enlarging their existing gaseous diffusion facilities nor by the centrifuge method, but by the plutonium process which source considered to be simpler. No attempt was made during the interrogation to investigate source's reasons for the statement, "You don't need that much U_{235}, because you make plutonium." [It is the impression of the interrogator that this statement was based not so much on insight into the Soviet plutonium program as on popular literature.] Source believed that the first USSR atom bombs were made of U_{235} and that the later ones were made of plutonium, and that the critical mass was six kilograms.

[redacted]

c. A very important reason why the Soviets would not be inclined to build the centrifuge cascade was the fact that the gaseous diffusion cascade was successful. It was a tried and true process, whereas the centrifuge method still required considerable development and might contain unforeseen difficulties: "There might still be people who will doubt that it would actually be possible to operate, really safely, a big plant of centrifuges that spin so rapidly. This is something that has never been in existence before."

d. Related to the immediately preceding argument was source's contention that for the construction of the centrifuge cascade the Soviets would require technical manpower which is a scarce commodity in the USSR. While it is true that scientists of the first order no longer need be assigned to the centrifuge development, the project would require engineers of the first order. The Soviets do not have an unlimited supply of first-class engineers, and all those available are already committed to the Soviet armament program.

e. Finally source stated that much depended on the position taken by the Kirov Plant in convincing the government as to the importance of the centrifuge. The Kirov Plant, in turn, was wrestling with intangible and subjective factors. In source's opinion, there was no one in OKB-133 as qualified to continue the centrifuge work as Kamenev was, but he did not belong to the insiders at the Kirov Plant and was regarded by the management of the plant as an outsider. A plant like the Kirov Plant could exhibit astounding energy when it came to closing its doors to outsiders like Kamenev, and such a factor might well influence the future of the centrifuge in the USSR.

118. Finally source was asked to estimate the time which would be required by the Soviets to construct a pilot plant of 40 centrifuges, starting with the material made available to them by the Steenbeck group in March 1953. After much hedging, he finally stated that this would depend on the pressure applied by the Soviet government. If a great deal of pressure were applied by the government, the Kirov Plant could build the pilot plant within six months to one year, using from 50 to 100 men. One to two more years would be required to build a cascade comprising 20 kilometers of tubing. The pilot plant would take so long, because it would have to include some unfinished development work. Without government pressure, on the other hand, the Kirov Plant "will fool around for 10 years, and it [the centrifuge] still won't amount to anything, or won't be any further than it was at the time we left. Whether the order came from the top 'Continue as you have' or 'Full-steam ahead,' that I don't know."

[redacted]

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SITE LAYOUT OF KIROV PLANT IN LENINGRAD

119. Source identified on USAF Target Complex Mosaic 0153-9997-4-25MA the annotation 4-212 as representing the Kirov Plant in Leningrad. The coordinates for the plant are: N 59-53, E 30-15. On page 53 appears a memory sketch of one section of the Kirov Plant with which source was familiar; except for purposes of orientation, the referenced Target Mosaic was not used in making this sketch. The memory sketch was drawn, using as a starting point the path which source followed daily from the entrance of the plant to his place of work, OKB-133. This path is marked A on the sketch.

120. Source had very little definite information on the Kirov Plant. He did not know the zavod number or the postbox number; the imeni designation was Kirova.[24] He stated that the plant was subordinated to the Ministry of Heavy Machine Building, and that its director possibly was named Voroshilov (fnu). He estimated very roughly that about 45,000 persons were employed in the Kirov Plant. He had no information on the production of the plant, except to state that the turtle-shaped tank was produced there.[25] This turtle-shaped Soviet tank was designed at the Kirov Plant by the chief designers Sinёv and Arkin, who each received a Stalin Prize in connection with this work. Source had no details regarding its production except that in the vicinity of OKB-133 the finished tanks were "adjusted" /see Point 14 on page 53/. Source knows also that the compressors for the Soviet gaseous diffusion cascade in Kefirstadt /Verkh-Neyvinskiy/ were designed in the building which was known as OKB-133 during the period from 1952 to 1953 when source was at the Kirov Plant. He did not know, however, whether the compressors were also produced in the Kirov Plant. Development work on the compressors continued in OKB-133 during the period of source's stay at the Kirov Plant. He believed that household products, primarily kitchen utensils, were produced in the Kirov Plant; that railroad signal equipment might be produced; and that during World War II torpedoes were produced.

121. The Kirov Plant was surrounded by a two- to three-meter high fence, part of which was made of solid wood and was painted green and parts, especially along Prospekt Stachek,[26] were of masonry.

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24. Although source remembered the Kirov Plant only by the name Kirov Werke, which corresponds to the designation Kirovskiy zavod found on the Soviet Plan Leningrada published in 1936 by the Leningrad Oblispolkom and the Leningrad Soviet, there is no doubt that the plant is that listed as Leningrad Heavy Equipment Plant, "Krasnyy Putilov/Kirov" 185 on USAF Target Complex Mosaic - Series 25, No. 0153-9997-4-25MA, under 4-212. USAF Target Mosaic - Series 10, No. 0153-0029-10M, showing the plant in larger scale, was unfortunately not available to the interrogators at the time of the interrogation.

