Alexander Pechenkin

L.I. Mandelstam and His School in Physics

Second Edition

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Preface to the Second Edition

The first edition of this book appeared five years ago. What remarkable works have appeared in the literature about the history of Soviet (and Russian) science over this period? It is worth pointing to the book which is directly connected with the subject matter of the present book: “Sergei Mikhailovich Rytov. Life story, recollections, interviews, notes, verses, documents” (composed by E. Berezanskaysa and N. Rytova, Moscow, 2015). S. Rytov is one of Mandelstam’s disciples of the second generation. It is also worth mentioning V.V. Kudriavtsev’s Dr.Sci. thesis “Scientific Schools in Russian Radiophysics” (2018).

The main conceptual context which is present in the above books is connected with the concept of a scientific school which is emphasized in the present book, too. It should be pointed out that the second edition is also engaged in a number philosophical problems: what is probability in theoretical physics, what kind interpretation of quantum mechanics was popular in the USSR, how the philosophical discussions entered into the scientist’s career.

The history of Russian science attracted the English-speaking historians of science as before: Loren Graham, *Lonely Ideas: Can Russia Compete?* (MIT Press, 2013), and Maria Rogacheva, *The Private World of Soviet Scientists from Stalin to Gorbachev* (Cambridge University Press, 2017).

However, the most remarkable phenomenon in the field, to which our book belongs, is the publication of S.I. Vavilov’ s diaries (two volumes, 2014, 2016). S.I. Vavilov (1891–1951), the brother of Nikolai Ivanovich Vavilov, who was arrested in 1940 and died in prison, one of the great figures in Soviet Union science. He was Director of Institute of Physics (FIAN) for which Mandelstam worked (1934–1951); he was President of the USSR Academy of Science (1945–1951) (see: A. Kojevnikov. President of Stalin’s Academy. The Mask and Responsibility of Sergei Vavilov—www.history.ubc.ca/sites/default/biblio/uj).

S.I. Vavilov’s experience as it is presented in his diaries cannot be overestimated. He wrote the notes for himself; he constructed an emotional model of the area within which he lived and the portraits of people with whom he spoke.

In what follows the text of the first edition is mainly reproduced.

The essential supplements are the following: a chapter about Michail Leontovich who was Mandelstam's student and became one of the leaders of the Soviet thermonuclear project, additional comments concerning Boris Hessen who was the Communist Party activist and Mandelstam's coworker, and a section on the philosophical and political discussion of Mandelstam's lectures on physics, the discussion which occurred posthumously in the beginning of the 1950s (Chap. 14).

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Preface to the First Edition

This book is concerned with the personal trajectory of the Soviet prominent physicist Leonid Isaakovich Mandelstam (1879–1944). Very often Russian books dedicated to biographies of Soviet scientists make aim to restore a historical justice, to return a person who has been forgotten or concealed for political reasons to the history of science. L.I. Mandelstam has not been forgotten. His five-volume complete works have been published in 1947–1955. These "Complete Works" contain a biography of Mandelstam written by his closest friends and collaborators. Additionally, a number of biographical and historical essays have been published concerning Mandelstam and his creative work. This book, therefore, aims to systematize what has been written about Mandelstam, to emphasize the controversial points, and to take into consideration the unpublished documents which would shed light on Mandelstam as a scientist and a personality.

This book is not only about Mandelstam as a teacher, a researcher, and a personality. This book is about a historical phenomenon named the Mandelstam scientific school, too. This school was one of the social and cognitive structures which determined the development of Soviet physics. In this connection, we should touch upon the phenomenon of scientific school in general; we need to explain how scientific schools arise, influence science, and come to crisis.

This book is also about the development of the great Soviet science. Mandelstam was not directly connected with nuclear physics and space research, which both are popular symbols of the great Soviet science. True, his former student Mikhail Alexandrovich Leontovich contributed to plasma physics (since 1951 he was Head of theoretical research in controlled thermonuclear fusion at the Institute of Atomic Energy—now the I.V. Kurchatov Institute of Atomic Energy). The student of Mandelstam's students Alexander Mikhailovich Prokhorov contributed a lot to the discovery of masers. In 1964, he received the Nobel Prize (together with N.G. Basov and H. Townes) for his contribution to quantum electronics.

The list of the achievements of representatives of the Mandelstam school could be continued. However, the most important contribution of the Mandelstam school to Soviet science was the development of scientific discourse and hence the development of scientific culture. Mandelstam, his friends, collaborators, and students were very careful in formulating the conceptual framework of physics. They discussed very subtle details concerning scientific conceptions. They liked discussions and cultivated the discussions of the foundations of physics not only directly (by organizing special seminars and lectures), but also indirectly within the framework of teaching and research in special issues of physics. Contrary to the widespread belief, Soviet philosophical discussions of the foundations of science were not separated off the Western discussions in this field. Mandelstam and his friends and students read the world literature and explicitly and implicitly pursued a policy of freedom with respect to such discussions.

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The author is grateful to the Fulbright program (USA), the British Academy (UK), German Museum of the history of science and technology (Deutsches Museum), and the Chemical Heritage Foundation for grants supporting his work at the foreign archives and libraries.

The author is grateful to C. Bissell (Open University, UK), V.P. Vizgin, A.V. Andreev, K.A. Tomilin (they all work (worked) for the S.I. Vavilov Institute for the History of Science and Technology, Russia), V.M. Berezanskaya (P.N. Lebedev Institute of Physics, Russian Academy of Sciences, Moscow), K. Hall (Central European University), G. Holton (Harvard University, USA), S. Schweber (Brandeis University), R. Siegmund-Schulte (Agder University College, Norway), and H. Kant (Max Planck Institute for the History of Science, Berlin) for illuminative discussions and useful comments. The author remembers his fruitful discussions with Rein Vihalemm (Tartu University, Estonia 1938–2015) and E.S. Boyko (Institute for the History of Science and Technology of Russian Academy of Sciences, 1939–2005). E.S. Boyko passed some material concerning A.A. Andronov to the author. Lidia Scharapenko (a translator) kindly helped him to translate some of difficult German texts.

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Chapter 1
Introduction

1.1 Why L.I. Mandelstam's Biography Is Interesting

Leonid Isaakovich Mandelstam was an outstanding physicist, not only in his home country, but in the context of world science. In the book "Russian Physics of the Nobel Level", L.I. Mandelstam is mentioned among nine scientists who could have received the Nobel Prize, but have not [242]. Some of them did not receive the Nobel Prize because it cannot be given posthumously. Others (including L.I. Mandelstam and G.S. Landsberg, who decisively contributed to the discovery of combinational scattering of light) have not received the Nobel Prize under the force of circumstance. The Indian physicist Sir Chandrasekhara Venkata Raman received the Nobel Prize for the discovery of the combinational scattering of light. This effect is named in honor of him.

Not only this makes the biography of Mandelstam interesting. A scientific community arose around L.I. Mandelstam. This community is commonly called the Mandelstam School. This was one of the foundational schools in Soviet prewar physics. In total, there were four such schools: the A.F. Ioffe, D.S. Rozhdestvensky, L.I. Mandelstam, and S.I. Vavilov schools [80, 81, 359–362].

A.F. Ioffe's school was based in Saint Petersburg (then Leningrad) Institute of Physics and Technology. The Nobel Prize winner Zhores Ivanovich Alferov was Director of this Institute from 1987 till 2003. The Faculty of physics and mechanics (Leningrad Politechnical Institute) trained potential members of the Ioffe school. This school yielded many outstanding physicists, in particular, Igor V. Kurchatov.

Dmitrii Rozhdestvensky organized a scientific community which received the name "Rozhdestvensky's school in optics". This school arose at the Physics Institute which belonged to Petrograd University. The State Optical Institute arose on the base of this school.

S.I. Vavilov's school is not so sharply defined. This is a school of optics physicists. It arose at the physics department of Institute of Physics and Mathematics (Academy of Sciences), then at the Institute which had been formed on the base of this department, that is, at the Physics Institute of the Academy of Sciences (FIAN).

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Mandelstam's school was formed back in his Odessa period (1918–1922). There he was joined by a devoted disciple, with whom for life. This was Igor Evgenievich Tamm (he became a Nobel Prize winner in 1958). However, the main events took place in Moscow. As is stated in the biography of Mandelstam that opens the first volume of Mandelstam's "Complete Works", "in 1925 the most fruitful and intense period of Leonid Isaakovich's scientific activity started. In a very short time, the scientific credibility of Mandelstam and his extraordinary personal charm unified the talented young scientists (G.S. Landsberg, I.E. Tamm, et al.) at the Research Institute of Physics, Moscow State University, and his lectures and highly informative seminars, outstanding in their content and form, which Mandelstam always very well prepared, attracted many talented young people—students and graduate students—to him. Under Mandelstam's guidance and being inspired by his ideas many young scientists started their research in a number of disciplines: radiophysics, optics, the theory of oscillations and molecular physics" [1, Vol. 1, pp. 27–28].

L.I. Mandelstam is also interesting within the framework of the general trends in the development of Russian physics. By tracing his personal trajectory, one can discern the specifics of the USSR scientific culture (the 1930s–1940s). Mandelstam was one of those physicists who brought the German scientific culture to Russia.

L.I. Mandelstam graduated from Strasbourg University and started as a researcher at the Strasbourg Institute of Physics. While living in Saint Petersburg and then in Moscow, he was still in contact with some of his Strasbourg colleagues.

L.I. Mandelstam was in the center of major scientific events and communicated with a wide range of Russian intellectuals (Mandelstam's students Igor Tamm and Alexander Andronov, and two Academicians of pre-Revolutionary Academy—Alexey Nikolaevich Krylov and Vladimir Ivanovich Vernadskii—should be mentioned first of all here). Mandelstam combined teaching and research work. The forth and fifth volumes of his "Complete Works" induce an association with German scientists' collected works where the conspectus of lectures figured prominently (these two volumes are constructed on the base of his students' records of his lectures and seminars and Mandelstam's own preparatory drafts). Mandelstam's biography enables one to discuss the interaction of education and research in the USSR, also from the point of view of world science.

Mandelstam's personal trajectory was dramatic. On arriving in his home country in 1914, Mandelstam had to start his scientific career again. Russia disclaimed German certificates of docent and professor. Since 1914 and up to 1925 Mandelstam was excluded from normal research work, he either worked in industry or taught (sometimes he worked as both an engineer and a teacher). During the revolution and civil war (and subsequent years of the economic devastation), there were not even the normal conditions for teaching and engineering work. It is worth noting that even after 1925, when Mandelstam became Professor at Moscow State University and had reached a stable social situation, the country was under repressions turning into terror turning into terror. The intelligentsia had to act as the "fellow travelers" who were obliged to prove their loyalty. Some of Mandelstam's former students were arrested and destroyed.

Finally, Mandelstam was of weak health and his health became worse year by year.

In tracing the life path of L.I. Mandelstam, we come to the theme of science, understood not only as a source of knowledge, but also as a source of vital energy, and even faith. At the end the 1920s and especially in the 1930s, Mandelstam had reached a stable (considering the times) social status and good financial situation. However, during the preceding decade teaching and research did not yield him anything financially significant. A feeling of science as a consolidating and creative principle helped him to stand frim. Mandelstam was a man of science; he quickly regained his scientific potential, once he found himself in favorable conditions for scientific activity and a circle of people who planned to start research formed around him. Science's worth is that it brings not only results but also hopes which strengthen the spirit! Here, we come to the interrelations of individual work and communication, specialization and broad horizons, physics and metaphysics.

1.2 What Has Been Written About L.I. Mandelstam?

The canonical biography of L.I. Mandelstam has been written by his friend and permanent coauthor Nikolai Dmitrievich Papalexy (1890–1947) [1, Vol. 1, pp. 7–31, 268]. Its title is "Leonid Isaakovich Mandelstam (a Short Outline of his Life and Activity)", and it has been published in the first volume of Mandelstam's "Complete Works". As is mentioned in the footnote, N.D. Papalexy, who died in 1947, was short of time to finish his "Short Outline".

In the first volume L.I. Mandelstam's "Complete Works", Papalexy's "Short Outline" is supplemented with a review of Mandelstam's activity in the last years of his life. The authors are the following: Grigorii Samuilovich Landsberg (1890–1957), Mikhail Alexandrovich Leontovich (1903–1981), Igor Evgenievich Tamm (1895–1971), Gabriel Semenovich Gorelik (1906–1957), Sergei Mikhailovich Rytov (1908–1996) [1, Vol. 1, pp. 7–66, 8]. In 1965, I.E. Tamm delivered a short paper in commemoration of L.I. Mandelstam [336].

N.D. Papalexy also wrote a Short Outline of Mandelstam's research in radiotechnology. He participated in the majority of that research [268].

In 1979, the collection of papers entitled "Academician L.I. Mandelstam: on the occasion of the centenary of the birth" appeared under S.M. Rytov's editorship. This collection is launched by N.D. Papalexy's "Short Outline" and by those supplements which the first volume contains.

Some letters to and from L.I Mandelstam are also published in this collection. These letters are taken from Mandelstam's collection which the Archives of the Russian Academy of Sciences contain. In addition, the collection of papers dedicated to Mandelstam's centenary contains the analytical articles and recollections describing Mandelstam's basic research and his teaching activity. These articles and recollections have been written by his colleagues and former students (in particular, Landsberg, Tamm, Andronov, Gorelik, Rytov, Shchegolev [18, 200, 301, 303, 305, 310, 333, 334]).

In 1945, A.A. Andronov with coauthors published a review of Mandelstam's research in non-linear oscillations [18]. S.M. Rytov, who was one of the authors of the biography of Mandelstam, published several articles dedicated to Mandelstam's ideas in the theory of oscillations [300, 302, 303, 305].

The above mentioned writings became the basis for the authors of the subsequent issues on Mandelstam and his work. These were Iakov Gr. Dorfman who wrote an article published in "Dictionary of Scientific Biography", Yu.A. Chramov who descibed Mandelstam's creative work in his reference-book "Physicists" and in his book about the scientific schools in Soviet physics (see above), V.V. Migulin who published an essay in journal "Priroda" [230] (see also [229, 231]), A.M. Livanova and V.A. Livanov in "The second degree of understanding" dedicated to L.I. Mandelstam [219], the historian of physics P.S. Kudriavtsev in his article about the Physics Department of Moscow State University [187]. It should be noted that Migulin, who was one of the youngest representatives of the Mandelstam school, described his research in radiointerferometry. In Livanova's paper we have her own recollections (she attended Mandelstam's lectures at the end of the 1930s), materials of the Mandelstam family's archives and interviews with Mandelstam's former students. The Livanov and Livanova's book followed the essay written by A.M. Livanova alone [220].

Two books have been published about Mandelstam's teacher the Nobel Prize winner Ferdinand Braun (1850–1918) [149, 192] (for the latter, there is an English version [193]) touch upon the Strasbourg period in Mandelstam's life (1900–1914). This period has been considered by the present author [271, pp. 74–77, 272, 273]. The present author also traced Mandelstam's contacts with the German engineer, mathematician and philosopher Richard von Mises [272, 275] and published the extracts from Mandelstam's letters to Richard von Mises.

The author of the present book has also published a number of articles about Mandelstam's philosophical position and his interpretation of quantum mechanics [80, 272, 275–278].

L.I. Mandelstam's biography is anyway touched upon in the books and articles about his disciples and collaborators [37, 49, 54, 56, 57, 80, 81, 142, 169, 224, 364, 374, 376, 377] and in the commemorations on Mandelstam's coauthors and other disciples. However, there are only a few new facts in these writings.

There are new facts in I.L. Fabelinsky's historical articles and his brochure dedicated to the discovery of the combinational scattering of light [97–102]. As was above pointed out, Mandelstam and Landsberg did not receive the Nobel Prize for this discovery. The Nobel Prize was given to the Indian physicist Ch. Raman, who together with Krishnan observed this phenomenon in liquid. Mandelstam and Landsberg observed it in crystals.

The new facts are present in V.P. Vizgin's articles about the March 1936 session of the Academy of Sciences [359–361], in G. Gorelik's articles about the 1937 philosophy discussions [139–141], in B.A. Minkus' commemorations about the Odessa period in Mandelstam's biography [235], in Andreev's book about the Research Institute of Physics at Moscow State University [12]. It was mentioned above that Mandelstam had worked for this institute since 1925.

The new facts (concerning presumably L.I. Mandelstam's colleagues) are given in the second edition of E.L. Feynberg's book, and some accents are placed in a new way [103, 104].

To sum up, it worth mentioning that Mandelstam's biography was basically outlined by N.D. Papalexy and some of L.I. Mandelstam's other colleagues in the 1979 book. What has been published later looks like supplements, appendices, comments.

1.3 About Key Points and Blank Spots

Let us mention some key points. In Mandelstam's biography which is published in the first volume of his "Complete Works", it is said that "the late decades of Mandelstam's life (after his move to Moscow in 1925) were the "golden age" of his scientific and pedagogical work. Groups of co-workers and students gathered around him. Many of them reached outstanding positions already during Mandelstam's life and became the guides of Mandelstam's ideas among wide groups of young people. Over this period Mandelstam delivered a number of important courses and seminars on fundamental problems of physics. The public recognition of Mandelstam's outstanding achievements and merits started from this. In 1928 Mandelstam was elected as a correspondent member of the USSR Academy of Sciences, and in 1929 he was elected as a full member. Mandelstam received very important prizes: the Lenin Prize (1931), Mendeleev Prize (1936) and Stalin Prize (1942). Mandelstam was decorated with the highest USSR honours: with the order of Red Banner of Labor (1940) and with the Lenin Order (1944).

Since 1925 Mandelstam had become the central figure of Moscow State University Physics Faculty. In 1934 the Academy of Sciences (Central Office) moved to Moscow and Mandelstam had taken an active part in the development of the Academy Physics Institute" [1. Vol.1, pp. 32–33].

E.L. Feinberg writes about Mandelstam in a different light. "Mandelstam's po litical position was trenchant and definite: he completely and sharply rejected the Soviet regime and all ideology and practice of Soviet life introduced by the Communist Party" [104, p. 47].

So, Mandelstam who actively and successfully participated in the life of two Soviet establishments (Moscow State University and the Academy of Sciences) rejected the theory and practice of Soviet life. How to understand it?

There are other questions. For example, we have no clear and distinct information on Mandelstam's work for the German radio industry. N.D. Papalexy only mentioned his cooperation with the company Telefunken and "the (for that time) big bonus which was given to him by the company" [1–3, Vol. 1, p. 12]. However, how close was this cooperation?

In 1907, Mandelstam published an article "About directed wireless telegraphy" in which he criticized the English radio-engineer John A. Fleming. Mandelstam again turned to the criticism of Fleming in 1930–33 (a couple of paragraphs in his "Lectures on Oscillations" and his review "Issues of the electrical oscillatory systems and radio-engineering" published in "Uspekhi fizicheskikh nauk").[1] Fleming probably did not know anything about this criticism. At least, the present author has not found any references to it in Fleming's publications and in his letters which the Archives of London University keep. Finally, Fleming is critically mentioned at the end of Mandelstam's 1944 lectures. These were the last lectures delivered by Mandelstam. He lectured no more.

What is the reason for the emphasis which Mandelstam placed on Fleming's approach?

The Odessa period in Mandelstam's life and work (1918–1922) is poorly elucidated.

L.I. Mandelstam was an influential figure in the Soviet Academy of Sciences. What position did he take up by participating in the Academy of Sciences meetings and conferences?

The present book tries to elucidate these and other questions. Mandelstam's biography as represented in it is documentary. This does not imply that I shall ignore the recollections about Mandelstam and his colleagues. It is not possible to write a book about Mandelstam without good attention to his friends' and coworkers' recollections. However, the sources will always be indicated in this book. The recollection will be separated from the sources of superior reliability, namely articles, letters, and administrative documents.

1.4 Sources

The basic source is L.I. Mandelstam's "Complete Works" (five volumes). Sometimes, I shall cite pieces from Mandelstam's German original papers rather than from their Russian translations which the "Complete Works" contain. Mandelstam's article jointly written with Papalexy dedicated to Ferdinand Braun will be used, too. Mandelstam's "Complete Works" does not contain this article.

N.D. Papalexy's articles collected in his "Collected Works" also shed light on Mandelstam's life and his research. The majority of them had been written as a development of their cooperation.

Mandelstam former students' books and articles (first of all Chaikin, Andronov, Vitt, Leontovich, Gorelik, Rytov should be mentioned) give an idea of the style of his instruction and inform about him as Founder of the scientific school. The Mandelstam

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[1] In particular in his Moscow lectures, Mandelstam criticized Fleming's explanation of resonance in [110].

collection, which the Archives of the Russian Academy of Sciences keeps, contains his autobiography, his letters and letters to him, the list of his publications and patents, his drafts, etc. Mandelstam's letters are kept in the collections of his friends and colleagues (S.I. Vavilov, V.A. Fock, N.D. Papalexy).

Administrative documents concerning L.I. Mandelstam are kept in the Archives of the Academy of Sciences Institute of Physics (FIAN).

The Archives of Lomonosov Moscow State University contain the documents which allow us to follow his career at the University.

The important source which was introduced by the present author to the history of science studies is a collection of Mandelstam's correspondence with Richard von Mises (the Harvard University Archives, HUG 4574.5, boxes 1–3).

Mandelstam's early scientific career is represented in the administrative documents of Kaiser Wilhelm Strasbourg University (Archives départementales du Bas-Rhin, Strasbourg, France).

These documents are supplemented with Ferdinand Braun's collection which is kept in Deutches Museum archives (Museum of the history of science and technology in Munich). J. Zenneck's collection which is kept there is also useful. The Deutches Museum library has a copy of J. Zenneck's "Erinnerungen eines Physikers" ("the Recollections of a Physicist") which describes some events in which Mandelstam has participated [392] (see also [391]).[2]

The Siemens Forum Archives (Munich, Germany) contain the materials about radio-engineering companies for which Mandelstam worked. These were Telebraun and Telefunken. In particular, these Archives have Mandelstam's letter to Ferdinand Braun and F. Braun's letter about Mandelstam to Wilhelm Siemens [400, LK-65].

Mandelstam's interpretation of quantum mechanics is rather popular among Russian scientists. This interpretation is represented in his 1939 Lectures on Quantum Theory. The collection of quantum physics (the Library of American Philosophical Society, Philadelphia, USA) helps us to understand the general situation of the 1930s in the interpretation of quantum mechanics.

J.A. Fleming's collection, which the Archives of London University (UK) keeps, contains his communication and manuscripts and sheds light upon the situation in radio-engineering and the radio business during the first decades of the 1920s. However, the present author has not found any references to Mandelstam.

The present author conducted a number of interviews with the people who were personally acquainted with either L.I. Mandelstam or with the people of his circle. This was an interview (19 December 1992) with Sergei Mikhailovich Rytov, who was Mandelstam's graduate student of a younger generation. As was noted, Rytov was Editor of the 1979 book dedicated to Mandelstam's centenary. Rytov has also performed the basic work of publishing Mandelstam's Complete Works.

Then we mention the interviews with the Nizhny Novgorod physicists who were personally acquainted with Mandelstam's former students A.A. Andronov and G.S. Gorelik. These were Yuri Isaakovich Neymark (2.7.2001), who was one of

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[2] J. Zenneck was one of the founders of the Museum of the History of Science and Technology, Deutsches Museum in Munich.

Andronov's last graduate students, Alexandra Grigorievna Liubina (3.7.2001), who was Andronov's student, his colleague and coauthor, and Mikhail Izrailevich Rabinovich (22.05.1992 and 30.3.1993), who was not Andronov's student, nevertheless was a person of Andronov's circle and considered himself to be a representative of Mandelstam's school.

Professors of Moscow Institute of Physics and Technology Stanislav Mironovich Kozel and Mikhail Vladimirovich Illarionov told the present author about G.S. Gorelik (21.12.1997).

The author has also had a conversation with Evgenii Livovich Feinberg, who communicated with Mandelstam's close colleagues and participated in the preparation of Mandelstam's "Complete Works" (23.12.1992).

In this book the interviews with S.M. Rytov and V.V. Migulin conducted by E. Gorelik are used. The American Institute of Physics (Historical Department) keeps these interviews.

E.S. Boyko, the author of two books about A.A. Andronov, handed over her materials about L.I. Mandelstam and A.A. Andronov to the present author.

1.5 The Plan

Often in the biographical writings published by Nauka publishers, the life story of a scientist is given separately from the analysis of his/her scientific results. In the present book, we describe our hero's life together with his creative work. We tend to describe the social and psychological circumstances of any considerable scientific result.

The main stages, however, were indicated by N.D. Papalexy and by the physicists who have supplemented his "Short Outline": (1) Strasbourg period—education and the beginning of scientific career (1899–1914), (2) years of traveling, the beginning in Petrograd (1914) and the end in Leningrad (1925), (3) Moscow State University (1925–1935), (4) the Academy of Sciences Institute of Physics and Moscow State University (1935–1941), (5) Kazakhstan resort Borovoie and the last year in Moscow (1941–1944).

Additionally, the formation of Mandelstam's school will be described, and brief biographical information will be given about the representatives of this school.

Chapter 2
Youth and Strasbourg Years

2.1 Youth

Let me cite a fragment of Mandelstam's autobiography dated 1929. This autobiography is contained in his personal file [399, 46-1-51].

> I was born in 1879. I received a secondarily education at the Second Odessa Gymnasium which I finished in 1897. In this year I entered the Faculty of Physics and Mathematics of Novorosiisk University (Department of mathematical sciences). I left the Novorosiisk University after four terms study and came to Strasbourg in 1900. There I entered the Physico-Mathematical Faculty where I specialized in physics under Professor F. Braun. In 1902 I obtained the degree of Doctor philos. Naturalia on the basis of my dissertation and examination. In the same year I worked on wireless telegraphy as a private assistant to Professor Braun in Strasbourg and Berlin. In the following years I was second then first assistant at the Institute of Physics of Strasbourg University and directed the student practical exercises in physics and research work of graduates. In 1907 I started to deliver lectures in physics as Privatdozent while holding the position of assistant. In 1913 I received the professorial rank and the Faculty's commission to deliver the course in applied physics. From 1907 till 1914 I delivered the following courses: electromagnetic oscillations, wireless telegraphy, the introduction to electrical engineering, the theory of telegraphy and telephony, resonance in physics, the theory of dispersion and other electromagnetic optical phenomena, the kinetic theory of gases. In July 1914 I returned to Russia.

With what to supplement this scanty line? A.N. Krylov, who became close to Mandelstam during the war years, said "Leonid Isaakovich was descended from a rich and highly educated Jewish family which lived in Odessa. His father Isaak Grigorievich was a well-known physician" [8, p. 85]. As L.I. Mandelstam did not fail to mention in his personal records of the Soviet times, his father was "the hereditary doctor and freeman" [399, 46-1-113].

Leonid Isaakovich's mother Mina Lvovna Kan was the stepmother of Leonid Gavrilovich and Alexandr Gavrilovich Gurvich (her mother, L.I. Mandelstam's grandmother Mina L'vovna Kan was remarried G.K. Gurvich, a notary from Poltava). Leonid Gavrilovich (1871–1926) became a prominent Petrochemist, and

A. Pechenkin, *L.I. Mandelstam and His School in Physics*,
https://doi.org/10.1007/978-3-030-17685-3_2

Alexandr Gavrilovich (1874–1954) became a prominent Physiologist. His biography is contained in the “Dictionary of Scientific Biography” where we find L.I. Mandelstam’s biography.

As Papalexy writes, Leonid Mandelstam left gymnasium with a medal and entered Mathematical Department of the Faculty of Physics and Mathematics at Novorosiisk University (Odessa). Papalexy also writes that Mandelstam was excluded from Novorosiisk University in connection with student unrest. In his autobiography, Mandelstam does not explain why he left Novorosiisk University for Strasbourg University. It is possible that Mandelstam’s parents simply preferred Strasbourg University since that was one of the best European universities and Strasbourg education in physics and mathematics was highly thought of.

According to Papalexy, in his childhood Leonid Mandelstam became close to Alexandr Gurvich who was just 5 years older than he was.[1] And Leonid Mandelstam’s entrance to Strasbourg University can be (at least, partially) explained by the reference to that Alexandr Gurvich had entered this university earlier.

Alexandr Gurvich, who lived in Geneva, came to Strasbourg to support his nephew, who went through his final examination. By proceeding from Mandelstam’s family legend, A.M. Livanova writes about the natural timidity of young Mandelstam. Before the presentation of his thesis, Mandelstam was in such a state big that A.G. Gurvich, who specially came from Switzerland, literally shoved his nephew into the audience where the examination took place [219, p. 170].

2.2 At Strasbourg University

According to the archival documents of Kaiser Wilhelm Strasbourg University [396], L.I. Mandelstam born in Mogilev, Jew, graduated from the university in January 1902 and became Privatdocent in February 1907. From 1903 to 1904, Mandelstam was Auxiliary Assistant at the Physics Institute. In 1904, he became Second Assistant, and in October 1906 he became First Assistant. When Mandelstam was Auxiliary Assistant, Jonathan Zenneck (1871–1959) was First Assistant. Zenneck became a famous specialist in radio-engineering, the author of the first fundamental textbook in radio-engineering. Herman Brandes was Second Assistant (he was born in 1870) at the time.[2]

After Zenneck left in 1904, Herman Brandes became First Assistant, and Mandelstam became Second Assistant. Brandes left after obtaining his doctorate in 1906 with a dissertation on “Damping and Energy Efficiency of Several Transmission Configurations in Radiotelegraphy”, and Mandelstam became First Assistant. In 1906, there was the following team of assistants: Leonid Mandelstam, Gustav Aeckerlein, Max Dickman (1882–1960). Dickman’s subject was wireless communication in aviation, and he became a prominent figure in radio-engineering [374].

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[1] See the biography of A. Gurvich [35].

[2] The author does not know the year of his death. Brandes published a number of articles on the theory of measurement in radio-engineering.

As mentioned in the Introduction, L.I. Mandelstam's Supervisor was Prof. Ferdinand Braun, Director of the Institute of Physics, which was a part of the Kaiser Wilhelm Strasbourg University. L.I. Mandelstam's Ph.D. thesis (for Russian readers: it corresponds rather to the diploma work) has been devoted to the then actual problem of radio measurement. Specifically, the theme was: "Determination of the vibrational period of the capacitor discharge". In the same year, 1902, Mandelstam's article representing his thesis was published in the prestigious journal "Annalen der Physik".

As Mandelstam had been included into the staff of the Physics Institute, F. Braun became his chief. According to Jonathan Zenneck's characterization [392, p. 62], which Mandelstam and Papalexy shared [4, p. 621], F. Braun was the "ideal chief". "As Teacher", Mandelstam and Papalexy write, "F. Braun will not be forgotten by everybody who was lucky to work at his institute. He did not prevent anyone to follow one's individuality. With constant interest he followed every research and was ready to consult and to help. His consultations always made the situation clear. Braun had his own view on experiment and he was possessed of an intuition which was not achieved by training. This intuition was given by nature. This provided a success in research. These qualities have been passed on to his students" [ibid].

J. Zenneck left Strasbourg to take the position of professor extraordinary at Higher Engineering School in Danzig (now Gdansk). In the next year, he took the position of full professor at Higher Engineering School in Braunschweig. In 1911, he returned to the more prestigious higher school in Danzig by receiving the position of full professor.

His correspondence with Ferdinand Braun (Deutsches Museum Archives) evidences that he kept connections with his Strasbourg colleagues.

Zenneck was one of the organizers of the journal "Jahrbuch des drahtlosen Telegraphie und Telephonie", and his specific role in the publishing of this journal was mentioned in the title. F. Braun and L.I. Mandelstam were Members of the editorial board (Fig. 2.1).

In his book "Electromagnetic oscillations and wireless telegraphy" (1905), J. Zenneck mentioned all three Mandelstam's papers which had appeared before. In the Foreword, Zenneck expressed his gratitude to Dr. Mandelstam, who had read the chapters on radio-engineering and made useful comments [386].

In 1908, Zenneck simplified his book and published a course oriented to engineers ("Leitfaden" which means "elements"); after its further revision (1912), it became a textbook ("Lehrbuch"). In all his books, Zenneck provided a review of the writings of his Strasbourg colleagues, including Mandelstam's articles [387].[3] In 1908 Zenneck in coauthorship published a book dedicated to high frequency technology.

L.I. Mandelstam kept on his work as First Assistant until his departure for Russia (June 1914). In particular, he was dissertations. Judging by the references in Zenneck's textbook, he supervised Max Dickman's and Herman Rohman's dissertation works (1907 and 1912, respectively). In 1912, Rohman (1886–1931) replaced

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[3] A Russian translation of "Leitfaden" was published in Russian in 1908 [389], and an English translation of "Lehrbuch" was published in 1915]. There are other editions of the English version.

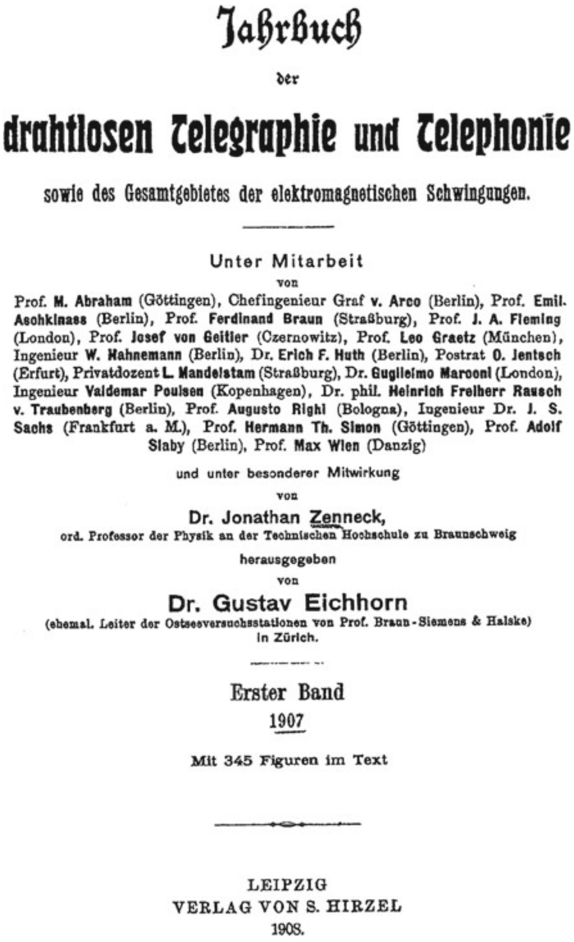

Fig. 2.1 Frontispiece of Journal “Jahrbuch der drahtlosen Telegraphie und Telephonie”

Jahrbuch
der
drahtlosen Telegraphie und Telephonie
sowie des Gesamtgebietes der elektromagnetischen Schwingungen.

Unter Mitarbeit
von

Prof. **M. Abraham** (Göttingen), Chefingenieur Graf **v. Arco** (Berlin), Prof. **Emil. Aschkinass** (Berlin), Prof. **Ferdinand Braun** (Straßburg), Prof. **J. A. Fleming** (London), Prof. **Josef von Geitler** (Czernowitz), Prof. **Leo Graetz** (München), Ingenieur **W. Hahnemann** (Berlin), Dr. **Erich F. Huth** (Berlin), Postrat **O. Jentsch** (Erfurt), Privatdozent **L. Mandelstam** (Straßburg), Dr. **Guglielmo Marconi** (London), Ingenieur **Valdemar Poulsen** (Kopenhagen), Dr. phil. **Heinrich Freiherr Rausch v. Traubenberg** (Berlin), Prof. **Augusto Righi** (Bologna), Ingenieur Dr. **J. S. Sachs** (Frankfurt a. M.), Prof. **Hermann Th. Simon** (Göttingen), Prof. **Adolf Slaby** (Berlin), Prof. **Max Wien** (Danzig)

und unter besonderer Mitwirkung
von
Dr. Jonathan Zenneck,
ord. Professor der Physik an der Technischen Hochschule zu Braunschweig
herausgegeben
von
Dr. Gustav Eichhorn
(ehemal. Leiter der Ostseeversuchsstationen von Prof. Braun-Siemens & Halske)
in Zürich.

Erster Band
1907

Mit 345 Figuren im Text

LEIPZIG
VERLAG VON S. HIRZEL
1908.

M. Dickman and became Auxiliary Assistant. Mandelstam went on to cooperate with Rohman, and in 1913 they published an article on the reflection of X-rays.

Rohman not only became made Auxiliary Assistant at the Institute of Physics. He married Ferdinand Braun’s daughter, and his signature is among of the F. Braun family under the notification about F. Braun’s death which occurred in 1918.

As Papalexy writes, “in 1907 Mandelstam married Lidia Solomonovna Isaakovich, the first Russian woman to get a diploma in architecture in Paris” [1, Vol. 1, p. 22]. In Strasbourg, she received the diploma of physician, too. In 1918 when she, together with Leonid Isaakovich, lived in Petrograd, she worked as Urologist.

In connection with Mandelstam’s marriage, F. Braun sent an application to the curator of Strasbourg University with the support of Mandelstam who had not needed more than the two-room apartment at the building of the Institute of Physics. One room, however, was kept for Mandelstam's meeting with students [396].

Thus, Mandelstam moved from a rent-free apartment. His financial position allowed him to do so.

In 1910 in Odessa, to the Mandelstam family a child was born. L.I. Mandelstam and L.S. Mandelstam named their son Sergei.

In 1904, L.I. Mandelstam began to collaborate with N.D. Papalexy, who graduated from Strasbourg University in 1904.[4] Although Papalexy's name is not in the staff register of the Physics Institute, he participated in Braun's and his assistants' radiotechnological experiments. In 1906, Mandelstam and Papalexy published an article dedicated to a method of obtaining oscillation lagging, but identical in shape. This Mandelstam–Papalexy research arose within F. Braun's project of directed wireless telegraphy. Braun proposed an antenna consisting of three vertical poles standing at the apexes of a rectilinear triangle. This antenna presupposed a new method of obtaining the lagging oscillations.

Mandelstam and Papalexy went on with their collaboration through their Strasbourg period, and then they worked together in Odessa, Saint Petersburg, and Moscow. Mandelstam and Papalexy became close friends. It is interesting, however, that the verbal ethic of the German university graduates, who called each other "Herr Doctor", influenced their style of communication. Throughout their life, they addressed each other as "Sie" and called each other by first name and patroname.

In his autobiography (see above), L.I. Mandelstam wrote that in 1913 he was invited to deliver the lectures on applied physics. There was the following chain of events. In 1908, German postal authorities established an examination for the competitors for the authoritative positions. Strasbourg became one of the cities where the examination would take place. Ferdinand Braun capitalized this situation to his institute's benefit. He organized the three-term course (a kind of school) including lectures, laboratory studies, and seminars [396]. During the week, two or three hours of lectures, five hours of laboratory studies, and two hours of theoretical seminars, were held.

All studies were carried out by Mandelstam. In fact, he organized the delivery of equipment. Siemens Company was a supplier of this equipment. Ferdinand Braun planned to borrow the equipment for laboratory studies from Siemens Company. This company, however, had kindly presented the equipment as a gift.

In 1911, Ferdinand Braun asked the curator to approve the course which Mandelstam taught. This course embraced electrical engineering, and wireless and ordinary telegraphy, and it was named "applied physics". F. Braun asked to officially entrust Mandelstam with the delivery of "applied physics" [396].

However, this instruction was issued in 1913 only. In 1913, Mandelstam also received the title of Professor. There were four types of professors at Strasbourg

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[4] N.D. Papalexy was born in the family of the Major of 51st Lithuanian regiment Dmitry Papalexy in 1890. He belonged to the nobility. In 1899, he finished Odessa gymnasium with a gold medal. Before he entered Strasbourg University, he was a student at Berlin University for one year ([399, stock 600, inventory 2, items 1–4]). In 1911, Papalexy took the status of Privatdocent.

University: Ordinarius, Extraordinarius, nonscheduled (ausserplanmässiger) professor, and titular professor (Honorar-Professor). Ordinarius was a full professor and Chair. The university as a corporative organization was embodied by the Collegium of Ordinarius professors (Ordinarien). An extraordinarius was not Chair and his salary was less (Privatdozent did not have a constant salary, his payment depends on the number of students who attended his lectures). The title of Ausserplanmässiger Professor was close to Ordinarius, but did not lead to the privilege of Ordinarius. Mandelstam received the title of Professor. He could be addressed as Professor, his career potential became higher, but for the rest he did not differ from Privatdozent (see Mandelstam's certificate of Professor on Fig. 2.2).

As Papalexy writes in his biography of Mandelstam, Mandelstam together with his wife and son arrived in Odessa on the day of the proclamation of hostilities. On October 1914, Ferdinand Braun issued a circulation stating that as First Assistant Prof. Dr. Mandelstam was abroad, Second Assistant Gustav Aeckerlein was entrust to take up Mandelstam's duties and the duties of Auxiliary Assistant H. Rohman were extended [396].

Before the circulation was issued by Braun the Senate of Strasbourg University deprived Russian docents of the right to teach (on 18 September 1914).

2.3 Strasbourg University

So, L.I. Mandelstam not only graduated from Kaiser Wilhelm Strasbourg University, but started his scientific career by working for this university. This university, which was founded in 1872, was being organized as an outpost of the German culture in the West, and it was called up for Germanization of Alsace, the controversial area located between Germany and France which Germany owned. In the beginning, the local population was rather skeptical with respect to the university. Alsatians initially treated the university as an alien institution. On the front of J. Craig's book dedicated to the history of Strasbourg University, there is a picture of how German students solemnly going under the banners to the university and how Alsatians looking at them with mere curiosity [83].

However, in the course of the decade Strasbourg University was not only integrated into the local cultural life, but also became its important factor. This at least partially resulted from the Strasbourg University line to the world teaching level. As an educational center, the Strasbourg University fulfilled the expectation of the most demanding. "With its productive young professors, its industrious students, and its magnificent library and laboratories, it quickly gained a position among the world's most distinguished universities" [83, p. 100].

One of the achievements was the division of the Faculty of Philosophy and the formation of the Faculty of Natural Sciences (1872). L.I. Mandelstam would later be one of the students of this Faculty.

Another achievement was the creation of Institute of Physics at the Natural Science Faculty (also in 1872). The founder of this institute was August Kundt (1839–1894), who contributed to the kinetic theory of gases and conducted the experiments on

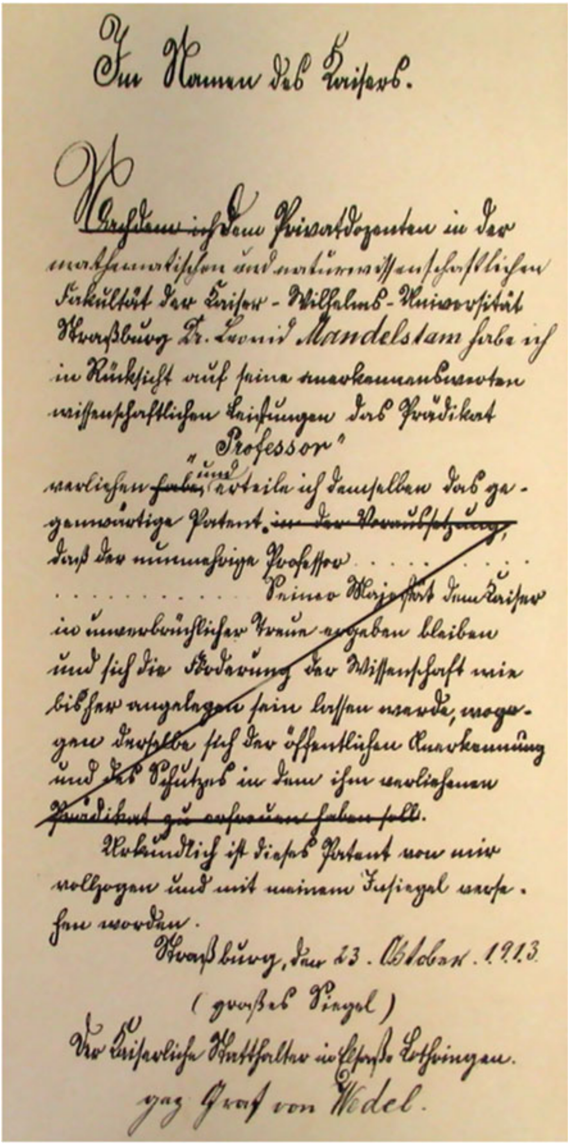

Fig. 2.2 Mandelstam's certificate of professor

Im Namen des Kaisers.