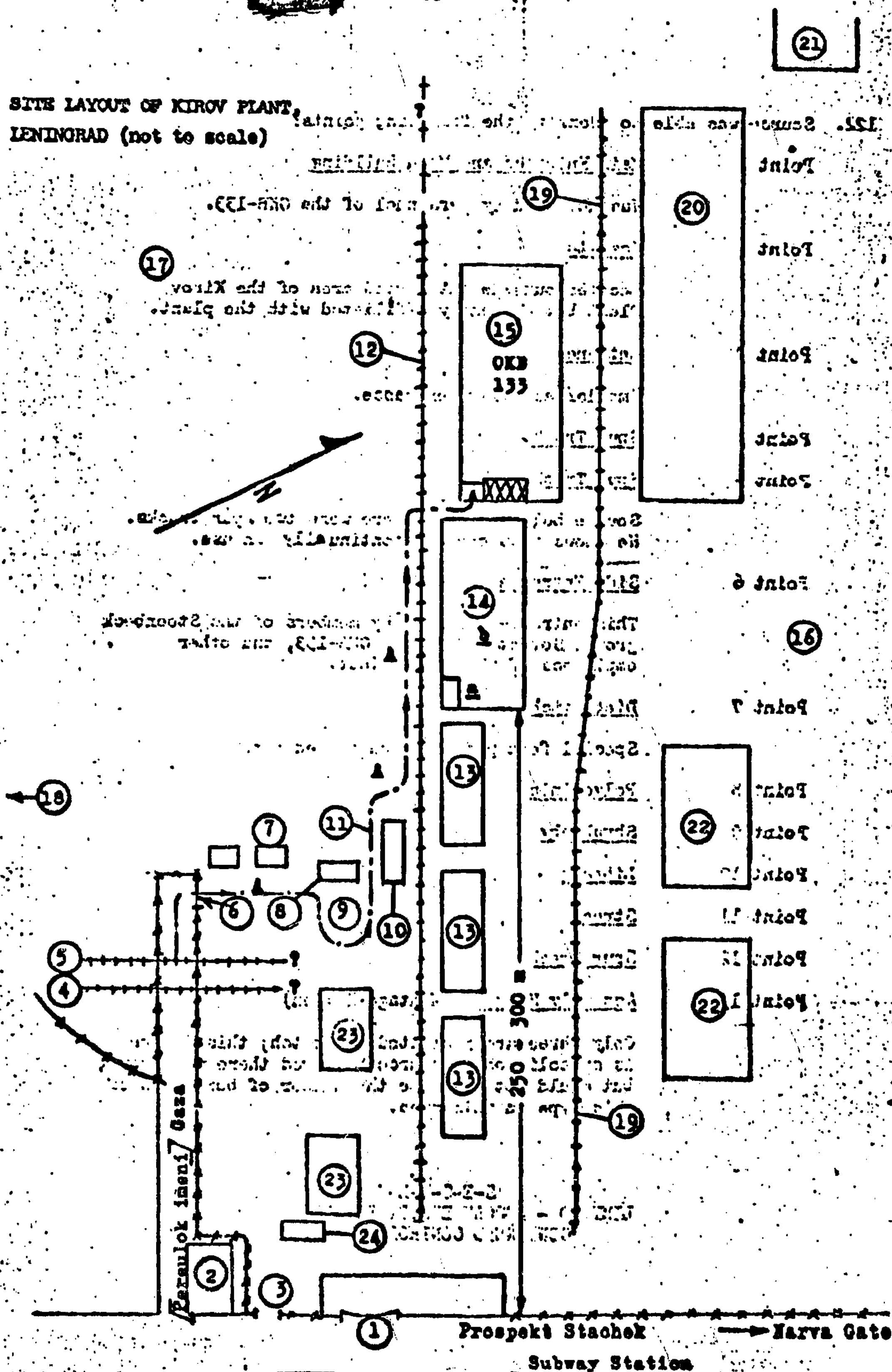
25. Details on this Soviet tank are given in STIB Report No. OLB/BL/56/223, published 22 November 1956. [redacted]

26. Source always gave the name of this street as Prospekt Stachek. On the Plan Leningrada, dated 1936, the street is called Stachek ulitsa.

[redacted]

[redacted]

SITE LAYOUT OF KIROV PLANT, LENINGRAD (not to scale)



[redacted]

122. Source was able to identify the following points:

Point 1 — Main Entrance and Main Building

Was not used by personnel of the OKB-133.

Point 2 — Hospital

Located outside the fenced area of the Kirov Plant but evidently affiliated with the plant.

Point 3 — Entrance

Smaller auxiliary entrance.

Point 4 — Spur Track

Point 5 — Spur Track

Source believed that there were two spur tracks. He knows that one was continually in use.