~~Nachdem ich~~ dem Privatdozenten in der mathematischen und naturwissenschaftlichen Fakultät der Kaiser-Wilhelms-Universität Straßburg Dr. Leonid Mandelstam habe ich in Rücksicht auf seine anerkennenswerten wissenschaftlichen Leistungen das Prädikat „Professor" verliehen ~~habe,~~ und erteile ich demselben das gegenwärtige Patent. ~~in der Voraussetzung, daß der nunmehrige Professor . . . . Seiner Majestät dem Kaiser in unverbrüchlicher Treue ergeben bleiben und sich die Förderung der Wissenschaft wie bisher angelegen sein lassen werde, wogegen derselbe sich der öffentlichen Anerkennung und des Schutzes in dem ihm verliehenen Prädikat zu erfreuen haben soll.~~

Urkundlich ist dieses Patent von mir vollzogen und mit meinem Insiegel versehen worden.

Straßburg, den 23. Oktober 1913.

(großes Siegel)

Der Kaiserliche Statthalter in Elsaß-Lothringen.

gez. Graf von Wedel.

the magneto-optic effect. Kundt was concerned with experimental facilities for the institute. For the Strasbourg Institute of Physics, a special building was projected and constructed; in this relation, it was the first institute in the world. This building was projected according to August Kundt's ideas.

In 1888, A. Kundt left Strasbourg University for Berlin University where he accepted Chair of physics, one of the most important chairs in Germany. Friedrich Kohlrausch took the position of Director of Institute of Physics in Strasbourg. However, in 1895 Kohlrausch became Director of the State Institute of Physics and Technology (physikalisch-technische Reichsanstalt) in Berlin (as successor to the great Hermann von Helmholtz) and took the chair of physics at Berlin University (Kundt died in 1894). Ferdinand Braun was invited to take charge of Strasbourg physics and he became Mandelstam's teacher.

Although Kundt was an Experimentalist, he understood the importance of the new discipline, theoretical physics. His research on the kinetic theory was conducted together with Emil Warburg who became Extraordinarius, representing theoretical physics in Strasbourg. As a matter of fact, Strasbourg Institute of Physics was among the first where the term "theoretical physics" became commonly used. Earlier, the term "mathematical physics" was universally accepted .[5] F. Braun was also inclined to cooperate with theoreticians. Emil Kon (1854–1944) was Extraordinarius representing theoretical physics in F. Braun's period. Emil Kon was the author of the textbook which enjoyed wide popularity, the textbook on electrodynamics.

Traditionally, mathematics was strong at Strasbourg University. H. Weber (1842–1913) taught there in F. Braun's time. He was the author of the famous course of the equations of mathematical physics, the course which arose on the base of Riemann's "Lectures" (Riemann B. "Partielle Differentialgleichungen der Physik", 1869). Following the established terminology, L.I. Mandelstam mentioned this course as Riemann-Weber in his letters.

As follows from Mandelstam's biography written by Papalexy, Mandelstam took the courses delivered by E. Kon and H. Weber. In the Archives [399, File 600], one can find Papalexy's records of E. Kon's courses "The theory of light" and "Contemporary research in electricity". Among mathematical disciplines that had been taken up by Papalexy (and probably by Mandelstam), there were the theory of potential, the theory of numbers, and analytic geometry.

It is worth to mention the themes of the graduation theses represented in 1912: "The foundations of Lorentz' theory of transformations", "The behaviour of ionized spark gap in a bond condensator circuit", "Optical and electrical behaviour of iode vapor" [396, AL 103, 1488].

The American Historian of science D. Cahan writes that German universities experienced an institutional revolution at the beginning of the second half of the nineteenth century: Their cabinets and laboratories had been transformed into research institutes [73]. Strasbourg University was in a better position. It had not experienced a radical transformation. Its Natural Science Faculty arose together with the Institute of Physics.

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[5] About the development of the speciality "theoretical physics" see [167].

The national politics of Strasbourg University should be noted. From the very beginning, this university was shaped as a European scientific center. Strasbourg University invites both local citizens and people from other regions to teach. The characteristic message of Strasbourg University was ethnic and religious tolerance. Among the university teachers, there were Protestants of different confessions, Catholics, and Jews. For example, according to the university documents, L.I. Mandelstam was a Jew (see above), and N.D. Papalexy was a Greek Catholic (which means that he belonged to the confession which followed the Orthodox Church Ceremony, but acknowledged the authority of the Pope).

Mandelstam's and Papalexy's Teacher Ferdinand Braun (Karl Ferdinand Braun) was a characteristic representative of Strasbourg University culture. Since not only Mandelstam and Papalexy, but also their followers considered themselves to be elements of the intellectual chain which started from F. Braun, his biography and Weltanschauung should be specially described.

2.4 Ferdinand Braun

The biography of F. Braun is described in two books. F. Kurilo's book [192, 193] attempts to bring back the wrongly forgotten F. Braun to the history of physics. F. Hars puts the other problem: to follow the reasons why F. Braun has been forgotten [149].

F. Braun followed the path typical for the German physicist of his time: "Extraordinarius at a University, Ordinarius at a Higher Technological School, Ordinarius at a University". Braun was a student at Marburg University and then Berlin University where Professor Georg Quincke (1834–1924) became his scientific advisor. Georg Quincke conducted research in capillary phenomena, behavior of materials in electric and magnetic fields, refraction of light. Sometimes, Quincke's interferometer is mentioned in contemporary textbooks [298]. However, Quincke is mentioned even less than Braun.[6]

In 1877, Braun obtained his first academic position, and he became Extraordinarius of mathematical physics at Würzburg University. In 1880, he obtained the same position at Strasbourg University where A. Kundt became his chief. In 1883, he left Strasbourg to take the position of Ordinarius at Higher Technological School at Karlsruhe. In 1895, he returned to Strasbourg to become Ordinarius of experimental physics. Braun left Gemany for the USA in 1915, and he died in the USA in 1918.

Like many physicists of the nineteenth century, F. Braun was mostly an experimentalist, and he conducted both fundamental and applied research; moreover, he combined research in physics and research in chemistry. To characterize F. Braun's achievement, let me cite Orest Chvolson's textbook popular in Russia at the first decades of the twentieth century. O. Chvolson points out ten results of F. Braun [82, Vol. 2, pp. 629, 707; Vol. 3, pp. 375, 587; Vol. 4, pp. 531, 603, 604, 624; Vol. 5, pp. 37, 150]. For example, he describes the phenomenon of electrostenolysis

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[6] About Quincke see my new book "The history of research on chemical periodic processes", Springer, 2018, pp. 6–7.

(after the Greek word “stenon”, which means narrowness) discovered by Braun: If the current through certain electrolytic solutions is made to pass through a narrow slit in a dividing wall, the slit begins to act as a third electrode (metal particles are deposited there, and bubbles of gas are formed).

This discovery is also described in Kurilo’s book about Braun [192, p. 76].

Even if Braun’s name is mentioned in contemporary textbooks, it is mentioned in connection with two discoveries: the Le Chatelier-Braun principle and cathode-ray tube.

Le Chatelier’s principle is the name given to the principle according to which a change in a chemical system in a steady equilibrium state prompts an opposing reaction. In chemistry, this principle was discovered independently by Henry Louis Le Chatelier and Karl Ferdinand Braun. Braun formulated this principle by studying the influence of pressure and temperature on solubility.

The cathode-ray tube was constructed by F. Braun in 1897, and it is applied to radio-technological measurement by him and his students (see [217]). The title of his 1897 paper is “On a method of demonstrating and studying the time dependence of variable currents”.

Braun’s results in radio-technology are not represented in contemporary textbooks on physics. Sometimes, there are references to them in books on electronics. However, Braun obtained the Nobel Prize in physics for his research in radio (wireless telegraphy). This Nobel Prize was shared by two persons: F. Braun and Guglielmo Marconi (1909). Braun came to study radio after Marconi (1874–1937): He improved the transmitter invented by Marconi in 1896. However, as is said in the biography of Braun, in all his reviews on radio-engineering “he invariably started from Heinrich Hertz”. Braun not only followed Marconi, but put research on the path of physical experiment and theory. He directly proceeded from the ideas of Hertz, who started to broadcast and receive radio signals in his laboratory as early as in 1887 by proceeding from Maxwell electrodynamics.[7]

Hertz’ transmitter went down in history as the “Hertzian dipole”. Two conductors were connected with an induction coil, and these conductors were placed close to each other and had two small balls at their ends. The induction coil produced the high voltage between the currents. A spark arose between the balls, and correspondingly electromagnetic oscillations arose in the system. The system radiated electromagnetic waves.

Hertz also constructed a resonator to pick up the “Hertzian dipole” radiation.

By proceeding from the ideas of his Teacher Augusto Righi (1850–1920), Marconi increased the power of the Hertz dipole. He equipped this dipole with an antenna, a wire placed at a height above the ground. In Germany, the Marconi experiments were reproduced by Adolf Slaby (Technical University of Berlin, Charlottenburg), who introduced his improvements. As Historian of radio-technology, V.M. Rodionov writes, “in the early radiocommunication the typical scheme for the transmitter sprang

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[7] According to J. Zenneck, F. Braun’s former student, with whom Braun collaborated, “radio was discovered by Hertz and Popov. Due to Marconi we practically have wireless telegraphy” [391, p. 409].

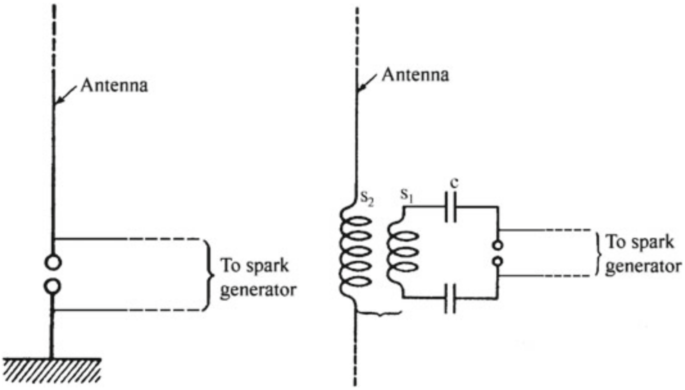

Fig. 2.3 Transmitter of Marconi is *on the left* and *on the right* the transmitter of Braun

up: a scheme consisting of high-voltage coil, power source, breaker, manipulator and radiating wire which is a frequency specifying oscillatory system" [297, p. 115]. In 1898, F. Braun proposed an alternative scheme of the transmitter (Fig. 2.3).

Braun set up Marconi's circuit, but with the modification that had proved so effective in improving the conduction telegraph: a primary coil in the oscillating circuit and a loosely coupled standard coil to transfer into the antenna-to-ground circuit [297]. In 1909 in Stockholm on receiving the Nobel Prize, Braun delivered a lecture [64].

In 1910, this lecture was published in Russian by L.I. Mandelstam and N.D. Papalexy. In the Foreword, they wrote the following:[8]

> Braun arrived to the conclusion that the problem of how to construct a powerful transmitter included two different problems: (1) the problem of how to come to high frequency current, (2) the problem of how to reach the rational radiation of electromagnetic waves. An antenna is a good radiator, however, it is not efficient as a generator of the high frequency current. One firstly needs to obtain high frequency current outside the antenna, then to deliver this current to the antenna by giving the antenna to fulfill its function (to radiate electromagnetic waves). The simple transmitter is not rational since its antenna is used as both a generator and a radiator. As a generator Braun used a closed circuit. It resulted in alternating current which was transferred to the antenna... One of the advantages of such a division of the functions is the possibility to reduce the unfavorable effect of the spark....
>
> Braun's idea was the following. Upon the break of a spark gap, rapid electric oscillations arise in the closed circuit. These oscillations are transmitted to the antenna. After a time, most of energy is concentrated on antenna. Energy cannot pass back to the closed circuit, since the spark is not able to transmit. At this moment, its unfavorable effect ceases. [63, p. VII]

F. Braun and his assistants improved the above transmitter. In 1903, Braun started his experiments with the directed wireless telegraphy, which would allow to broadcast to a selected region.

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[8] This Foreword is reprinted in Mandelstam's "Complete Works" [1, Vol. 3].

Why was F. Braun, the Nobel Prize winner for physics, forgotten? F. Haas answers this question in such a way: Braun was a nineteenth century physicist who turned out to be in twentieth century. His main achievements are historically and logically connected with classical physics of which the acme time fell on the second half of the nineteenth century. At the end of the nineteenth century physics lived by anticipating the future of great events, in the first decades of the twentieth century physics experienced these events. F. Braun did not participate in them.

Like the majority of physicists of the nineteenth century, Braun was a multifield Scientist. He contributed to many fields of physics, and he actively worked in chemistry. The Le Chatelier-Braun principle and the phenomenon of electrostenolysis have been already mentioned. However, in the famous five-volume book on the history of chemistry by Partington one can even find the Braun reaction. But in the twentieth century, the physicists who deeply penetrated into one problem or into several interconnected problems regularly won.

Moreover, those who concentrated on atomic physics or on the problem of absolute and relative motion, took the palm.

At the second decade of the twentieth century, Braun experienced discomfort by meditating on his status in physics. The paragraph in his 26.9.1912 letter to Zenneck is evidence of discomfort (this paragraph is taken by F. Haas as the epigraph). "By the way I see that requirements abundantly and steeply rose. Who did not deal with the principle of relativity, who did not read Sommerfeld's discussions in the café and were not be able to do other such things, lost.... A technician only wants his money, this is simply, this is the direct way" [149].

Braun meant the famous informal seminars which A. Sommerfeld started to conduct as he became Professor of theoretical physics at Ludwig Maximilian University of Munich (see: [94, pp. 247–248]). Peter Debye, Werner Heisenberg, Wolfgang Pauli participated in this seminars.

However, Hars is not entirely right by insisting on the archaism of his hero. F. Braun was a Physicist who contributed much to technology. An integration of physics and technology in one institute was of much importance for him. Braun can be considered to be one of the originators of physico-technological research and physico-technological education, and in this respect he can be considered to be a predecessor of the twentieth-century science.

In his lecture dedicated to Kaiser's birthday, Braun told about "the generations of thinkers and poets who would take the leading role in technology on the wave of national enthusiasm" [59, p. 23]. As an organizer of science, he posed the problem more sharply. He not only oriented Strasbourg Institute of Physics toward radiotechnology, he proposed the sixth faculty in the structure of Strasbourg University, the Faculty of Technology (see [59, p. 23]). In such a way, he proposed to react to the success of higher technological schools and to organize education in the new dynamic field—radiophysics (elements of statistics in the amount of higher technological schools are given).

In his 1905 lecture delivered before the full gathering of professors of the Strasbourg University, Braun spoke about the fusion of pure and applied science. "Sometimes—especially in the last years—the idea of opposing pure science to applied research has taken the floor. For its high status natural science is indebted to its industrial applications. Here one makes sacrifices, this is justified. However, before to apply something, one needs to have it. Usually priority belongs to pure science" [59, p. 22]. Later in the Soviet Union, Mandelstam and Papalexy used the principle of the unity of fundamental and applied research in their rhetoric by reacting to the state slogan to make science close to life.

The technological faculty did not arise within the framework of Strasbourg University. Technological higher schools were becoming more and more influential in German science and education. They were equalized by right with universities with respect to the Ph.D.

In favor of F. Hars, however, it should be noted that F. Braun remained distant with respect to the modern idea of engineering physics. He tried to bring radio-engineering up to the status of a physical discipline. However, there was another direction: to develop research in physics for the production of new effects. For example, P.L. Kapitsa worked in Cavendish Laboratory under Ernest Rutherford in 1923–1925 and later in the USSR toward the same goal.

2.5 Braun's Philosophy of Science

Hars referred to the philosophy of Braun as eclectic as it combines both empirical criticism and Kantianism. This calls for an explanatory comment. There are no logical contradictions in Braun's philosophical detours. Braun formulated his philosophy of science with such caution as to allow him later to review the tendencies in the development of science and point to the problems that he considered to be essential. Here, we will focus on two lectures by Braun, mentioned above, which he delivered at the Strasbourg University. The first—in 1899—was held in honor of the birth of Kaiser, and the second took place in 1905, when Braun represented Strasbourg University as its rector.

The 1899 lecture was entitled "*Über Physikalische Forschungsart*", which could be translated as "on a kind of research (peculiar to) physics". This lecture is imbued with the spirit of Kantian philosophy which is approached in a quite modernist manner and is rather loosely interpreted. Braun's lecture begins with an overview of the development of physics from Galileo and proceeds to Newton who developed a theory that would form the basis of textbooks, proposed a method for explanation in physics, and also proposed the idea to rely on forces of attraction between particles. It is interesting that Braun annotated this piece of Newton when his lecture was published. Braun contrasts Newton's method in the spirit of which electrical and magnetic phenomena were treated at the end of the eighteenth century and at the beginning of the nineteenth century with the method of "integral laws" and treated the latter as more promising.

In the Russian literature, the method of Newton is described as the "method of molecular mechanics" [62, pp. 118–132]. This is a method of explaining physical phenomena by analogy with Newton's explanation of Kepler's laws and of tides.

This explanation is based on the forces of attraction (and in some situations of repulsion) acting between particles. These forces do not necessarily obey the inverse square law, and their action does not necessarily follow the three laws of Newton. Rather, the "method of molecular mechanics" involves the formulation of new laws that would describe the action of attractive and repulsive forces between particles. Coulomb's law, Ampère's law, and a number of rules describing chemical affinity were discovered and formulated within this framework.

By chemical affinity is understood the force which manifests itself in chemical processes. This is a force of chemical interaction that is treated as an attraction of the chemical elements' atoms. The peak of the Newtonian theory of chemical affinity was the law of mass action, where the affinity (reaction rate) between two substances A and B is described as a product of "mass action" (the concentrations of reacting substances A and B), and each raised to a particular power equal to a corresponding stoichiometric coefficient in the equation of the reaction (if $aA + bB = C$, the reaction rate is proportional to $[A]^a\ [B]^b$).

Braun used the predicate "integral" for laws which connect macroscopic magnitudes to each other. These laws do not apply to the forces acting among invisible particles. For Braun, the law of conservation of energy was an important example of the "integral law". In the spirit of the physicists, who proclaimed energetism, Braun stated that "along with indestructible substance there is another constant magnitude, the energy of the universe" [59]. According to Braun, the law of conservation of energy is based on facts and plays the role of a "regulative principle": It demands that in the course of natural processes the amount of total energy should remain constant.[9]

In addition to the law of conservation of energy, Braun treats Faraday's law of electromagnetism as integral. It is possible to see the Maxwell equations in view of Faraday's results. However, Braun spoke about Faraday's original observations and was not in touch with their proper mathematization. For Braun, it was important that Faraday explained electric and magnetic phenomena by referring to the states of a macroscopic substance, the electromagnetic field. By extrapolating Faraday's approach, Braun wrote that it would be incorrect to explain the fall of a stone by referring to the force of Earth's attraction. The field lines of Earth's gravitation explain this effect. Braun did not reject any of the real methods and rather spoke in favor of pluralism. He emphasized that physics should not proceed from the generalization of empirical data and that research in physics presupposed "a skillful combination of facts". "The skillful combination of facts", Braun said, "provides a happy penetration to the essence (we characterize this penetration as intuition). This is a starting point of any research in any field" [59, p. 12].

Braun directly turned to Kant when he spoke about causality and cognizability of the world.

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[9] Under the name "energetism", the history of philosophy combines different concepts, which consider energy as a substance (either the only absolute substance or, along with matter, one of two basic substances).

> We proceed from the principle of cognizability of nature. We also accept the concept of causality (although its meaning and range are under discussion). Although nature is not so far comprehended as a whole, all known facts are ordered in accordance with the categories of understanding and can be logically interpreted as cause and effect [...]. According to the common use of the word, cause occupies the primary position in this one-to-one and reversible relation. We also expect that phenomena should be connected by not only qualitatively via logical laws, but also quantitatively, say, via the law of conservation of energy.
>
> Thus, we establish a kind of integrity of spirit and nature. But we repudiate an interpretation of this integrity as an a priori construction of real phenomena by means of our spirit. By following Kant we postulate that our spirit is able to cognize the world of phenomena. However, we do not know a priori in which way new contents would fill our cognitive structures, which by themselves are empty. We learn this by means of experience. If we are constructing a priori, we create a number of probable and possible universes which exist only logically and do not need to be real. Deductive research consists of constructing of a possible nature. Experience teaches us that a priori constructions seldom turn out corresponding to reality. [59, pp. 16–18]

Braun speaks about the inclination of German spirit toward *Naturphilosophie* and hence toward a priori theorization. He also contrasts apriorism with the Newton–Faraday empirical method. So, Braun accepted Kant's philosophy but rejected radical apriorism. For Braun, Kant's philosophy is rather a starting point for his own methodology. Scientific research is characterized by its method which could be changed under the pressure of empirical facts. For example, there is a method to proceed from the integral laws, say, the law of conservation of energy. However, this method is not absolute. Braun said,

> And then I ask, how does this aspiration for novelty (which is present in every person) pave its way? In other words, we know that nature has laws, for example, the law of conservation of energy which is confirmed by facts. However, we are able to imagine that we consciously seek and yet cannot find a confirmation to this principle. To do this would be enough, so we just did not notice some kind of substantial magnitude characterizing a small form of energy. If with Kant we assume that a thing in itself exists, a thing which may not be exhausted by its forms of expression that are appropriate to our cognitive abilities, then is it not surprising that we still give a quantity, and, in accordance with our logic, a value to a quantitatively essential element of the chain of transforming one into another forms of energy? Or is it just a coincidence? Are there other immanent laws of nature that we are, due to a lack of relevant organizations, never to learn? Why does our common sense try to escape from the latter assumption? [59, pp. 20–21]

Braun further points out that these issues are already present in the field of philosophy, especially metaphysics, which has attracted the attention of humanity for a long time, and in which there are no final answers. According to Braun, philosophy provides the necessary balance between "positive knowledge" and "unknown gloom" surrounding any new scientific problem. In contrast to philosophy, "positive knowledge", however, is focused on practice. "Aber wie für transscendentale, so gibt die Naturforschung ihre Resultate auch ab zur Verwertung in ein nach der anderen Seite gelegenes Gebiet, das reale, der praktischen technischen Anwendung" (however, with respect to the transcendental, scientific research provides its results for application on the other side of an adjoining area, actually in the field of practical technological activity) [59, p. 22].

In other words, Braun, highly appreciating Kant's philosophy, distinguished between two principles of scientific inquiry—namely the transcendental (mental designing) and the real (observation, experimentation, technical application). Both of these principles are in constant development and interaction. For Braun, as a representative of classical physics, cognizability of the world is connected with the implementation of the law of causality. Knowledge of the world is the knowledge of causal relationships, in which the transcendental cause and effect model is performed with experimental facts. Braun, however, presupposed the relativity of scientific laws, even the relativity of such a general law as the law of conservation of energy. This law is based on a mental scheme "in the transformation of qualitatively different forms of energy their quantitative equality is valid". This notional circuit is filled with facts—with the results of measurements of energy. It directs the researcher to search for new forms of energy. If somewhere the equality of energies is violated, then some form of energy has not been taken into account. Yet, situations are possible where the discrepancy between a thought scheme and empirical facts shows that the law of energy conservation becomes problematic.

Braun took into account the situations where the thought schemes do not regulate empirical facts, the situations when possible worlds are being constructed: for example, a world where the law of conservation of energy is not true. However, as Physicist he was not interested in discussing such worlds. These are the worlds for philosophers. However, philosophy formulates the questions which allow us to determine what is not cognizable. These are questions of the following kind: Is there an alternative physics which we never cognize because of the particular qualities of our mind and body?

The 1905 lecture was entitled "On wireless telegraphy and new researches in physics". This lecture is less philosophical than the one delivered in 1899. Braun showed an extensive retrospective of the development of radio—he mentioned Guglielmo Marconi, Marconi's company and his own work. At the end of the lecture, he came to two philosophical conclusions. The first concerns the unity of fundamental and of applied research (see the discussion in the previous section), and the second was also partially cited and read as follows: New ways of research cannot be designed a priori. As always, the fundamentally new problems presuppose fundamentally new methods. Atoms do not represent any ultimate reality or real $\alpha\tau$ oμos—although this is a firm belief. Apparently, there is no point in moving along beaten tracks. "With bolts and levers we have no chance to penetrate into nature. Permanent and tenacious efforts—this is what could be helpful instead. Lucky discovery of a new relationship will bring us the right understanding" [61, p. 22].

As early as in 1899, Braun cautioned against *Naturphilosophie* and spoke about the relativity of all natural laws. In his 1905 lecture, the tenor of empirical criticism resonates more strongly. Braun analyzed the concept of homogeneity and showed this concept's relativity with respect to the experimental devices and correspondingly to the problems which a researcher sets before him/her. He proposes the following view. There are a number of stone poles which are lined up with an interval comparable with their sizes. A train of electrical waves falls down on this line of stone poles. If the wavelength is the order of the width of stone poles, then the waves behave themselves like the waves of a river which meet the piers of a bridge. The waves partially pass through the obstacle and partially fall back. If the wavelength surpasses the width of the stone poles, the view is different. Now by their own size, the waves do not perceive the small gaps in the line of stone poles, and this line behaves itself as a continuous wall which uninterruptedly and steadily fills up the space, and it behaves itself as a homogeneous body [59, p. 14].

In terms of electric waves of 70-cm wavelength, a conglomerate consisting of bricks and gaps between them is a homogeneous body. To our sense organs, this conglomerate appears different. This relativity of physical schemes and models with respect to experimental devices became a favorite topic in the lectures of Mandelstam and the philosophical essays of his disciples.

Having discussed the homogeneity of solid bodies, Braun turned his attention to the smallest particles. Having witnessed the splitting of the traditional atom, he dedicated himself to the mystery of radium, the element discovered in the process of research of uranium. However, Braun was a scrupulous experimenter and strongly advised to distinguish between fact and fiction. "We don't know this substance well enough and on closer examination startling phenomena turned out to be unprepossessing. The method of research of radioactivity involves an observation of the velocity with which the detecting foil (electroscope's plate) falls. This is our dry residue" [59, p. 21].

Here, Braun is referring to Ernest Rutherford's famous experiments concerning the deviation of α-rays in electric and magnetic fields (1902). Rutherford's rays, emitted from a layer of radium salt, were passed through a very thin aluminum plate and fell into a chamber where they ionized the hydrogen contained in it. The ionization was measured by the rate of the fall of the plate from the foil in the electroscope. If a horizontal magnetic field is created, the α-rays were deflected toward the metal plates and absorbed by them. As a result, the smaller amount of particles comes to the chamber, the smaller ionization they provide, and the foil in the electroscope falls more slowly.

The final section discusses Braun's relation to Ernst Mach (1838–1916). Although Braun allegedly was not a Machist, he sympathized and kept up correspondence with Mach since 1894 (see [159]). Braun joined the number of physicists and scientists who, between 1910 and 1914, nominated Ernst Mach for the Nobel Prize in physics.[10] Braun's nomination letter indicated that as the Nobel Prize might soon be awarded for the new theory of space and time, it should first be given to Mach, an early Advocate of these ideas and leading experimental Physicist. Braun also insisted on Mach's wider influence via his "philosophical explications" and "his clear, profound historical–physical studies" (the letters to the Nobel Prize Committee on behalf of Mach from Braun and other prominent physicists are published in [48]). In his letter, Braun wrote that Mach proposed a "strict idea of how our fundamental physical concepts were being formed" and, "from the point of view of the theory of cognition, answered the question of what our definitions of physical concepts meant". As noted

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[10] Among those who sent letters to nominate Mach were also Hendrick Lorentz and Wilhelm Ostwald; see [48, 159].

above, Braun's sympathy for Mach did not mean devotion to Mach's philosophy. However, in German physics, Mach's ideas were becoming commonly held. For example, young Papalexy's synopsis includes a sentence which is basically a quotation from Mach's *Mechanics*—this is about Galileo Galilei who redirected the study of free fall from the question 'why?' to 'how?' and a comment that in physics one should describe empirical facts rather than look for explanations [399, 600-1-12].

In his Nobel Lecture, Braun spoke about his contribution to radiophysics and radio-engineering, and his achievements for which he had received the Nobel Prize. He did not touch specifically upon fundamental philosophical problems. However, philosophy was present in his Nobel Lecture, and quite rightly so, because Braun took into consideration an extensive retrospective of the development of physics. This was the philosophy of the unity of electrical and optical phenomena taken as oscillatory phenomena. Having described his achievements in directional radiotelegraphy where the molecular oscillators excited by the antenna's radiation are essential, Braun [64, p. 241] said the following:

> If nowadays optical phenomena are ascribed to electrical molecular resonators, then electrical processes, as demonstrated here by a single example, can also be linked up with optical phenomena, though this can hardly be experimentally verified in this field. Here, the study of electrical oscillations supplements that of optical oscillations, and since we are in a position to tackle a problem in either field by analogy with a phenomenon which is comprehended in the other field, the first attack on the problem can be made from the electrical or the optical standpoint according to whichever presents the easier concept to realize.

The oscillatory unification of different fields of physics was practically realized at the institute headed by Braun. In this connection, Nikolay Papalexy writes in his biography of Mandelstam: The atmosphere of electromagnetic oscillations, in which Leonid Isaakovich found himself by entering into scientific life, has played the great part in formatting the basic lines of his scientific activity and has determined the "oscillatory approach" which was significant for his creative work [1, Vol. 1, p. 14]. Zenneck, who was Braun's First Assistant at the Institute of Physics when Mandelstam started at this institute, declared something along similar lines. From Zenneck's *Recollections*, it follows that Rayleigh's *The Theory of Sound* (1891) was popular at Braun's institute [391, p. 102]. In spite of the fact that its title suggests that this is a study of acoustics, it is a fundamental source on the theory of oscillations. Papalexy emphasized that *The Theory of Sound* was of seminal importance in Mandelstam's education. Mandelstam biographers correlate his "oscillatory approach" elaborated in Strasbourg with his Moscow ideology of oscillations. "As early as in Strasbourg Mandelstam scrutinized closely the classical theory of oscillations. The main source was Rayleigh's two-volume book *The Theory of Sound* and a large body of his papers. Mandelstam inherited Rayleigh's linear oscillatory culture and he did his best to transmit this culture to subsequent generations [...]. However, Mandelstam was not merely Rayleigh's successor in the field of linear oscillations. Under his guidance a new scientific area emerged and obtained widespread recognition. This area is connected with research into nonlinear oscillations" [1, Vol. 1, p. 40].

It is worth mentioning that Braun's idea about the "oscillatory unification" coincided with the trend peculiar to German science—the trend toward the development of unified technological studies in the area of oscillations [33, 123] and in general toward the theory of oscillations unifying mechanics and the theory of electricity [123, 361].

In 1909 at Strasbourg University, a professor who was not merely Mach's follower appeared. He regarded Mach's life in science as exemplary. This was Richard von Mises (1883–1953), who became Mandelstam's friend, and their friendship would not end when they turned out to be separated by the war in Europe and then the revolution in Russia. As was noted in the Introduction, 39 letters received by Richard von Mises from Mandelstam and his wife became the memorial of this friendship.

2.6 Richard von Mises

When Mandelstam met Richard von Mises, the present author could not find out. Most likely, it happened a short time after Richard von Mises' arrival in Strasbourg (1909). Richard von Mises was called to Strasbourg as Ausserordentlicher Professor (Extraordinarius) of applied mathematics (see his biography [39, 314, 315]). As N.D. Papalexy writes, "in Strasbourg an acquaintance of Leonid Isaakovich and the famous German applied mathematician Richard von Mises happened, the acquaintance turning into friendship. Leonid Isaakovich often conversed with Richard von Mises, who was a brilliant mathematician with sharp mind which enjoyed rigorous logical constructions and fine logical differences. Discussions about the role of axiomatics in the logical foundations of mechanics and exact sciences, in particular, of statistical physics, which is based on the theory of probability, met Leonid Isaakovich's requirement to have complete lucidity of mind. Along with H. Poincaré's ideas represented in his beautiful book "Science and Hypothesis" these conversations helped Leonid Isaakovich here in Moscow to construct the complete and consistent base for statistical physics" [1, Vol. 1, p. 32].

Papalexy seems to have meant the interpretation of quantum mechanics in Mandelstam's 1939 "Lectures".

Richard von Mises was in the humanitarian school in Vienna. He finished this school in 1901. In 1906, he graduated from the Vienna University of Technology (or, which is the same—the Vienna Technical Higher School) and began teaching at the Higher Technical School in Brno. In 1908, he received a Ph.D. in Vienna, presenting a thesis "Investigation of the inertial mass in the slider-crank mechanism".

About what would L.I. Mandelstam and Richard von Mises speak? Richard von Mises was a specialist in the equations of mathematical physics, that is, in the theory into which Mandelstam went deeper in the course of his research in optics. In the article "On the application of integral equations to the theory on optical images" (1912), there is acknowledgment in which the author thanks Richard von Mises for consultations. Mandelstam used the theory of the Fredholm and Hilbert equations in this article. In Moscow in his lectures on the theory of oscillations (1930–1932), Mandelstam presented the theory of integral equations and reproduced the results of his 1912 article.

The second topic in the Mandelstam–Richard von Mises conversation was the foundations of statistical physics. As a matter of fact, Papalexy writes about it in the cited extract from his biography of Mandelstam. Richard von Mises evidently spoke about his empirical (frequency) conception of probability and his attitude to classical probability, which can be traced back to Laplace. Richard von Mises treated probability as the limit of the sequence of relative frequencies, whereas according to the classical treatment probability is the relation of the number of favorable outcomes to the number of the equally possible outcomes. Richard von Mises was already thinking over his conception of probability.[11] He represented it in his 1916 address and published it in 1919 (Mandelstam asked Richard von Mises to send a copy of his article in his 24 September 1921 letter).

Papalexy's passage quoted before evidences that in their Strasbourg conversations Mandelstam and Richard von Mises were concerned with philosophy (to put it more precisely: with the philosophy of science). However, one can only conjecture which philosophical positions were declared by them. Richard von Mises probably followed E. Mach's philosophy. The subsequent chapters will show that Mandelstam was also close to positivistic philosophy, and Mandelstam and Richard von Mises shared a number of philosophical positions (e.g., about the nature of physical concepts).

Which was the following Richard von Mises trajectory? Since 1914 to 1917, Richard von Mises served in the newly formed Flying Corps of the Austro-Hungarian Army. He served as test Pilot and Instructor. In 1918, he came to teach mathematics at the University of Frankfurt am Main. In 1919, he was invited to the Technical University of Dresden. In 1920, Richard von Mises was appointed as Full Professor (Ordentlicher Professor, Ordinarius) of applied mathematics at Berlin University, where he founded and directed its Institute for Applied Mathematics. He also founded and began to edit the "Journal for Applied Mathematics and Mechanics" in 1921.

In 1918, the first book by Richard von Mises "Elements of Technical Hydrodynamics" was published. Richard von Mises started to prepare this book as early as in Strasbourg. In the same year, he published the textbook "Fluglehre". Its Russian translation was published under the title "The basic principles of aircraft" in Moscow in 1926 [367].

Richard von Mises and his friend Philipp Frank wrote an influential two-part work on the differential and integral equations of mechanics and physics, the Frank-Mises of 1925 and 1927, with its second edition following in 1930 and 1935. This work originated in two volumes, the Riemann-Weber of 1900 and 1901 (1910 and correspondingly 1912) the second edition 1910 and 1912 respectively. Riemann-Weber arose as a result of an extensive revision of Bernard Riemann's lectures on partial differential equations [313].

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[11] Richard von Mises wrote in the Foreword to his book "Probability, statistics and truth" that he started to elaborate his approach to probability as early as his Strasbourg years [367].

Richard von Mises sent his two-volume book to Mandelstam. In his 09.02.1928 letter to Richard von Mises, Mandelstam acknowledged the receipt of the second volume, called this book “Riemann-Weber”, and wrote that the first volume had become for him and his collaborators a “handbook”.

The Russian translation of the second edition of the Riemann-Weber was published in 1937.

As was noted, in his philosophy of science Richard von Mises firstly followed Ernst Mach. But Richard von Mises’ philosophical horizon was broad. He studied philosophical writings of H. Poincaré, H. Hemholtz, H. Hertz, he was engaged with the discussions of Frank Brentano and Edmund Husserl, and he read and discussed the early works of G. Frege and B. Russell (see the introductory article in [366]).

Richard von Mises’ deepening interest in the poet Rainer Maria Rilke and his conversion to Catholicism date from his days in Strasbourg. He composed the famous collection of Rilke’s writings.

In 1928, Richard von Mises succeeded to publish the book “Probability, statistics and truth”. This book has already been mentioned. It is interesting that its Russian translation appeared as early as 1930, whereas its English translation was published in 1939 and this was a translation of the second edition.

L.I. Mandelstam’s 27.4.1929 letter to Richard von Mises sheds light on this quick development. Mandelstam wrote:

> I should like to say a couple of words about the translation of your book. I spoke about it with Prof. Kogan, who is the head of the scientific department at State Publisher. He is going to write you, or probably he has already written. You will probably understand by reading his letter why I don’t enlarge upon it. As far as it is concerned with me, I am eager to read your book. I consider that it is very desirable to translate your book in Russian.

About what did Mandelstam not write in his letter to Richard von Mises? It is not difficult to catch a hint of the answer by looking at Russian version of Richard von Mises’ book. The title is “Probability and statistics”. In the editorial Foreword, the author’s philosophical position is put under criticism. Richard von Mises was a Machist, but Machism was disavowed by Lenin in his book “Materialism and Empirio-criticism (Critical Comments on a Reactionary Philosophy)”. Lenin’s book was above any criticism.[12] In 1938, Richard von Mises published his main philosophical book “A small textbook of positivism”, which really was an extensive book describing not only the positivistic approach to science, but also the positivistic interpretation of ethics and esthetics. The publication of this book was timed for the Ernst Mach centenary.

In 1938, Richard von Mises was Professor at Istanbul University. In 1939, he left Turkey for the USA where he accepted the junior position at Harvard University. In 1945, he became Professor of aeromechanics and applied mathematics at Harvard University In 1951, he published an English version of his “A small textbook of positivism”.

It is entitled “Positivism. A study in human understanding”.

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[12] However, Richard von Mises’ philosophical lecture about modern physics [365, 366] got a Russian-speaking reader in 1924.

Together with Richard von Mises, his former student and collaborator Dr. Hilda Geiringer arrived in USA (in 1934, she joined Richard von Mises in Turkey). In 1944, they married. Previously, Richard von Mises was not married. Hilda Geiringer' first husband was Felix Polachek, Mathematician.

The above information will be helpful when the reader reads the quotations from Mandelstam's and his wife's letters to Richard von Mises published in Chaps. 5 and 6.

Chapter 3
The Strasbourg Period: Radio-engineering

3.1 Radio-engineering at Strasbourg University

In Strasbourg, Mandelstam conducted research in radio-engineering and optics. By working in radio-engineering, he followed F. Braun and he was involved in the mainstream of research which was conducted at Strasbourg Institute of Physics, the institute which was headed by F. Braun. As was noted above with reference to the biography of F. Braun written by F. Haas, since 1901 all the dissertations supervised by Braun had been dedicated to wireless telegraphy.

Let me cite from Strasbourg University newspaper (Straßburger Akademische Mitteilungen für die Studierenden Der Keiser-Wilhelms-Universtät in Straßburg. Winter term 1909/1910. No. 4. Wednesday, 15 December 1909):

> The first experiments in the field of wireless telegraphy were conducted by F. Braun in Strasbourg in 1898. That year researches on telegraphy through water were completed. These experiments were conducted in the old defensive ditches which have been scheduled for backfilling. The first Braun patent was received in 1898. Next year in summer in Cuxhaven the experiments were conducted by Prof. Dr. Kantor and Prof. Dr. Zenneck. On 1900 November Braun reported about his research for the first time… In 1901 in summer in Strasbourg fortifications he conducted experiments with the help of Captain von Siegfeld, who was known by his scientific aeronautics and got into trouble later. Next year with the help of Dr. Mandelstam and Dr. Brandes Braun conducted the successful radio-transmission from the fort. In the same place he conducted the initial experiment of a directional receiving. In 1905 Dr. Papalexy took part in research in the directional telegraphy.

Till 1905 in Strasbourg in radio research, not only Ferdinand Braun, but also his First Assistant J. Zenneck played a decisive role (in 1905, Zenneck became Professor in Danzig and then in Braunschweig). In his monograph and textbooks (they were mentioned in the previous chapter, Sect. 2.2), Zenneck had systematized his Strasbourg colleagues' achievements and compared them with the world level.

As already mentioned, in Strasbourg, Mandelstam, working in radio-engineering, began to cooperate with N.D. Papalexy, and their cooperation has been continuing throughout his life. Reminiscing about his first collaboration with L.I. Mandelstam, N.D. Papalexy with a share of nostalgia writes about the epoch of "stridulous spark",

A. Pechenkin, *L.I. Mandelstam and His School in Physics*,
https://doi.org/10.1007/978-3-030-17685-3_3

about the time "when the discharge of a capacitor was a generator of oscillations" [267, p. 376]. The theme of the oscillatory discharge of a capacitor was in the center of L.I. Mandelstam's first research and his degree work, or, as it was called in German universities, dissertation.

Is it worth to describe Mandelstam's early research? Since that time, radio-engineering passed through a revolution or rather through a number of revolutions. Is not mere applied science, but it formulates its own volitions [239]. Let us take these Mandelstam's early papers as elegant etudes. Besides, starting with these early papers one can trace back Mandelstam's mature research (Central Radio Laboratory, Moscow State University, the Academy of Sciences) and the examples in his lectures delivered in Moscow State University.

3.2 Mandelstam's Degree Work

This research was dedicated to the development of an indirect method of frequency measurements (to put it more precisely, of measurements of the period of oscillatory capacitor discharge). According to conventional terminology, indirect measurement is a measurement in which the value of a magnitude is found on the base of the law which connects this magnitude with another magnitude that could be directly measured. A direct measurement is a measurement in which the value of magnitude is directly found from sense data (see [32, p. 4, 88, p. 6]).

The direct measurement of the period of the oscillatory discharge of the capacitor is possible with the use of the Braun tube. A bright spot is forced to oscillate with the help of the coil, with a streamlined alternating current which is under investigation. By means of a rotating mirror, these oscillations are transformed into a curve.

However, such a measurement is suitable for relatively small frequencies.

F. Braun put before L.I. Mandelstam a problem to elaborate an indirect method which would differ from those which were already in application. As Mandelstam wrote,

> Professor Braun proposed me to follow the principle: an oscillating current divides into two branches, one of which consists of a self-inductance and one wire of the differential thermometer, and another consists of a non-inductive resistance and the second wire of the thermometer.
>
> It is necessary to find an ohmic resistance, in which the two wires give the same heat. This would mean that the ohmic resistance is equal to the apparent inductive resistance, from here under a given self-inductance frequency is determined. [1, Vol. 1, p. 69]

This problem can be elucidated as follows. Let us take a parallel connection of two conductors. One of them is the inductance which equals L, and the other is the ohmic resistance which equals R. Let the signal having the frequency ω be going along this connection. Then, the impedance of the first branch equals $i\omega L$, and the impedance of the second branch equals R. We need to choose such a value of R

as would provide in both branches the currents with the same amplitude. The heat effect is used here: By changing R, we obtain equal heat on both branches. Then, the formula $\frac{R}{L}$ gives the frequency.

Mandelstam, however, formulated the problem in another way: He elaborated the method of frequency measurement for "such discharges of a condenser which do not give simple sinusoidal current". He used such a shunt chain which joining with the main chain does not change the period of oscillations in the latter. A shunt circuit has two branches: The first contains an ohmic resistance, and the other has a self-inductance. As a matter of fact, measurement is provided by means of this shunt circuit.

Mandelstam used the differential equation technique. The differential thermometer shows a temperature difference and allows us to compare the heat effects in the branches of the shunt circuit. The following formula expresses an equality of heat effects: $\int (i_1^2 - i_2^2)\mathrm{d}t = 0$, where i_1 and i_2 are the currents in the two branches.

The solution of this equation (with the help of approximations and simplifications) leads to formulas which provide the calculation of the period of oscillations in the main chain by using its parameters.