Point 6 — Side Entrance

This entrance was used by members of the Steenbeck group, Soviet workers of OKB-133, and other employees of the Kirov Plant.

Point 7 — Diet Kitchen

Special food prepared and served here.

Point 8 — Polyclinic

Point 9 — Shrubbery

Point 10 — Library

Point 11 — Street

Point 12 — Spur Track

Point 13 — Assembly Hangars (Montage-Hallen)

Only three are indicated on sketch; this figure is symbolic only; source believed there were more, but could not estimate the number of buildings of this type in this area.

[redacted]

[redacted]

Point 14 — Open Field

a. At this corner of the open field the turtle-shaped tanks produced at the Kirov Plant were tested throughout the period of source's stay in Leningrad. Source described this as follows: The tanks ran up a 15° to 20° inclined ramp. The turret was then traversed about 180° (never more than 180°) and the gun was lowered and raised through an arc of about 30° to 45°. Source did not describe the tank except to say that it was about 2.5 m high, drove rapidly, smoothly, and quietly. He thought that the treads might have been of rubber.

b. Monument with T-34 tank.

Point 15 — OKB-133

For details see pages 56 to 58.

a. Entrance and propusk office for OKB-133.

b. The hatched part was recently added.

Point 16 — Unidentified Area

Point 17 — Unidentified Area

Point 18 — Unidentified Area

Source believed that the Kirov Plant extended in this direction but could give no details on construction.

Point 19 — Spur Track

Point 20 — Large Building

This building was larger and newer than others in the Kirov Plant. No details on what was produced in it.

Point 21 — Factory Building

In this building, and possibly others in this area and not shown on the sketch, household articles and kitchen utensils were produced.

Point 22 — Factory Building

Production use of buildings unknown. The two buildings shown are symbolic only; there probably were more.

[redacted]

[redacted] EG-1802

Point 23 Factory Building

Point 24 Apartment House

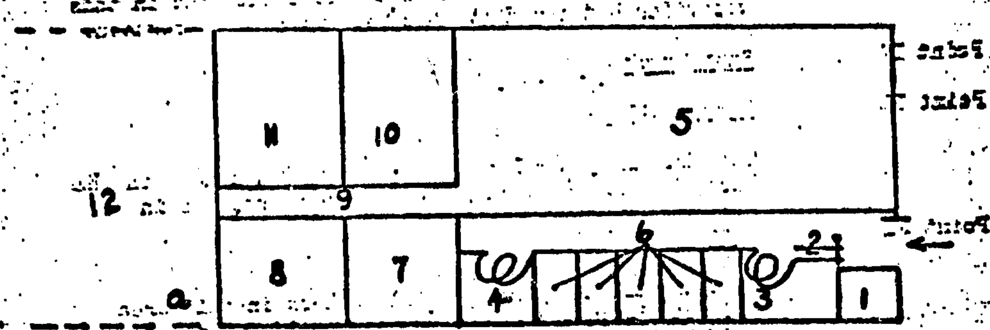
A small, old building. Source did not know whether it was still used as a dwelling.

The typical plant buildings listed as Points 13, 22, and 23 were old, steel-framed and stucco-finished buildings, 80 m x 30-50 m x 10 m.

Floor Plans for OKB-133

123. The Steenbeck group worked from September 1951 to May 1953 in OKB-133. They were confined to one section of the three-story brick building, measuring 150 m x 80 m, which is shown on page 53 as Point 15. The third floor of the building was off limits to the Steenbeck group. Source knows that in the remainder of the OKB-133 building the compressors for the Soviet gaseous diffusion plant had been developed. Additional development work or tests were continued on the compressor throughout the period of source's stay at the Kirov Plant. Though he never saw a compressor, he identified the hum of the compressor as being in the range from 100 to 150 cps.

124. Source prepared the rough floor plans given below and on page 58.



Ground-Floor Plan
of KB-133

[Legend on page 57]

[redacted]

[redacted]

Legend to Floor Plan of OKB-135 (Ground Floor)

Description of Points

Ground Floor

Point 1	Byuro propuskov
Point 2	Narrow hallway with guard
Point 3	Staircase
Point 4	Staircase
Point 5	Mechanical workshop
Point 6	Offices (out-of-bounds to Germans)
Point 7	Small mechanical workshop with two or three lathes, the largest having a maximum working length of 250 cm, and one milling machine.
Point 8	Testing area for centrifuges. Tensile strength tests were made with specially designed machines.
Point 9	Hallway
Point 10	Large room for centrifuges. Separation tests were carried out here. Here stood the 45-cm centrifuge which was built shortly before the Steenbeck group left the Kirov Plant, and here the two 45-cm centrifuges were to be set up, as soon as they were built.
Point 11	Compressor test room. This area was specially guarded and source heard the characteristic sound of compressors coming from here.
Point 12	Area out-of-bounds for members of the Steenbeck group. Source believed that at (a) was located the mass spectrometer and alpha counter for enrichment measurements.

[redacted]