In his book "Electromagnetic oscillations and radio", J. Zenneck discussed the methods of measurement of the oscillatory discharge of the condenser. Zenneck compares two indirect methods (see Fig. 3.1). Firstly, Zenneck describes a method which is alternative to the above which has been worked out by Mandelstam (Fig. 3.1, on the left): The capacitor circuit is inductively coupled with the circuit containing a thermoelectric wire of the thermometer. This second circuit must be so removed from the first, so as not to cause an impact on it. Then, a sharp rise in temperature, fixed by the thermometer, shows a resonance: the proximity of the frequencies of oscillations in the condenser circuit, with respect to which the measurement is produced, and the measuring circuit. Clearly, the measuring circuit must be prescaled; i.e., its eigenfrequencies (natural frequencies) must be known.

Although the resonance method has for a long time been used in radio measurement, it suffered from a significant drawback: It is still not possible to avoid a disturbance of the capacitor circuit from the side of the measuring circuit. The method developed by L.I. Mandelstam minimized this disturbance if the resistance and inductance of the measuring circuit are quite high.

It is interesting that J. Zenneck mentioned L.I. Mandelstam along with a number of physicists who worked in the same direction (the most famous is E. Rutherford, who published his method in 1897 in "Transactions"). Mandelstam's method is one of a number.[1]

In Zenneck's subsequent books [e.g., in his textbook on wireless telegraphy ("Leitfaden")], L.I. Mandelstam's method was not mentioned. Radio measurements have evolved rapidly.

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[1] Compare with the enthusiastic estimation of Mandelstam's degree work in the Soviet 1951 book [349, p. 196].

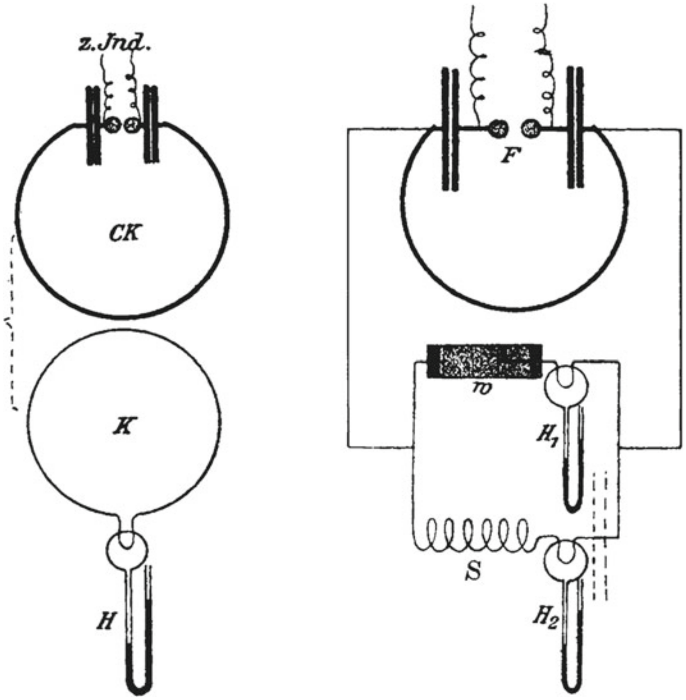

Fig. 3.1 From Zenneck's book "Electromagnetic oscillations". Two ways of how to measure the oscillatory discharge of a condenser. *On the right*: *CK* is a condenser circuit which induces e.m.f. in the closed circuit *K*, equipped by the measuring instrument *H*. *On the left*: the current of the discharge goes along two branches. The upper branch has a considerable electrolytic resistance *w*, and the lower branch has a conductor with the inductance *S*. Both upper and lower branches are equipped by their measuring instruments. The instruments H_1 and H_2 should be identical

3.3 Experiments with Loose Coupling

In Sect. 3.1, Strasbourg University newspaper was cited. The experiments conducted by Mandelstam and Brandes were mentioned there. These were the experiments with loose coupling between primary and secondary circuits in the Braun transmitter and receiver. F. Braun himself wrote about the Mandelstam–Brandes experiments in the printed version of his lecture "Wireless telegraphy and the problems of physical cognition" (1905) [62], and he mentioned them in his Nobel Lecture. About these experiments J. Zenneck wrote in his book "Electromagnetic oscillations…", which was mentioned above [386, p. 986]. In the biography written by N.D. Papalexy, it is said that Mandelstam together with Brandes invented the loose coupling [1, Vol. 1, p. 51]. Zenneck wrote more gingerly: The phenomenon of loose coupling was "theoretically described by Max Wien and almost at the same time experimentally invented by Mandelstam and Brandes" [381, p. 387, a comment in the footnote].[2] Everybody points to the applied importance of Mandelstam–Brandes' experiments that led to the improvement of reception and to the rise of the receiver selectivity.

Let me cite F. Braun's Nobel Lecture [63, p. 234]:

> In the summer of 1902 I was able to erect two experimental stations on two forts at Strasbourg for the purpose of closer study. The task which I had set for us was to determine the most favorable conditions in the receiver. We adopted the resonant circuit, in which known capacitances were combined with calculated self-inductances, so as to bring both parts of the transmitter system into the same natural frequency of oscillation. We fixed likewise the two oscillations arising from the coupling and searched for these with the receiver. The result of the test was, for that time, surprising, as an example will show. If, by means of a coil in the receiver circuit, the oscillations were transferred inductively into a second coil located in a tuned circuit containing the indicator (parallel to a small capacitor), not only was the sharpness of the resonance but also—and here was the surprise—the *intensity* of the excitation was raised as soon as the two coils were *moved away* from one another. The intensity increased with increasing distance between the coils, though naturally beyond a certain limit there was again a decrease. Described in the customary expression, the effectiveness increased with *looser* coupling. This result in the receiver was *not* subject to a similar loose coupling in the transmitter.

What is this "loose coupling"? This is a coupling between an antenna and a closed circuit in the Braun transmitter and in his receiver. By leaving aside the strict definitions which Mandelstam formulates in his 1930–32 lectures, it is possible to provide the following picture: The force of coupling is proportional to the coefficient of mutual inductance of coils coupling the primary and secondary circuits, and it is inversely proportional to the product of the inductances of these coils. In the case of a strong coupling of primary and secondary circuits, their system works as a single whole. The primary system acts on the secondary one, and the secondary one acts on the primary one (as a mechanical model can be taken the oscillations of two pendulums connected by a spring). In this case by altering the parameters of the system, it is possible to reach an increase of the amplitude in the secondary circuit. The strong coupling allows us to set up the situation where the amplitude of voltage oscillations across the capacitor plates would exceed the amplitude of the oscillations of the spark voltage. "By increasing the capacity of the primary circuit, Max Wien writes, and by decreasing its self-inductance, it is possible to considerably increase the ratio of amplitudes V_1/V (V_1 is the voltage across the condenser plates of the secondary circuit, V is the spark voltage)" [382, p. 46] (a Russian translation is cited). However, the damping of oscillations in the system consisting of strongly coupled circuits is more intensive than in each circuit taken separately.

In the case of the loose coupling, the back action of the secondary circuit on the primary circuit is small. It allows us to reach a weak damping of forced oscillations in the secondary circuit. However, the amplitude of these oscillations will be less than in the case of the strong coupling.

Note that, according to Zenneck, L. Mandelstam and Brandes experimentally and Wien theoretically opened namely the loose coupling. The case of an infinitely loose

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[2] For M. Wien's papers, see [381, 382].

coupling was described by V. Bjerknes as far back as in 1895 (see [47]).[3] However, Mandelstam and Brandes applied this idea in radio-engineering. They showed that the quality of reception increased in the case of an infinitely loose coupling when "two oscillations approximate the natural frequency, that is to say, become equal to each other" [61, p. 231]. In this case, the secondary system demonstrates two kinds of oscillations: the forced oscillations corresponding to the oscillations of the primary system and oscillations which are close to its (secondary system's) natural oscillations. If the frequencies of the primary and secondary systems approach each other (the systems are tuned in on each other), one can reach an almost full energy transfer from the primary circuit to the secondary circuit and the maximal amplitude of oscillations in the secondary system. In his 1931 lectures, Mandelstam mentioned that in the case of loose coupling one can reach "strong coherence".

For a certain time, a receiver with an infinitely loose coupling became the "calling card" of Telefunken Company which was founded with the participation of Ferdinand Braun (see, e.g., "The specifics and novelty of the schemes and constructions of the "Telefunken" system").

The Braun transmitter allows one to regulate the coupling force. If one needs the high transmission range and hence a high power of the transmission, one should prefer the strong coupling. If the problem of noise immunity arises, a loose coupling between the primary circuit and the antenna is preferable.

3.4 The F. Braun Energy Scheme

L.I. Mandelstam participated in Braun's two-circuit scheme which was mentioned in Chap. 2 (Sect. 2.3) and in the present chapter (Sect. 3.1). Together with his teacher, he considered the problem of how to increase the transmission range and hence the power of the transmitter. The F. Braun energy scheme (1902) appeared as a result of this research. This scheme became a noticeable event in the history of radio-engineering.

Mandelstam did not publish any paper on this theme. His participation in this research is only captured in Papalexy's recollections and in a patent which was taken by him and Papalexy due to their development of an apparatus theoretically connected with the Braun scheme. There is more evidence. Siemens Company Archives keep F. Braun's letter to W. von Siemens. In this letter, Braun recommends Mandelstam as a talented young engineer who participated in the development of his two-circuit scheme [400, LK 205] (the 1903 letter).

N.D. Papalexy provides the following description of the problem which F. Braun formulated [267]. The theory of electricity (the energy stored in a condenser equals $\frac{1}{2}CU^2$, where C is the capacity and U the voltage) points to two ways of how to

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[3] Vilhelm Bjerknes (1862–1951) was Assistant to H. Hertz in 1890–91. The concept of the extremely loose coupling he mainly developed in 1895–1898. He became a famous earth physicist and meteorologist later.

increase the transmitter power: (1) to increase the capacity of the condenser and (2) to increase the charging voltage. As F. Braun explained, both methods are vulnerable. If we increase the capacity of the condensers, we decrease the self-inductance. However, the coupling of the whole system decreases with the reduction in the self-induction of the oscillating circuit (containing a spark gap) and the secondary circuit (containing an antenna). Besides, practical difficulties arise: An increase of capacity of condensers leads to an increase of their self-inductance of the condensers originating in the coating of the jars themselves but especially in the connections.

The increase of the charging voltage (even if this is possible from an engineering point of view) leads to an increase of the length of the spark and hence to the considerable energy loss. Here, difficulties with insulation arise, too.

> It was tried out many different schemes, partly suggested by L.I. Mandelstam, writes Papalexy. About these unsuccessful attempts, Leonid Isaakovich shared with me and our mutual friend Brandes at meetings over lunch. Have been struggling with this problem for a week and getting no result, Prof. Braun and Mandelstam left the laboratory in the reduced mood. The next day (Sunday) for lunch, Leonid Isaakovich told us that he was from yesterday all the time thinking about this problem and apparently solved it. In his characteristic caution and modesty, Leonid Isaakovich put it this way: "I seem to have found a way to solve, but I fear that Professor Braun has also come to this solution". Indeed, when the next morning, Mandelstam entered the laboratory of Professor Braun, then he met this exclamation: "Herr Dr., Wissen Sie, ich habe gekriegt!" Thus arose the famous "Braun energy scheme", the essence of which lies in the ingenious use of a parallel charging of the capacitors appearing in n oscillatory circuits, and sequential discharging them through the self-inductances of these circuits, which along with the capacitors constitute a serial (main) circuit of the transmitter. [267, pp. 376–377]

By following F. Braun's article in the journal "Electrician" [61], there were two stages in the development of the original scheme. The first result was reached in the same 1898, when Braun created his inductive coupling system, alternative to Marconi's scheme. Braun's reflections had led him to the arrangement of Fig. 3.2. Three condenser circuits of exactly the same frequency are connected in series. "The transmitter passes through them all. As shown by the polarity signs (±), the condensers were charged in series. The energy of n capacities C is $\frac{1}{2}(C/n)(nV)^2 = nCV^2/2$. Each spark, however, has only a damping corresponding to the part V of the *P.D*". [Ibidem, p. 20].

Figure 3.2 represents the Braun scheme consisting of three capacitor circuits. Figure 3.3 shows how the Braun transmitter works (three capacitor circuits, taken together, are inductively connected with the antenna).

Fig. 3.2 From Braun's article in the journal "Electrician". Two series connections of the three condenser circuits

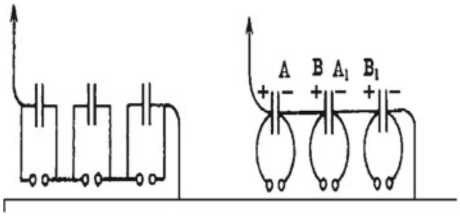


Fig. 3.3 Three capacitor circuits, taken together, are inductively connected with the antenna

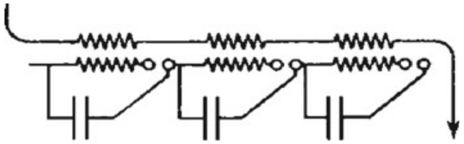
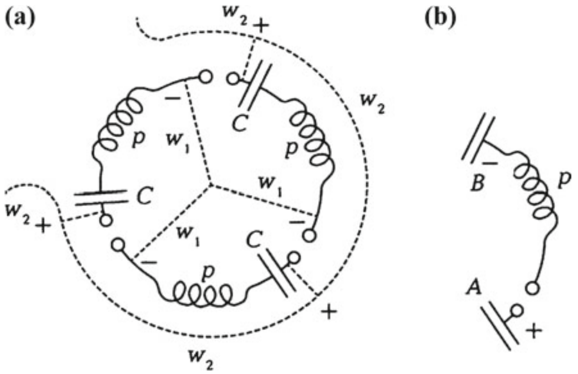


Fig. 3.4 Braun energy scheme. *On the right*: the partial system. From Braun's article in "Electrician"



The problem of synchronism was left. "The difficulty was to so couple circuits of this kind together that they would all start to discharge at the same moment, for example within exactly 1/10 of a millionth of a second", Braun said in his Nobel Lecture. "This task occupied me on repeated occasions". In 1902 autumn, Braun started to solve this problem by recruiting the newly made Doctor Leonid Mandelstam.

As a result of this work, the Braun energy scheme arose, the scheme about which Papalexy said: three oscillatory circuits, arranged in a circle (Fig. 3.4). "I reverted to these arrangements in the autumn of 1902,—F. Braun writes,—after having developed methods which allowed the phase-difference between rapid oscillations to be detected and measured without in influencing in a disturbing manner the system to be tested. The outcome of these experiments was that the oscillatory circuit were coupled more closely; such a coupling, which assists the simultaneous jumping of the spark, exists already in the isochronous oscillations of the transmitter, which passes through the entire system" [61, p. 20].

Braun's energy scheme maintained the advantages which were reached by him in 1898. However, the energy scheme showed synchronism. This system had symmetry: If the parameters of the circuits were equal to each other, all three circuits acted identically.

> The condensers are charged in parallel, and are connected for this purpose with non-inductive or inductive resistances ω_1 and ω_2. As soon as one spark occurs the whole circuit closes within itself ....
>
> The considerations from which this arrangement has been developed are very simple and clear. Assuming, for reasons of simplification, that all capacities C and all self-inductance are equal, n capacities then contain a total energy $nCV^2/2$, in which V equals a charging voltage. The period of an oscillation, however, depends upon the product Cp, because the

> capacities when discharging are in series, all have capacity of *C/n*. They have capacity of *C/n* only, when self-inductance is then represented by *np*. Therefore a period of oscillations is the same when one condenser is closed by one self-inductance *p*. [61, p. 20]

For the F. Braun *energy scheme*, a description was possible that was not valid for the 1898 system of circuits arranged in a line. This scheme could be treated as a system consisting of three circuits each of which contains a capacity, an inductance, and a spark gap. It could also be treated as a set of the "partial systems" (Fig. 3.4, on the right). By a "partial system", Braun meant the circuit consisting of the following elements: a capacitor plate, an inductance, a spark gap, and a next capacitor plate. The "partial systems" had the same electrical characteristics as the circuits composing the energy scheme.

As was noted, the Mandelstam–Papalexy 1913 patent application arose as an extension of the Braun–Mandelstam research. Mandelstam and Papalexy proposed an improvement of Braun's energy scheme. They struggled against the energy loss which resulted from the spark. Mandelstam and Papalexy used Max Wien's conception of the transmitter (it was mentioned in Chap. 2, Sect. 2.3). The Wien transmitter is close to the Braun scheme. However, a very short spark (a very small discharge gap) is used in it. By having excited oscillations in the secondary circuit (antenna), the spark dies away. Thus, the primary circuit turns out to be disconnected and there is no energy loss in it.[4]

In their 1913 scheme, Mandelstam and Papalexy combined two ideas: Winn's idea about the disconnection of the primary circuit and the F. Braun energy scheme. However, this disconnection is provided by a special construction of the transmitter and its parameters.

During their Strasbourg years, Mandelstam and Papalexy received 15 patents in total [399, 600-2-3]. The above scheme shows the idea of one of them.

3.5 On the Theory of F. Braun's Transmitter. Coupling and Coherency

The first sentence in the title of the present section reproduces the title of L.I. Mandelstam's article published in 1904.

In this article, Mandelstam proposes the unified theory of different modifications of the Braun transmitter. In Chap. 2, a version of the Braun transmitter was described (Fig. 3.5, on the left) within the framework of the comparison of this system with

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[4] L.I. Mandelstam mentioned the principle of the Wien transmitter in his lectures on the theory of oscillations [1, vol. 4, p. 279; 2, p. 256]. "Wien invented a very small spark gap, Mandelstam said. When the breakdown happens in it, it is becoming a conductor. However, under certain conditions the spark gap is losing its conductivity if the current is small. Then, the first circuit is automatically disconnected, when the amplitude of its oscillations is becoming very small, and back energy transfer does not take place".

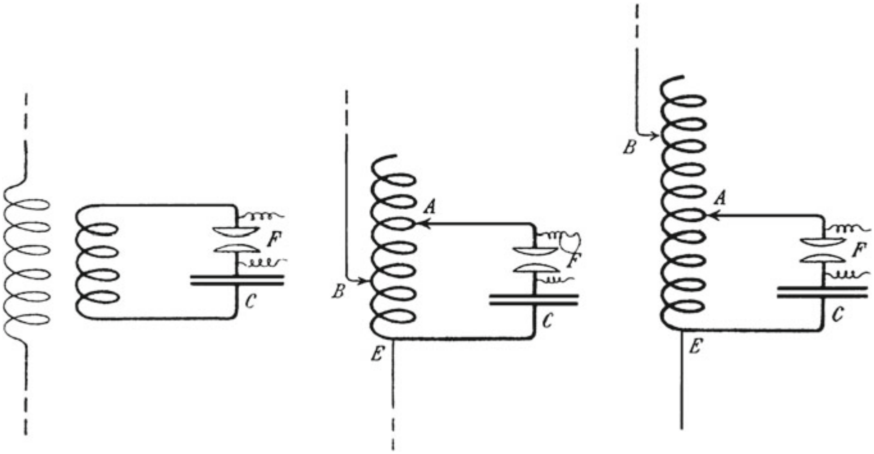

Fig. 3.5 Three modifications of the Braun transmitter. From Zenneck's book "Wireless telegraphy". *On the right* and *in the centrum*: the primary and secondary circuits are connected by autotransformer coupling. *On the left*: the primary and secondary circuits are connected by the inductive coupling

the Marconi scheme. The primary and secondary circuits are inductively connected with each other through a couple of coils (and each coil belongs to its own circuit).

However, besides the inductive coupling F. Braun considered the direct one: The secondary circuit is directly attached to the coil belonging to the primary circuit (Fig. 3.5, two schemes on the right). As a result, the coil turned out to be divided into two parts: the upper which is connected with an antenna (overhead wire, as people called it at that time) and the lower part which is grounded. As the secondary circuit, one should regard the combination consisting of antenna, grounding wire, and that part of the coil which is located lower than the point where the antenna is attached.

The direct coupling is also called the autotransformer. An autotransformer is a transformer in which the primary winding is a continuation of the secondary one.

Max Wien proposed the first mathematical theory of the Braun transmitter, by describing the case of inductive coupling (in 1902—M. Wien's article has already been mentioned in connection with the loose coupling). J. Zenneck, however, pointed out that between the inductive and direct coupling there is no fundamental difference. Mandelstam confirmed Zenneck's conclusion by presenting the mathematical structure of the system (he considered strong coupling only). Mandelstam presented an antenna (an open circuit) as a sequence of m closed circuits, including a circuit, which is directly inductively coupled with the primary closed circuit, and proposed an appropriate system of differential equations. Unlike M. Wien, who considered the Braun transmitter as a system with two degrees of freedom, Mandelstam in the spirit of mathematical physics presented the antenna as a system with many degrees of freedom. If $m = \infty$, the inductance and capacity are continuously distributed along the series of closed circuits. He further showed that the effect of continuity is reached at great but finite m. He also showed that the system with autotransformer coupling is described by the same system of differential equations as the system with inductive coupling. His conclusion on the theory of the Braun transmitter was the following: "the system with autotransformer coupling is reduced to the system with inductive coupling" [1, Vol. 1, p. 98].

J. Zenneck, quoting Mandelstam's article about the Braun transmitter, emphasizes one of the conclusions drawn in it [386, p. 880]. The point here is yet another modification of the Braun transmitter. In Fig. 3.5 is shown the structure in which the antenna is trivially grounded. F. Braun, however, examined a replacement of the so-called ground wire by a "symmetrization", a "counterbalance" (making it a mobile transmitter—it can be placed on a trolley). Instead of grounding, the antenna was attached to the bottom of the iron lattice, oriented parallel to the ground. Mandelstam showed that "grounding is not equivalent to the inclusion of "symmetrizating wire", but rather corresponds to a strong coupling between the primary and secondary circuits" (ibid.).

In his book, J. Zenneck proceeded from the usual definition of the coupling strength. He calculated the coefficient of coupling strength as follows: The numerator equals the square of the mutual inductance of coils, and the denominator equals the product of the inductances of the primary and secondary circuits. When lengthening the antenna, the numerator is not changing, and the denominator increases, since the inductance of the antenna increases. Introduction of the symmetrizating wire means that the antenna is becoming longer. This results in an increase of its inductance and therefore in a reduction in the coupling strength.

L.I. Mandelstam, however, conducted a more sophisticated analysis. As noted above, he started out from the system of differential equations, one of which represented the primary circuit, the rest represented the elements. In analyzing the system of equations and resorting to a number of assumptions, L.I. Mandelstam derived a relation that allows one to calculate the frequency:

$$\frac{1}{L_1 n} - C_1 n = \frac{L_2}{L_1}\sqrt{\frac{E}{L}} \cdot \frac{\sin n\sqrt{LEl} \cdot \sin n\sqrt{LEl_1}}{\sin n\sqrt{LE(l_1 + l)}},$$

where C_1 and L_1 are the capacitance and inductance, respectively, the primary circuit, but also L_2 is the inductance of the secondary circuit, E and L are capacitance and inductance per unit length of the antenna, and l and l_1 are lengths of right and left sides, respectively, of the antenna (secondary circuit).

The derived equation has an infinite number of roots representing the fundamental oscillations (harmonics) of a system of two circuits. The practical significance, however, has a range of

$$\frac{\pi}{\sqrt{LEl}} > n > 0,$$

and one of the fundamental oscillations of the system will be lower, and the other will be higher than the lowest and highest, respectively, eigenoscillations of both circuits taken separately. Strengthening the coupling between the primary circuit and the antenna corresponds to the convergence of the frequencies of the "normal" oscillations. If the antenna included “symmetrizating wire”, the maximum coupling strength occurred in the “symmetrical excitation”. This means that the circuit inductively coupled to the primary circuit is located right in the middle antenna length (as a result, the lengths of the air and symmetrization parts are equal). However, Mandelstam conducted a calculation which showed that the grounding of the antenna leads to an even stronger connection between it and the primary circuit.

It is interesting that L.I. Mandelstam came back to the problem of coupling in his 1930–1932 “Lectures on oscillations” delivered at Moscow State University [2].

L.I. Mandelstam distinguished between coupling and coherency of two connected systems (he called them “partial systems” by referring to a couple of connected systems each of which can be “fastened”). The adequate treatment of the phenomenon of coherency requires delving into the mathematics of the theory of oscillations. However, it can be explained by referring to the example of two pendulums connected by a spring (Mandelstam himself constructed such an example in his lectures). If one of the pendulums is fastened, the other can be treated as a partial system which has its own frequency. The coherence of the oscillations of two pendulums depends on the difference between the “partial frequencies”. The coherence is higher if the difference is smaller. In turn, the coupling depends on the strength of the spring. As a matter of fact, strong coherence can be achieved under loose coupling.

In essence, in his early writings on the loose coupling L.I. Mandelstam implicitly introduced the concept of coherency. He described the phenomenon of strong coherence under loose coupling.

Let us turn to the L.I. Mandelstam and N.D. Papalexy achievement which is noted in many publications on radio-engineering (not only in Zenneck’s books and not only in German publications in general).

3.6 A Definite Phase Difference

N.D. Papalexy writes in his biography of Mandelstam: “1904 is the year of our first collaboratory work in the field of oscillations and radio, which continued in Strasbourg, and in Russia until recently. This work was dedicated to a method of obtaining the phase lagging, but identical in shape oscillations, which formed the basis of experiments on the directional wireless telegraphy and radio interference” [1, Vol. 1, p. 9].

A brief comment. N.D. Papalexy writes about the collaborative work carried out in 1904. Mandelstam–Papalexy’s joint paper on the method of obtaining lagging but identical in shape oscillations was not published until 1906. However, already in 1904 F. Braun in formulating his method of the directional radiotelegraphy referred to the result of Mandelstam and Papalexy. He also mentioned this result in his 1905 lecture “On wireless telegraphy and new research in physics”, which was cited above.

We present the problem by following J. Zenneck (we shall not follow his book quoted above, “Electromagnetic oscillations…”, published in 1905, but shall follow his textbook on wireless telegraphy, published in 1908) [387] (for Russian translation [389, p. 390], see also [162, p. 61]).

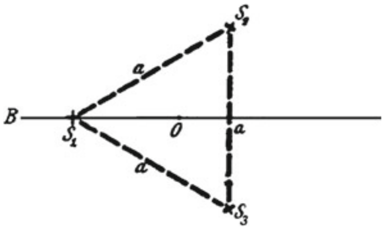

Fig. 3.6 From Zenneck's book "Wireless telegraphy". The F. Braun antenna. View from above

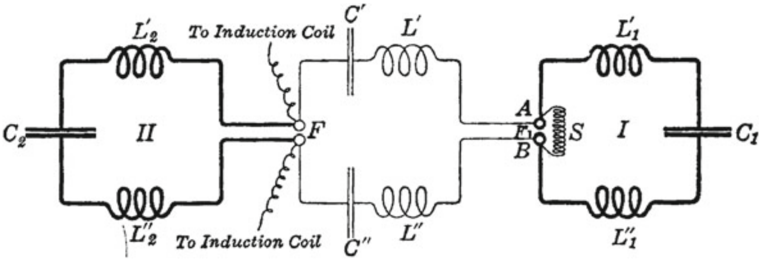

Fig. 3.7 Mandelstam–Papalexy scheme providing the phase lagging, but identical in shape oscillations. From Zenneck's book "Leitfaden der drahtlosen Telegraphie"

Figure 3.6 shows the 1904 Braun arrangement. In the antennas S_2 and S_3 oscillations are in phase, in contrast, in the antenna S_1 oscillations are phase-shifted by 270. The amplitudes in S_1, S_2, S_3 are in the ratio of 1:0.5:0.5; distance $a = \lambda/4$. In this case, the calculation shows that maximum radiation is in the direction of the SO, and in the opposite direction there is no radiation at all. In agreement with this, the experiments provide a particularly strong effect in the direction of the SO, and in the opposite direction the effect does not disappear, but it turns out to be negligible. The main difficulty with such a setup consists in excitation in different antennas of the oscillations with the described phase difference. This difficulty was solved by Mandelstam and Papalexy.

L.I. Mandelstam and N.D. Papalexy proceeded from the known fact that a high self-induction coil locks high-frequency oscillations, but ignores the low frequency. They had constructed the arrangement consisting of three circuits which contain capacities, self-inductances, and spark gaps (Fig. 3.7). The second circuit is connected to the transformer of high frequency. The first circuit provides the result. The third circuit $\underline{FC'L'AL_1'C_1L''BL_1''C_1''F}$ connects the first and second and partially includes the first circuit. The first and second circuits must be identical. The oscillations in them should come with a specified phase difference.

Let the circuits *II* and *I* ($\underline{FL_2'C_2L_2''F}$ and $\underline{F_1L_1'C_1L_1''F_1}$) be tuned up to the same frequency. The wavelength in the circuit $\underline{FC'L'AL_1'C_1L_1''BL''C''F}$ is larger than the wavelength in the circuit $\underline{F_1L_1'C_1L_1''F_1}$.

The coil *S* has a considerable reactance for high frequency, and it provides a short circuit (Kurzschluss) across the capacity C_1, while C_2 is being charged by supplying voltage (of low frequency).

The coil *S* does not influence the processes of high frequency. Spark gap *AB* is adjusted in such a way as to have a spark occurring at a time when the potential difference across the contacts *AB* is at the high (more precisely—when the amplitude of the voltage oscillation is on the high side). When a spark arises in the spark gap *F*, high frequency oscillations appear in the circuit *II* and in the interconnecting circuit $\underline{FC'L'AL'_1C_1L''BL''_1C''_1E}$. Thus as the amplitude of the high voltage reaches its maximum, a spark breaks the spark gap F_1 and the circuit *I* becomes active. This happens after half of an oscillation in the circuit $\underline{FC'L'AL'_1C_1L''BL''_1C_1F}$ (or, equivalently, in a time equal to a half of the period of the circuit).

Why namely "after half of an oscillation"? In their article, L.I. Mandelstam and N.D. Papalexy explained the phase relations in the circuits *I* and *II* by formulating the equations. By retelling their article, J. Zenneck, however, used graphic diagrams. Anyway, an explanation is based on two well-known facts: In the capacity, the phase of the current lags that of the voltage by $\pi/2$, and in the inductance the phase of the voltage lags that of the current by $\pi/2$.

The Mandelstam–Papalexy method was quoted not only by F. Braun and J. Zenneck but also by a person of a competitive community, namely J.A. Fleming who was the closest collaborator of Marconi's and who seldom mentioned the German radio-engineers in his publications [106, 109–111]. As was noted, Mandelstam criticized Fleming by considering his approach to the directional radiotelegraphy. The time has come to explain the world of radio-engineers as it was in the beginning of the twentieth century. Hence, it is worth to have an excursion to the history of radio business.

3.7 Radio Business

Radio in those days was not only a subject of research, but also an area of businesses, and research and business were not independent of each other. On the one hand, the radio immediately became an attractive area of capital investment. Especially, the militaries and shipowners had needs in the development of radio. On the other hand, the scientific development of the radio since 1898 required significant financial resources.

In 1900, a limited company—Professor Braun Telegraph or abbreviated Telebraun—was formed. The principal contribution to the capital of this partnership came from the patent (1898) belonging to Braun. Thus, Braun became a competitor of the company which had been formed around Marconi in England (1897), as well as he was a competitor of German engineers Adolf Slaby and George Count von Arco. Adolf Slaby and George Count von Arco together with electrotechnical company AEG formed the partnership AEG-Slaby-Arco radio in 1898.[5] In 1901, the cooperation of F. Braun with the well-known entrepreneur Siemens resulted in a new company which arose on the base of Telebraun and Siemens and Halske (Siemens & Halske). Its name was "The Prof. Braun and Siemens-Halske partnership of the system of wireless telegraphy" (or for short the "Braun-Siemens partnership"). Telebraun Company, however, continued its activities until 1913, when it was eliminated because of its unprofitability. In 1903 on Kaiser Wilhelm's orders, the two largest companies Braun-Siemens and AEG-Slaby-Arco merged into a powerful company, which became famous under its telegraphic address—Telefunken.

In the book on the history of radio, the situation on the radio market has been summed up as follows: "By 1901 Marconi's Wireless Telegraph Company had already won a leading position in ship to shore communications in Britain, was reaching out for a similar position in the United States, and, with much publicity, was entering the fight for transatlantic traffic in competition with the cables. In Germany there was the Telefunken Company, using the Slaby-Arco-Braun patents and aggressively seeking to establish itself in maritime and European traffic with the strong support of the German Government. In the United States the United Wireless Company, using apparatus based on De Forest patents, operated a network of stations handling ship traffic on the Atlantic and Pacific coasts.[6] And there are other, less formidable rivals" [6, pp. 143–144].

This does not mean that radio-engineering was compartmentalized along "business apartments". There was a unified stream of results in the area of radio. There were international journals, which published both English and German radio-engineers and radiophysics. The major was the English "Electrician" and the German "Jahrbuch der drahtlosen Telegraphie und Telephonie". As noted above, Braun, Zenneck, and Mandelstam entered on the editorial board of "Jahrbuch der drahtlosen Telegraphie und Telephonie". In this editorial board was also included G. Marconi. But the descriptions of the technical devices and inventions have always shown preferences.

Radio industry resulted in the strong competition between the companies. Partially, this competition was represented in the discussions of priority. F. Braun specially proved his priority concerning the two-circuit scheme which he proposed in 1898. In 1900, the scheme which was close to Braun's was put forward by Marconi [60]. F. Braun repeatedly emphasized that his English application for a patent had been ignored (at least up to the date when he secured his first German and equivalent British patent, No. 1862, of January 26, 1899).

The English engineer John Ambrose Fleming (1849–1945) accentuated the secondary role of German inventions. In 1899, Fleming had taken the position of scientific advisor at Marconi's company which was then named *Wireless Telegraph and Signalization Company* [330]. Let us cite a long passage from the first edition of

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[5] AEG—Allgemeine Electricität Gesellschaft.

[6] Lee de Forest was the American radio-engineer and entrepreneur. Having improved the diode invented by Fleming, Forest took out a patent for triode, a three element device. Forest contributed a lot to the development of radio in USA (see, for example, [6]).

Fleming's book "The principles of electric wave telegraphy". Fleming himself called this book his "principal book" [114, p. 175]:

> Braun's suggested direct coupling of an aerial wire a nearly closed oscillation circuit, consisting of a Leyden jar and associated inductance and spark balls, compared with the simple insulated conductor or aerial of Marconi, separated from the earth by a spark gap, does not produce a radiator having any special advantages, unless there is a syntonism between two coupled circuits. Neither is the inductive coupling of any special advantage unless the oscillation transformer is constructed in a particular manner. There is some indication in the opening remarks of Braun's specification, that he considered the real novelty in his invention to be the employment of the oscillations or discharges of a Leyden jar to create electric waves for telegraphic purposes, in place of the oscillations established directly of a simple linear or open circuit radiator containing a spark gap. This conception, however, is seen to have no foundation as soon as we make a metrical study of the phenomena, and the conditions which must be fulfilled for any useful result to take place. There are, in fact, only two modes of coupling an open and closed oscillatory circuit which have any technical value. First, we may couple together the circuits in such a manner that a single pure oscillation or one single period of vibration is forced upon the aerial or radiator, not its own natural period, but that of the actuating closed circuit. Secondly, we may couple together circuits which have the same free natural time period when separate, and thus establish a syntonism between the circuits which, under the condition of somewhat "loose coupling", results in the radiation of waves of two different wave lengths.
>
> The first mode or operation was described by J. S. Stone...,[7] and the second was discovered and worked out practically by Marconi. It has sometimes been suggested that Marconi availed himself of Braun's prior invention, but in truth his (Marconi's) investigations were carried on quite independently, and conducted to a more practical issue than those of Braun. [106, pp. 490–491]

Having described the principle of operation of the Telefunken (Braun-Siemens) transmitter, Fleming stated:

> The comparison... which the German writers and inventors at that date invariably insisted upon making was to take as typical of Marconi's methods of the original single wireaerial transmitter of Marconi, direct connected to one spark ball of the induction coil, the other being earthed. Marconi had advanced far beyond this stage at the end of 1899 and beginning of 1900. [106, p. 500]

Although in the subsequent editions (in total, there were four editions [106, 109–111]) Fleming had excluded this critique, he presented Braun's achievements superficially. Basically, he mentioned his results concerning the directional telegraphy. He even kept this line in third and fourth editions which appeared after the Nobel Prize was given to Ferdinand Braun for his contribution to radio-engineering and radiophysics!

The Papalexy words were above quoted that "the atmosphere of electromagnetic oscillations, in which Leonid Isaakovich found himself by entering into scientific life, had played a very important role in shaping the main directions of his scientific career". But early in his career, L.I. Mandelstam was in the environment of not only radio researches but also radio business. Already the early experiments provided by

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[7] J.A. Stone was the English radio-engineer who contributed to the construction of the transmitter.

Mandelstam and Brandes with the power of communication have been adopted by radio business. These experiments were initially funded by Telebraun and then were supported by Telefunken. Their results were included in the promotional booklet, "The specificity and originality of design and architecture of Telefunken systems". As Papalexy notes, from Telefunken Company L. Mandelstam has received a significant premium for those times for these achievements [1, Vol. 1, p. 12].

As was noted in Sect. 3.4, in 1903 in his letter to Wilhelm von Siemens, F. Braun characterized Mandelstam as a talented engineer who enthusiastically accepted Braun's energy scheme [400, Lb. 703]. In 1904, Mandelstam and Papalexy worked for Telefunken Company (see [5, p. 312]). Judging on F. Braun's 6.11.1912 letter to Zenneck, they continued to work for this company till this time [397, DM, NL 53/032]. Braun had disagreement with the administration of this company, and he recommended Mandelstam and Papalexy to stop their cooperation with this company. Braun disapprovingly wrote that Mandelstam and Papalexy left for Berlin for a half of year to work for Telefunken.

It should be noted that the Mandelstam and Papalexy participation in business was restricted. In contrast to Braun and Zenneck, they did not participate in the administration. They were engineers engaged by Telefunken Company. However, they felt themselves as members of a group involved in radio business.

In Strasbourg, L.I. Mandelstam became a member of the team headed by F. Braun. Having returned to Russia, Mandelstam continued to feel himself as a member of the community which arose around F. Braun. As was mentioned, in "Die Naturwissenschaften" Mandelstam and Papalexy published a paper in commemoration of F. Braun. On October 30,1918, Mandelstam wrote to Richard von Mises: "I was shocked by the news about Braun's death. I was so attached to him". As follows from Mandelstam's wife letter to Richard von Mises [395], he maintained contact with his Strasbourg younger friend Rohman. We have no clear information about the level of Mandelstam's relations with J. Zenneck, who became an important administrative figure after World War I. However, Zenneck's recollections and Papalexy's writings about Mandelstam show that Zenneck and Mandelstam sympathized with each other. In Papalexy's notebook, there is a note that he had a conversation with Zenneck during one of his trips abroad (Autumn 1930) [399, 600-2-33].

3.8 L.I. Mandelstam Criticizes J.A. Fleming

The Mandelstam–Fleming polemics, which is mentioned in Introduction, started in Strasbourg and was induced by Mandelstam and Papalexy's research in directional radiotelegraphy (see above). Mandelstam and Papalexy followed Braun who proposed an antenna consisting of three poles. In turn, Marconi proposed an alternative setup, namely a Γ-shaped antenna (bent antenna) which radiates stronger in the direction opposite its free end. Fleming and Marconi conducted experiments which showed the advantages of the bent antenna. Besides, Fleming developed a theory of this antenna (see, e.g., [107]). Mandelstam published two articles (1907 and 1908) in which he criticized Marconi's experiments and critically analyzed Fleming's theory of the bent antenna.

L.I. Mandelstam described the situation as follows.

> Prof. Braun proposed a setup providing with antisymmetrical radiation. Many years before he described a method of the directional reception.
>
> Marconi published the results of the experiments of the directional reception which is based on the same principle that Braun proposed. Besides, Marconi has described the experiments with the directional transmitter. J.A. Fleming has theoretically described those experiments.
>
> Fleming's calculations are not correct, therefore their confirmation by experiment is only seeming.
>
> Marconi-Fleming's measurements don't form the base for the solution of practical problems since they conducted at short rang [1, vol. 1, pp. 143, 144].

Marconi's Γ-shaped antenna was successfully used for the transatlantic transmission by its inventor. As Zenneck writes, this is the best proof of the advantages of the Γ-shaped antenna [388, p. 427]. Therefore, it is worth to be in touch with Mandelstam's criticism of Fleming.

Like the other radio-engineers, Fleming proceeded from the model of an oscillator: An antenna together with its grounding wire (which is treated as a symmetrical extension down under ground) was taken as a radiating oscillator which is upright standing on the ground. It is obvious that this rod must radiate equally in all directions which have the line of the rods for an axis.

If the rod has two bends made in it so as to make an oscillator resembling in shape three sides of a rectangle, then the symmetry of radiation is destroyed (Fig. 3.8, the left part).

We may consider the oscillator as constructed in the following manner. Imagine a rectangular circuit placed perpendicularly to the earth surface, and let it be traversed by a high-frequency oscillation. Then, if a horizontal line is drawn through the center of rectangle and two points and chosen at equal distances of the right and left sides, the magnetic field at these points will be equal and normal to the plane of rectangle. If at any instant the current is flowing counterclockwise around the circuit, the magnetic forces H at the points will be directed away from the observer (Fig. 3.8, the right part). Next, suppose a wire to be placed in contiguity to the right side of the rectangle and to be traversed by an equal current in opposite phase to that in the side of the rectangle adjacent to it. The field due to this open-circuit current at two points a

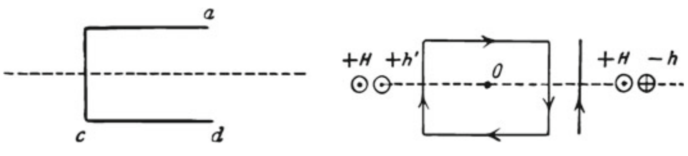

Fig. 3.8 From Mandelstam's article criticizing the Fleming theory of the bent antenna. *On the left*: the ground is taken as an ideal mirror. *On the right*: the bent antenna was supplemented to get a rectangle

and b will be toward the observer at the right-hand point and away from him at the left-hand point. Hence, if h and h' are the magnetic forces due to the open-circuit current at the points in question, the resultant fields are $H - h$ at the right-hand point and $H + h'$ at the left.

To understand Mandelstam's criticism, one needs to turn to Fleming's figure, which precedes the left part of Fig. 3.8. This is a picture of a vertically disposed rod: An antenna together with its grounding wire (which is treated as a symmetrical extension down under ground) was taken as a radiating oscillator which is standing upright on the ground. The coordinates of the middle point of the rod are designated as x', y', z'. To construct the left part of Fig. 3.8, Fleming needs to move the rod to the left by $\delta y/2$. As a result, the coordinates of the middle point have values x', $y' - \delta y/2$, z', where δy is the length of the horizontal oscillator *ba*.

Fleming calculated the electromagnetic field of the rod (dipole) in the x, y, z point. Mandelstam calls it the "point of observation". To calculate the dipole scalar and vector potentials, one needs to differentiate the magnitude containing r (the distance from the point of observation to the middle point of the rod).

Both Fleming and Mandelstam differentiate the magnitude, which is a complex function of the kind $F(f(t))$. Fleming differentiates $F(f(y))$ with respect to y; that is, he really differentiates with respect to the point of observation rather than to the middle point of the rod. Mandelstam differentiates $F(f(y'))$. However, the shift of the rod to the left means the shift of the point of observation to the right. This results in the formulas which Mandelstam emphasizes: $\frac{\partial}{\partial y} = -\frac{\partial}{\partial y'}$. Fleming does not take this into consideration. He differentiates with respect to y as if $y = y'$ [1, Vol. 1, p. 157].

The coordinate of origin of the coordinate system is located in the middle point of the rod. To construct the left part of Fig. 3.8, Fleming needs to move the origin of the coordinate system to the center of the rectangle. He needs to provide $\delta y/2$ shift of the rod, where δy is the length of the horizontal oscillator *ba*. As a result, the middle point of the rod has received the value of $-\delta y/2$.

The German engineer Karl Uller pointed to the same mistake in Fleming's writings [348].[8]

Mandelstam points to another mistake. He emphasized that Fleming had reached the expression of the magnetic field amplitude by neglecting the terms of the second-order infinitesimal. However, by proving the asymmetry of the radiation of the bent antenna Fleming solved the extremum problem where he took the terms of the second-order infinitesimal in the expression of the magnetic field amplitude.

Mandelstam gave an example where such an operation with the second-order infinitesimal resulted in an essential mistake.

Mandelstam resumed his criticism of Fleming as follows: "In his article published in "Proceedings of Royal Society" Mr. Fleming made an algebraic sign mistake and, besides, on the reason of incorrect interpretation of his formulae he came to the mistaken conclusion concerning asymmetry" [1, Vol. 1, p. 181].

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[8] Uller published a number of articles on the propagation of radio waves.

As early as 1905 F.Braun was critical with respect to Fleming but for another reason. In 1905, Braun called Fleming's statement that Marconi's apparatus withstands disturbances from foreign wireless stations a "fantastic story" (see [62, p. 25]). However, L.I. Mandelstam became the main critic of J.A. Fleming.

Fleming has not acknowledged his errors. In [108], Fleming reacted to Mandelstam's criticism. Fleming wrote that he "has not been unable to agree with this criticism" [108, p. 329]. In the same issue, Mandelstam published an article where developed his criticism [1, Vol. 1, pp. 194–161]. Fleming, however, wrote in the second edition of his book "The principles of electric wave telegraphy" the following [109, pp. 650–651]:

> Although the mathematical method of treating the problem of the bent antenna given in this section is based on ideas which are in accordance with experience, and is confirmed by the experimental work of Bellini and Tosi described below, it has been criticized by K. Uller and L. Mandelstam, who have taken exception to it on the ground the above formula… the algebraic sign prefixed to he second term inside each bracket should be changed, and −M written for +M. The author has, however, been unable to agree with this criticism. But for the detailed discussion of this difference of opinion the reader must turn to the critical articles in the Jahrbuch der Drahtlosen Telegraphie und Telephonie, Vol. 1, p. 291 and 333, 1908, and the author's rely on p. 329 of the same journal.

Fleming did not mention Mandelstam's reply to his reply in this book. In the third and fourth editions, Fleming did not mention Mandelstam's criticism at all.

3.9 The Mandelstam–Papalexy Induction Dynamometer

Mandelstam and Papalexy have not invented the dynamometer. However, in 1910 they developed the theory of this measuring instrument (this theory is reproduced in Zeneck's textbook) and invented its effective variety—short-circuit frame dynamometer.

The Mandelstam–Papalexy induction dynamometer "made it possible to read directly the values of frequency and damping decrement" [1, Vol. 1, p. 15, a comment in the footnote]. This apparatus had commercial success (see Zenneck [390, pp. 132–136]). The Mandelstam–Papalexy idea was developed in some writings (see the references in Chvolson's textbook on physics [82, the second edition, Vol. 5, pp. 384–385]).

By describing Mandelstam's dissertation (graduation work) in Sect. 3.2, we mentioned the resonance method to measure the frequency. At that time in radio-engineering, there was another problem—the estimation of oscillation damping. To measure it, one should find not simply a resonance, that is, the frequency of the measured system that would correspond to the frequency of the measuring instrument.

To estimate damping, one needs to obtain the resonance curve showing an increase of the amplitude as the difference of frequencies is decreasing.

The resonance curve can be plotted by proceeding from the heating effect of current. Mandelstam and Papalexy used the dynamometer effect. The core of it runs as follows (I follow Zenneck's book cited above). Assume a movable coil, S_2, in a vertical plane, e.g., suspended on a vertical wire, placed within a fixed coil, S_1, also in a vertical plane. If a current I_1 is passed through S_1 and I_2 through S_2, the turning moment to which the movable coil is subjected equals $I_1 I_2$. If I_1 and I_2 vary rapidly with time, e.g., in high-frequency alternating currents, the coil will in general not respond to the rapid variations and its motion will be determined by the average turning moment, i.e., the average value of $I_1 I_2$. This average value is called the dynamometer effect $I_1 I_2$ (from the use of this arrangement from a movable coil in the field of a fixed coil in the well-known dynamometer type of wattmeters).

Assume now that a primary circuit of constant frequency acts inductively upon a secondary circuit of variable wavelength, e.g., an adjustable condenser circuit. Let I_1 and I_2 represent the currents in the primary and secondary circuits, respectively. The dynamometer effect of two currents is measured and a curve plotted in which the abscissa are the wavelengths of the variable secondary circuit, the ordinates being the corresponding dynamometer effect.

The resulting curve passes through the axis of abscissa when the wavelength of the secondary circuit is equal to that of the primary, i.e., when the two circuits are in resonance.

The form of this resonance curve, similar to the current effect curve, depends on:

- the sum of the decrements of the primary and secondary circuits;
- the degree of coupling between two circuits.

If the coupling between primary and secondary circuits is extremely loose, we are able to deduce simple formulas which allow us to calculate the decrements proceeding from the "dissonance values". A dissonance value is the common fraction where the numerator is a difference in constant and variable frequencies and the denominator is a variable frequency. The constant frequency is the frequency at which the dynamometer effect equals zero. The "variable frequencies" are frequencies at which the resonance curve has some specific points. For example, let x_1 and x_2 be the dissonance values at which the dynamometer effect has its maximum positive and negative values, respectively (Fig. 3.9). Then, we have the following formula ($d_1 + d_2$ is the sum of the primary and secondary decrements):

$$d_1 + d_2 = 2\pi x_1 = 2\pi x_2 = 2\pi \frac{x_1 + x_2}{2}$$

It is interesting that in L.I. Mandelstam's 1931–1933 lectures on the theory of oscillations there is a piece which provides the foundations of the above formulas. Mandelstam proceeded from the differential equation for the forced damped harmonic oscillator:

$$\ddot{x} + 2\delta\dot{x} + \omega^2 x = E\cos pt,$$

where p is the frequency of the outside force (this is the measured frequency), ω is the frequency of the oscillator (this is the measuring device), δ is damping (here we have no damping of the force acting on the oscillator, for simplicity sake the dynamometer measures its own damping).

The solution of the equation is

$$x = X\cos(pt - \varphi),$$

and

$$tg\,\varphi = \frac{2\delta p}{\omega^2 - p^2}.$$

Mandelstam said in his "Lectures on the theory of oscillations":

Let us take a dynamometer. Between the two coils through which currents i_1 and i_2 pass, there arises a pair of forces giving a turning moment

$$M \approx i_1 i_2$$

Let the current i_1 be directly taken from the source of the variable electromotive force. The current i_2 is passed through the resonance circuit. We have then

$$\begin{aligned} i_1 &= \alpha E \sin \rho t \\ i_2 &= \dot{X}\sin(\rho t - \varphi), \end{aligned}$$

where α is a constant. On the base of these formulas, we have

$$M = \text{const} \sin\varphi\, \overline{\sin pt\ \sin(pt - \varphi)}$$

However, $\overline{\sin pt \sin(pt - \varphi)} = \frac{\cos\varphi}{2}$.

Therefore, we have

$$M = \text{const}\ \sin\varphi\ \cos\varphi = \text{const}\cdot\sin 2\varphi.$$

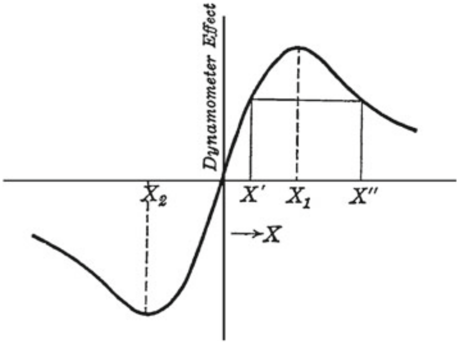

Fig. 3.9 From Zenneck's book "Wireless telegraphy". Abscissa axis—dissonance value, ordinate axis—dynamometer effect

Under the tuning in $\sin\varphi = 1$, $\cos = 0$; that is, in the case of resonance the turning moment equals to zero [2, p. 150].

However, the value of damping is not shown by resonance. This value should be estimated proceeding from the behavior of the resonance curve.

The maximum and minimum of the curve M are reached as $tg\,\phi = 1$, respectively, -1. Since $tg\,\varphi = \frac{2\delta p}{\omega^2 - p^2}$ or approximately $tg\,\varphi = \frac{\delta}{\omega - p}$ [it has been taken into account that at a small detuning $\omega^2 - p^2 = 2p(\omega - p)$]. By introducing the logarithmic decrement d through the formulas $\frac{\delta}{\omega} = \frac{d}{2\pi}$, we have $tg\,\varphi = \frac{\omega d}{2\pi(\omega - p)}$. If $tg\phi$ equals 1 (maximum of the ratio of the difference between variable and constant frequencies to the variable frequency), which is the value which Mandelstam and Papalexy introduced in their article about dynamometer. Let us recall that for the sake of simplicity we consider the measurement of d rather than $d_1 + d_2$ (we follow Mandelstam's "Lectures").

G.S. Gorelik, the disciple of L.I. Mandelstam, writes that in Strasbourg L.I. Mandelstam was occupied by the "countless variety of the phenomenon of resonance" [138, p. 139]. In his Moscow lectures on oscillations, Mandelstam generalized his Strasbourg experience.

Chapter 4
The Strasbourg Period: Optics

4.1 Mandelstam and Optics

In 1907, L.I. Mandelstam started to publish on optics. By conducting research in optics, he was being formed as Theoretician. In his first article “Über optisch homogene und trübe Medien” (“On optically homogeneous and turbid mediums”), Lord Rayleigh’s famous theory of the blue color of sky was taken under criticism. Three articles dedicated to the criticism of M. Planck’s theory of dispersion followed (1907–1908). In 1911, L.I. Mandelstam’s article “On Abbe’s theory of microscopic images” appeared. This article was followed by the article “On application of the integral equations to the theory of optical images”. As was noted, L.I. Mandelstam consulted Richard von Mises on preparing this article.

A new stage in Mandelstam’s research in optics is represented by his article “Über die Rauhigkeit freier Flüssigkeitsoberflächen” (“About the roughness of free surface of liquid”) developing statistical ideas of M. Smoluchovski and A. Einstein on the interconnection of the scattering of light and fluctuations of a scattering medium density.

The second part of the article “On optically homogeneous and turbid mediums” is dedicated to the experiment supporting the theoretical part. Mandelstam also described the experiments supporting his theory in the article “On the roughness of free surface of liquid”.

By studying optics, L.I. Mandelstam proceeded from F. Braun’s methodology. As we have seen, in his Nobel Lecture F. Braun emphasized the theoretical unity of two oscillatory sections of physics, namely optics and radiophysics. Mandelstam used his experience in radiophysics in his research in optics. This was emphasized by Landsberg who wrote about the “radio-engineering genesis of Mandelstam’s results in optics”.

The Strasbourg period is a period of the great polemic activity of L.I. Mandelstam. In the previous chapter, there was a paragraph about Mandelstam’s attack of English radio-engineer J.A. Fleming. However, L.I. Mandelstam also attacked the two great physicists Lord Rayleigh and Max Planck by publishing articles in “Annalen der

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Physik" and "Physikalische Zeitschrift". The Mandelstam–Planck polemic consists of three Mandelstam articles and two notes by M. Planck, written in response to the first two articles of Mandelstam.

In describing Mandelstam's criticism of Lord Rayleigh and the Mandelstam—Planck polemics, N.D. Papalexy unequivocally is on the side of his friend and coauthor. In the same way, Landsberg highlights key points. Landsberg's position is of much importance here. He is not merely Mandelstam's friend and coauthor, but he is an outstanding specialist in optics.

Almost everybody who wrote about the modern history of optics followed Papalexy and Landsberg. M.V. Volkenstein in his popular books [363, pp. 15–17], S. Gorelik in his classic textbook [136, p. 559], Ia.G. Dorfman in his Mandelstam biography (it was mentioned in the Introduction), D.V. Sivukhin in his "General course of physics" [318, p. 579], D.I. Trubezkov in his book on oscillations and waves [345, p. 140] went along this line.

Yu.L. Klimontovich's excursion into the Mandelstam–Planck polemics can be treated as an exception. Together with his scientific adviser V.S. Fursov, Yu.L. Klimontovich published an article on a close subject. Klimontovich referred to H. Lorentz' article "On the question of light scattering by molecules" (1910), according to Klimontovich, Lorentz "reconciled" Mandelstam and Planck by showing that their results are valid for two limiting cases (Yu. Klimontovich's essay in [377, p.66]).

It should be noted that the reconciliation, about which Yu. Klimontovich writes, arises as a result of his reconstruction of historical events. In his 1910 article, H.A. Lorentz made no mention of the Mandelstam–Planck polemics.

I.L. Fabelinskii, who wrote a lot to the history of the combinational scattering of light (Chap. 6), has not mentioned Mandelstam's criticism of Rayleigh and Planck in a historical part of his book dedicated to the scattering of light [97].

E.L. Feinberg touched upon another aspect of this story. E.L. Feinberg published a book of his commemorations entitled "Epoch and Personality". He wrote about the "self-confidence and aggressivity peculiar to young Mandelstam and overcome by him at a mature age" [103, p. 17]. This self-confidence and aggressivity appeared in the Mandelstam–Planck polemics where Mandelstam was not completely correct.

E.L. Feinberg referred to the consultations with I.I. Sobelman, who published an analytical article about Mandelstam's criticism of two theories: Rayleigh's theory of light scattering and Planck's theory of dispersion in 2002. Feinberg did not refer to this article: It was published when E.L. Feinberg's recollections had already been sent to the publisher. Feinberg and Sobelman died approximately at the same time.

4.2 "On Optically Homogeneous and Turbid Mediums"

The problem of light scattering in the terrestrial atmosphere was first considered by Lord Rayleigh in the end of the nineteenth century. He assumed that molecules scatter incoherently because they participate in thermal motion. This allows a summation to be made over the intensities of scattering by individual oscillators.

In contrast to Lord Rayleigh, Mandelstam believed that the molecular motions in the atmosphere did not make it a nonhomogeneous medium which is able to scatter light. As all "small space volumes" (approximately equal to λ^3, where λ is the length of light wave) contain the same number of molecules, waves emitted by them are coherent, contrary to Lord Rayleigh—the motion of molecules in small volumes does not make any difference. As corresponding fields are summed, the scattering does not arise. The waves which go away are mutually suppressed; we have only the waves which propagate in the direction of the incident wave.

Rayleigh explained the blue color of the sky referring to the dependence of scattering on the wavelength of the scattered light. Short wave light (namely blue light) is scattered more than, say, red light, which is long wave. According to Mandelstam, Rayleigh's conception of the atmosphere does not allow us to treat it as an optically heterogeneous medium. The atmosphere is an optically homogeneous medium, and it does not scatter light. According to Mandelstam, it is worth to look for the explanation of the blue color of sky by referring to foreign particles suspended in the atmosphere.

G.S. Landsberg's summary of L.I. Mandelstam's article "On optically homogeneous and turbid mediums" runs as follows [200, p. 88]. As was noted, Landsberg was on Mandelstam's side by considering Mandelstam's criticism of Rayleigh's theory.

> In this important research, Mandelstam set a principal question about the physical nature of the turbidity of an optically homogeneous medium, that is the medium, small (as against the length of a light wave) parts of which contain the particle numbers proportional to the volumes of the parts. L.I. showed falsity (or as he preferred to speak "insufficiency") of Rayleigh's well-known theory of the molecular scattering of light, the theory which presupposes that the molecular motion can explain the violation of the coherence of light scattered by the variety of particles constituting that medium. Molecular motion hence resulted in the turbidity of the medium that is the light diffusion in it.
>
> In turn, Mandelstam showed that in the case of homogeneity, it is possible to mark out small volumes of medium, which linear sizes would be small as compared with the wavelength, the volumes containing the constant number of particles. In a point *P*, the radiation of particles results in the light field of the same phase whether particles move or not. In the case of motion, individuality of particles changes in the given spot of the medium, but radiated phase is determined by the position of a small volume because in it, the number of particles is constant and sufficiently great.
>
> Mandelstam complemented his theoretical analysis by discussing simple, but convincing experiments and thus clearly showed that an optically homogeneous medium cannot scatter light irrespective of whether the particles are at rest or in motions.
>
> This Mandelstam's research paved the way for the statistical fluctuation theory developed by M. Smoluchovski, H. Lorentz and A. Einstein for the explanation of light scattering and critical opalescence. Mandelstam himself developed this theory for the phase interface.

What does Mandelstam himself write about Rayleigh's theory?

> In his theory of muddy media Rayleigh assumed the random motion of particles. His argumentation is approximately the following: if a plane wave falls on unmovable particles, they start to oscillate with the constant phase shift. In some point *P*, depending on the direction and distance, a certain interferential picture arises. Thus, we do not need to summarize the

> intensities (proportional to square of amplitudes of field strengths) produced by every single particle in the point P, we need to summarize the field strengths themselves. If particles are in movement, they will not have any more constant shift of phases. The field strengths in the point P do not have constant phase shift, too (apart from the case when the direction to P and a line connecting the particles coincide with the direction of the wave propagation). As the wave length is small, the phase shift runs along all possible meanings even over a short time. In this case it is possible to sum up the intensities.
>
> This is valid for a couple of particles. If we have many particles, then, I think, it does not make any difference whether an interferential pattern in the point P is produced by two certain particles or two spatial areas which are small with respect of wave length and equal to each other with respect of a number of particles constituting them. However an optically homogeneous medium can always be divided into such space areas, because this is the definition of homogeneity. Thus we conclude that an optically homogeneous medium cannot be turbid irrespective of whether the particles are at rest or in motion. I consider inadmissible to apply the Rayleigh theory of muddy media to the atmosphere. Air should be treated as an optically homogeneous medium since a cube the edge of which equals to the wave length of sodium light contains $5 \cdot 10^6$ molecules which Rayleigh considers to be scattering particles. [1, Vol. 1, p. 116]

Both Rayleigh and Mandelstam treated the molecules scattering the light of the sun as electron oscillators. I.I. Sobelman (see preceding section) pointed out, however, that the coherence of forced oscillations of the oscillators should be estimated by comparing the wavelength with the mean free path rather than with the average intermolecular spacing. It makes clear that the molecular motion is essential if the average intermolecular spacing is less than the wavelength is. “Induced oscillations of an oscillator in the field

$$E = \exp[-iwt + kR(t)]$$

contains a factor $\exp[ikR(t)]$, where $R(t)$ is the oscillator coordinate. In a mean free time τ of a molecule, the phase $kR(t)$ acquires an increment $k\nu\tau$, where ν is the oscillator velocity. But $\nu\tau = l$, where l is the mean free path. Hence one finds $\kappa\nu\tau \cong \frac{2\pi}{\lambda} l$ and the question of coherence of induced oscillations of the oscillators in the medium should be solved by comparing λ not with R, but with the mean free path l” [322, pp. 75–76].

An estimation of the phase shifts in the Earth atmosphere is provided in the following way. The wavelength of visible light is of order of 10^{-4} cm. For the upper terrestrial atmosphere, the average distance between molecules is 10^{-6} cm. A volume smaller than λ^3 contains approximately 10^6 molecules. According to Mandelstam, the light scattering by the particles which constitute the medium is responsible for the light field of the same intensity and phase, irrespective of whether the particles are in rest or in motion. There is no incoherence.

However, let us consider the mean free path $l = 1/N\sigma$, where σ is the elastic cross section and N is the amount of oscillators in cm^3. We conclude that $l > 5 \times 10^{-5}$ cm and $l/\lambda > 1$. So the random phase shifts $k\nu\tau$ are considerable and exceed 2π.

Rayleigh was right when treating the light scattering by the molecules of the upper atmosphere as incoherent.

It is remarkable that L.I. Mandelstam's criticism has not influenced the treatment of molecular light scattering in the textbooks. Only Russian textbooks referred to Mandelstam's criticism (see Sect. 4.1).

4.3 The Mandelstam Criticism of M. Planck's Theory

In the biography of Mandelstam (that opens the first volume of Mandelstam's "Complete Works"), the description of the Mandelstam–Planck polemic subsequently followed the exposition of Mandelstam's article dedicated to Lord Rayleigh' theory. Mandelstam's biographers write:

> Mandelstam's papers "On the theory of dispersion" are closely connected with this article. They are dedicated to the discussion of possibility to explain the light attenuation by referring to the light scattering. M. Planck proposed such an explanation in his theory of dispersion. However Mandelstam showed that Planck's theory was not able to explain the attenuation of the transmitted wave. Mandelstam conducted the calculations which showed that the essence of the problem consisted in a distinction between the damping of isolated oscillators and the damping of the oscillators constituting a medium. [1, Vol. 1, p. 15]

This historical excursion was finished in an amusing way. M. Planck's postcard is cited in the biography of L.I. Mandelstam. In this postcard, Planck agrees with Mandelstam and writes that he made a corresponding correction.

Here, there is a lack of coordination. L.I. Mandelstam's article "On the theory of dispersion" dedicated to the criticism of M. Planck's theory was published in 1907. Planck's card is dated by 1904. Probably, it was M. Planck's reaction to some unpublished Mandelstam address.

As was noted, in 1907 L.I. Mandelstam took under criticism the famous theory of the blue color of sky put forward by Lord Rayleigh at the end of the nineteenth century. Mandelstam continued this criticism in his article "On the theory of dispersion" ("Physikalische Zeitscrift", 1907) where the main target became Max Planck's theory. Planck rejected Mandelstam's criticism in a short note published in "Physikalische Zeitschrift" in the same year. Mandelstam reacted by publishing the article where he developed his criticism ("Physikalische Zeitscrift", 1908). Planck again rejected Mandelstam's criticism ("Physikalische Zeitscrift", 1908). Mandelstam insisted in an article ("Physikalische Zeitscrift", 1908) which had not already received Planck's reply. However, in 1909 in "Annalen der Physik" R. Gans and H. Happel published an article "Zur Optik der kolloidaler Metallösungen" ("On the optics of colloidal solutions of metals") which contained a section dedicated to Mandelstam's criticism. Gans and Happel analyzed the Mandelstam 1907 attack on Planck's theory of dispersion and objected to Mandelstam's logic and conclusion.

What was the point of the Mandelstam–Planck controversy? In his article on the theory of dispersion, Mandelstam argued that under Planck's presumption and contrary to Planck "the wave attenuation resulting from dispersion should not be anticipated" [1, Vol. 1, p. 125]. Like Planck, Mandelstam treated molecules scattering light as elementary oscillators (resonators). Planck, however, showed that the attenuation of a transmitted light wave in the absence of dissipation resulted from its scattering. This is connected with radiative damping which results from the deceleration of oscillator oscillations caused by its intrinsic radiation field. Planck adopted Rayleigh's presupposition that the light scattering in the terrestrial atmosphere proceeds incoherently.

Mandelstam's discussion differed from that of Planck. Mandelstam called Planck's approach quasistatic. Taking under consideration the interaction of oscillators in small volumes, Mandelstam supposed that damping resulting from the radiation of oscillators was compensated by the radiation action of the other oscillators in the small volume.

L.I. Mandelstam proceeded from the Maxwell equations where the induction vector is

$$D = E + 4\pi N p.$$

Notation: E is the average space value of the electric field, p is the oscillator dipole moment, and N is the number of oscillators per cubic centimeter.

For $p(t)$, advantage is gained by the equation

$$\ddot{p} + \omega_0^2 p - \frac{e^2}{m}\frac{2}{3c^3}\dddot{p} = \frac{e^2}{m}E',$$

where p is the oscillator dipole moment, E' is the electric field which would exist at the location of an oscillator if we had removed the corresponding oscillator together with its field. Planck referred to E' as "the driving force".

In order to calculate E, the average spatial electric field, both Planck and Mandelstam, took the difference $E - E'$ under consideration (the difference between the average and driving fields). E differs from E' by two positions: (1) E refers to an infinitesimal element of space, but E' refers to the points where there are oscillating oscillators; (2) when constructing E', we should take the corresponding oscillator as removed together with its field.

The term containing $\dddot{p}$ is responsible for damping due to radiative friction. Since the damping is weak, for harmonic oscillations at a frequency ω it is possible to make the change

$$-\frac{e^2}{m}\frac{2}{3c^3}\dddot{p} \cong \frac{2}{3}\frac{e^2\omega^2}{mc^3}p = \gamma_{\text{rad}}p,$$

where γ_{rad} is the radiative damping constant, $p = ex$ (p and x are vectors).

According to Planck, one sets'

$$E' = E + \frac{4\pi}{3}Np.$$

Mandelstam's calculations give

$$E' = E + \frac{4\pi}{3} Np - \frac{2}{3c^3} \dddot{p}.$$

Therefore, the radiative friction is removed from the equation

$$\ddot{p} + \omega_0^2 p - \frac{e^2}{m} \frac{2}{3c^3} \dddot{p} = \frac{e^2}{m} E',$$

which was above written.

As was noted, L.I. Mandelstam insists that it is not correct to proceed from the static state of medium as Planck does. "This means to neglect the terms which have the same order and form (with respect to partial derivations) as the term $\frac{2}{3c^3}\frac{\partial^3 p}{\partial t^3}$" [1, Vol. 1, p. 128].

Later in another article by reacting to Planck's objections, Mandelstam conducted a visual discussion of the compensation of effects represented in the mathematical formulas. "The main result of Mr. Planck's theory can be presented as follows. If a light wave is transmitted through an optically homogeneous medium, a part of the energy is scattered by the elementary oscillators. The scattered energy equals to the sum of energies which were emitted by the oscillations of each oscillator in accordance with its oscillations and by means of its radiation as if this oscillator was alone in the field. As a result we have the attenuation which can be interpreted as an absorption" [1, Vol. 1, p. 170].

Mandelstam proceeded from the assumption that "by the part of the force which results in the damping of an oscillating electron, this electron acts not only on itself but also on each charge which is located at a distance which is small with respect to the wavelength". This is physically obvious. Let us have two oscillators which are located within the wavelength distance. Let us give them identical but oppositely directed moments. Let them oscillate without any additional supply of energy. In this case, damping which results from radiation should be small as compared with the damping of oscillations of a single oscillator, the damping resulting from its radiation. This means that "the dissipative part of the force which an electron acts on itself is compensated by a corresponding part of the force which acts on this electron due to another electron" [1, Vol. 1, p. 169].

In the article written in reply to M. Planck's counter-criticism, Mandelstam wrote: "Mister Planck predetermined optical homogeneity. He also admitted that the oscillator sizes are vanishingly small as compared with their mutual distances.

Mathematically this means that the damping of oscillators results from the term $\frac{2}{3c^3}\frac{\partial^3 p}{\partial t^3}$ which appears in the equation of oscillations of an electromagnetic oscillator which is under the action of the external field.

I have shown that the term $\frac{2}{3c^3}\frac{\partial^3 p}{\partial t^3}$ has only appeared due to Mr. Planck's mistake and under a correct calculation it has not appeared" [1, Vol. 1, pp. 170–171].

G.S. Landsberg's statement about "radio-engineering genesis of Mandestam's research in optics" was mentioned above. It was just said in connection with Mandelstam's articles on dispersion. "Their essence comes to research in damping of isolated oscillators and oscillators constituting a homogeneous medium. By explaining this research Mandelstam pointed to the analogy with the phenomenon which every radio engineer can manage now. This phenomenon is the following: radiative resistance of every oscillator constituting an antenna can be considerably less than radiative resistance of such an oscillator taken separately" [200, p. 89].

4.4 Max Planck's Reply: Polemics

In the same 1907, M. Planck replied to Mandelstam's criticism by publishing a short essay in "Physikalische Zeitschrift" [281]. Planck emphasized that he does not agree with the cancelation of the item containing the third derivation since the "different oscillators have different (phase shifted) moments".

"Indessen kann Ich die Rechnungen des Herrn Mandelstam nicht als korrekt ansehen, auch nicht in erster Annäherung, und zwar deshalb, weil in denselben das Moment p eines Resonators nur als Funktion der Zeit behandelt wird, während p doch auch vom Ort des Resonators abhängt" [281, p. 214].

("I can not consider Mandelstam's calculations as correct and also correct in the first approximation, namely because in them the moment of the resonator p is taken as a function of time only, whereas the moment p also depends on a space coordinate of the resonator" [281]).

In the second article on dispersion (1908), L.I. Mandelstam recounted the field of oscillators in another method and again concluded "that the space average electric field does not contain the item $\frac{2}{3c^3}\frac{\partial^3 p}{\partial t^3}$. This takes place irrespectively to the fact whether the other oscillators have the same moment" [1, Vol. 1, p. 168]. "Within the small range with respect to the wave length the field of an oscillating electron contains the constant item $\frac{2}{3c^3}\frac{\partial^3 p}{\partial t^3}$. Thus this item does not appear in the equation connecting the field in the area of an oscillating electron with its moment" [1, Vol. 1, p. 169].

"In response to Planck's criticism, pointing out that the radiation fields of the neighbors should also be included, Sobelman writes, Mandelstam took these into account in the subsequent papers. He carried out an extensive calculation of the radiation fields of the oscillators in the medium, but in the summation of the fields of the neighboring oscillators be made every effort to retain the homogeneity of the medium. In calculating the resultant sums, a large volume V is divided into cells, each of which contains strictly one particle.

As a result, Mandelstam obtained a complete compensation for the radiative friction forces… No attenuation occurs due to scattering… " [322, p. 78].

M. Planck reacted by the essay in which he stated that he did not understand the essence of Mandelstam's recalculation. What is correct in Mandelstam's article is well known, and what Mandelstam puts forward as a novelty is not understandable. "Mr. Mandelstam's model is so oblique and obscure that I cannot hope to be successful in my meditations like I was successful by taking the preceding more simple model" [282].

M. Planck wrote: "All the principal controversy about whether it is possible to explain the scattering of light in the dispersion medium by referring to the radiation of the elementary oscillators which provide dispersion, as far I understand, comes to the following. When the term of higher order is taken into account, actually there is no scattering. This corresponds the situation that identical oscillators adjoining each other constitute the medium which can be treated as absolutely homogeneous. However, my analysis gives a theory completely similar to the theory which Lord Rayleigh put forward. This theory takes into account the terms of higher order, which originate from the atomistic structure of matter, and it comes to the conclusion about the scattering by means of radiation" [ibidem].

Mandelstam also sharply reacted. "Mr. Planck objected that his equations are valid in spite of my objections. One only needs take into consideration, Mr. Planck wrote, that different oscillators have different out of phase moments. This means that the term $\frac{2}{3c^3}\frac{d^3p}{dt^3}$ should again appear in the equations. Mr. Planck has only designed his calculations. I have conducted this calculation by proceeding from Mr. Planck's project and I again came to my result.

Besides this calculation, Mr. Planck's objections are falsified by my discussion which showed that the equations of oscillator oscillations that don't contain $\frac{2}{3c^3}\frac{d^3p}{dt^3}$ can be deduced in such a form which shows that the moment of other oscillations do not play a part.

I do not understand the criticism directed against it by Mr. Planck" [1, Vol. 1, p. 171].

Planck had not published anything in reply to L.I. Mandelstam's third article on dispersion.

However, R. Gans and H. Happel article "Zur Optik kolloidaler Metallosungen" appeared in 1909. In this article, one section was dedicated to the Mandelstam–Planck polemics. This section was entitled "Beziehung zwischen der elektrischen Feldstärke und dem elektromagnetischen Schwingungszustande eines Metallteilchens. Der Mandelstamishe Einwand gegen die Planksche Theorie (The relation between the electric field strenghth and electromagnetic oscillatory state of metallic particles. Mandeltam's objections against Planck's theory)".

Gans and Happel supported M.Planck's result. True, they took into consideration Mandelstam's first article only.

Gans and Happel counted that Mandelstam made a mistake when he "assumed that on average the M_1 oscillators are contained in some sphere irrespective whether this sphere is arbitrary chosen or this is the sphere in the centrum of which an oscillator is located" ("Das Mandelstamshe Fehler ist vielmehr der, dass er annimmt, es liegen in einer Kugel im Mittel M_1 Dipole, gleichgültig, ob die Kugel beliebig aus dem Medium herausgegriffen wird, oder ob man nur solche Kugeln wählt, in deren Zentrum sich ein Dipol befindet") [127, p. 291].

"We shall show, Gans and Happel write, that in the latter case $M_1 + 1$, oscillators are contained in the sphere. Therefore, M_1 oscillators are contained in it when the oscillator which was located in the centrum of the sphere has been removed. As a result, the controversial term in the difference $\Xi^1 - \Xi$ disappears and Planck's result is valid" (ibidem).

Let us "translate" the Gans and Happel arguments into Mandelstam's terminology. As was noted, Mandelstam analyzed the difference $E - E'$, where E is the average spatial electric field and E' is the electric field which would have existed at the location of a resonator had we removed the corresponding oscillator together with its field.

Thus, both E and E' can be taken as the sums of three items: (1) the external field, (2) the field of the oscillators that are located outside the sphere, and (3) the field of oscillators that are located inside the sphere. The first two items are identical for E and E'. Mandelstam refers to the third items as B and, respectively, B'. Under some natural assumptions, $B = -\frac{4\pi}{3}Np$ where N is a number of oscillators in an unit volume. Planck admits that $B' = 0$. For E, he writes an equation containing an item with the third time derivative of the dipole moment.

According to M. Planck, if there is no dissipation (and correspondingly there is no item containing the first time derivative of moment in the expression for E), the attenuation of a transmitted wave results from scattering. In other words, this attenuation is provided by the slowing down of the oscillator by its own radiation field. It is essential for Planck that the expression for this field contains an item with the third time derivative of moment. This item refers to the additional forces depending on the time derivation of the charge acceleration, and it explains the scattering of light (these forces slow down oscillators).[1]

L.I. Mandelstam insists that it is not correct to proceed from the static state of medium as Planck does. "It is not correct to calculate B and B' on the base of formulae corresponding a statistical state. This means to neglect the terms which have the same order and form (with respect to partial derivations) as the term $\frac{2}{3c^3}\frac{\partial^3 p}{\partial t^3}$ [1, Vol. 1, p. 128].

Let me repeat some of Mandelstam's discussion. Mandelstam takes a system of coordinates with the origin in the center of a sphere. Let the axis z, Mandelstam writes, be directed as the resultant moment of all the oscillators which are contained in a sphere.

He calculated the intensity of the field produced by an oscillator located at the distance r from the center. This intensity is the sum of three components containing zero and, respectively, second and third time derivatives of the moment. The first component corresponds to a static state. This component was taken into account by M. Planck. The second component can be thrown away by reason of the symmetry. Having calculated B and B' by having taken into account the third component, Mandelstam came to the following formulas:

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[1] The L.D. Landau and E.M. Lifshitz course refers to this effect as the retardation of radiation [198].

$$B = -\frac{4\pi}{3}Np + \frac{2}{3}N_1\dddot{p},$$
$$B' = \frac{2}{3}(N_1 - 1)\dddot{p},$$

where N_i is an amount of oscillators inside the sphere.

This means that

$$E' = E + \frac{4\pi}{3}Np - \frac{2}{3c^3}\dddot{p}\,.$$

Above we have already seen this formula.

In Mandelstam's terminology, what Gans and Happel designated as $\Xi^1 - \Xi$ is $E - E^1$, what Gans and Happel designated as M_1, is N_1. According to Gans and Happel, $B' = \frac{2}{3}N_1\,\dddot{p}$ and the expression for E' does not contain the item with the third time derivative of the moment.

Neither Mandelstam, nor his disciples, who wrote his biography, reacted to the comment of Gans and Happel. It should also be noted that Gans–Happel's article had not any considerable resonance in literature.

Paul Ehrenfest (1880–1933), who lived in Russia, then wrote several letters to Mandelstam (24.5.1911, 2.6.1911, 22.9.1911, 5.1.1912, 8.11.1912). These letters were published in the book dedicated to Mandelstam's anniversary.

P. Ehrenfest was concerned with the Mandelstam–Planck polemics and Gans–Happel's criticism of Mandelstam's critics of Planck's theory [8, p. 55]. Ehrenfest sympathized with Mandelstam's position, but he had some doubts about it. Mandelstam's replies to Ehrenfest are not known to the present author.

4.5 The Years of Democracy: I. Sobelman Criticizes L.I. Mandelstam

By the end of the twentieth century, good circumstances for research in the history of science arose in Russia. Many archival documents became available for research; many themes which were prohibited under communist power became open. But there is another point. Many specialists of high level were concentrated in the institutes of the Academy of Sciences, and under a deficit of young scientists, these specialists often turned to historical subjects. Their historical essays were published in the authoritative scientific journals.

Such a historical essay was mentioned in Sects. 4.1 and 4.2. This is I.I. Sobelman analytical article dedicated to Mandelstam's criticism of Rayleigh's theory of blue sky and the Mandelstam–Planck polemics. In the present section, I.I. Sobelman's analysis of this polemics is under consideration. At the beginning Sobelman writes [322, p. 80]:

> When discussing the Mandelstam-Planck polemics I will endeavor to assume an unabiased attitude. I will note fallacies and inaccuracies, but in doing this I will not simplify the problems that faced the physicists a century ago. I will also try to show that the dispute between Mandelstam and Planck was actually concerned not with a particular problem of light scattering. The case in point was a controversy about whether a medium can be homogeneous despite the thermal molecular motion in the medium. Or whether a medium without fluctuations is possible, as we would put it today. But at that time the concept of fluctuations, their unavoidable and universal nature did not exist. The works of Smoluchowski and Einstein made their appearance later. Planck proved to be right in this dispute. Although he did not invoke the notion of fluctuations explicitly, the results for light scattering in gases he arrived at turned out to be the same as if he were doing all the calculations with due regard for fluctuations.

Let us reproduce Sobelman's argumentation in favor of M. Planck and contrary to L.I. Mandelstam.

Mandelstam indeed proceeded from the presumption that a transparent medium is homogeneous. Although he does not explicitly declare it, he admits that oscillators are regularly located in space. According to him, the interaction of oscillators through their radiation fields results in the complete compensation of radiative damping. There is no attenuation of intensity of a light beam, and there is no scattering which would result in the attenuation.

By contrast, Sobelman emphasized that M. Planck adopted, after Rayleigh, that independent oscillators incoherently scatter light. He constructed theory which would give the attenuation of intensities of a light beam. He introduced fluctuations implicitly. Later on, when the concept of fluctuations as realized (M. Smoluchowski, A. Einstein), it become clear that the scattering in rarefied gases is determined by the fluctuations of density or the number of particles, i.e., by the quantity $\overline{\Delta N^2}$. But for an ideal gas, one finds that $\overline{\Delta N^2}$ simply equals *N*, i.e., the number of oscillators in a unit of volume. In other words, the result arrived at is precisely the same as in the consideration of the light scattering by individual oscillators. "In the Mandelstam—Planck discussion Planck was doomed to obtain the correct result. He supposedly sensed that the thermal molecular motion is bound to disturb the homogeneity" [322, p. 77].

By reacting to M. Planck's objections, Mandelstam again proceeded from his treatment of homogeneity that is from the regular spatial arrangement of oscillators. "Mandelstam, Sobelman writes, carried out an extensive calculation of radiation fields of the neighboring oscillators in the medium, but in the summation of the fields of the neighboring oscillators he made every effort to retain the homogeneity of the medium" [322, p. 78].

Following Y.L. Klimontovich, Sobelman appealed to H.A. Lorentz' 1910 article "On the question of light scattering by molecules". True, in contrast to Klimontovich, Sobelman did not write that H.A. Lorentz "had reconciled" Planck and Mandelstam. Sobelman writes that "one can see from the text of the paper that the paper was a direct answer to the questions posed by Mandelstam. Lorentz gave a through derivation of the formulas which define the interaction of oscillators in the medium via their radiation fields. The resultant sums over the oscillators of the medium surrounding a given oscillator were calculated in two ways—first assuming the oscillators of the medium to be regularly distributed in space, and next for an irregular distribution. In the former case, the result he obtained is that in the absence of dissipation the $\varepsilon(\omega)$ function is real and Im $\varepsilon = 0$. In the latter case, he arrived at the result of Rayleigh and Planck" [322, p. 78].

In conclusion, I.I. Sobelman writes that his article is principally of historical importance. "The works of Lorentz and Einstein dotted the i's and crossed the t's. The Mandelstam–Planck polemics ceased" [322, p. 80]. In his 1913 article which will be described in the following section, L.I. Mandelstam completely abandoned the postulate of optical homogeneity of a medium which he adopted in his articles dedicated to the criticism of M. Planck's theory.[2]

4.6 "On Roughness of Free Surface of Liquid"

In 1913, L.I. Mandelstam published an article "On roughness of free surface of liquid". In this article, he already proceeded from the ideas developed by M. Smoluchovski and A. Einstein on the base of statistical mechanics. About this article, G.S. Landsberg says in the fragment the beginning of which was cited in Sect. 4.2:

> This Mandelstam's research paved the way for the statistical fluctuation theory developed by M. Smoluchovski, H. Lorentz and A. Einstein for the explanation of bulk light scattering and critical opalescence. Mandelstam himself developed this theory for the case of scattering by the phase boundary.

Here, a comment is needed. As we have seen, Planck rather than Mandelstam paved the way for statistical fluctuation theory. Although Planck did not invoke the notion of fluctuations explicitly, he arrived at the results which turned out to be the same if he were doing his calculations with regard to fluctuations.

By developing the fluctuation theory, L.I. Mandelstam followed M. Smoluchovski, H. Lorentz, and A. Einstein.

The roughness means deflections of the free surface of a liquid which is in a gravitational field. These deflections arise due to heat motion. Mandelstam writes "The surface of liquid which would be planar under ideal equilibrium, is persistently deformed due to irregular thermal motion. When a light beam is reflected from such s surface, along with regular (specular) reflection, diffuse reflection is also bond to occur. Very small roughness (with respect to wave-length) is enough to make scattering to be noticeable" [1, Vol. 1, p. 247].

In the article under consideration, Mandelstam does not argue with the authorities. To the contrary, he is clearly aware that his article represents the trend which was indicated by Marian Smoluchovski and Albert Einstein. Mandelstam emphasizes that roughness, which he takes into consideration, was predicted by Smoluchovski on the base of a statistical approach. He also emphasizes that he will calculate roughness according to Einstein's method.

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[2] Sobelman did not take into consideration [180] where Plank's theory is criticized.

By considering fluctuations to be the cause of optical inhomogeneity Einstein expanded the density perturbations into a three dimensional Fourier series. In turn, Mandelstam expands the roughness into a two dimensional Fourier series. As Mandelstam's biographers write, "every component of two dimensional series is treated as a diffraction grating. L.I. Mandelstam directly points out that in essence scattered light is light diffracted by these gratings. This is contemporary conception of the molecular light scattering" [1, Vol. 1, p. 127].

I.L. Fabelinsky in his book "Molecular Scattering of Light" provides the following description of the story [97, p. 9].

"The density fluctuations… produce a fluctuation in the optical dielectric constant. These random fluctuations of the dielectric constant are in fact the sole of physical reason for the optical inhomogeneities of the medium and the scattering of light in optically pure media.

The fruitful idea of Smoluchovski (1908) on fluctuations as the reason of light scattering lay at the base of the statistical theory of light scattering developed by Einstein. In addition to fluctuations in the density, Einstein also took into account fluctuations of concentration which take place in solutions.

Fluctuations not only destroy the optical homogeneity within the substance, but also lead to the destruction of the mirror smoothness of the surface of a liquid or boundary of two immiscible liquids. Thermal fluctuations oppose the forces of surface tension that create the surface, thus making the surface 'rough'.

As a result of the molecular 'roughness' of the surface of a liquid, molecular light scattering takes place in directions different from the direction of specular reflection of the primary light beam.

This phenomenon had already pointed out in the work of Smoluchovski. Mandelstam gave the theory of the phenomenon and experimentally discovered the molecular scattering by the surface of a liquid.

As a result of these investigations of Rayleigh, Smoluchowski, Einstein, and Mandelstam, a new direction was opened up for molecular physics and molecular optics".

Mandelstam's article "On roughness of free surface of liquid" attracted the attention of A. Einstein. Einstein's short letter from Zurich (27.1.1913) evidences it [8]: "Dear Mister Mandelstam. I have just reported about your fine work on the fluctuation of surfaces. P. Ehrenfest told me about it. I am sorry that you are absent. With best wishes, Yours Albert Einstein".[3]

Besides Einstein's signature, there are signatures of other participants of Zurich colloquium. The historian of physics Boris Iavelov decoded the majority of signatures and reconstructed the situation which had a place when Mandelstam's article was under discussion [384]. Paul Ehrenfest, Otto Stern, Max Laue, and others participated.

As was noted, P. Ehrenfest was acquainted with L.I. Mandelstam and discussed the problem of light scattering with him. Max Laue (1879–1960) was a prominent spe-

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[3] Since 1912 to 1914, Einstein was an Ordinarius of mathematical physics at Federal Polytechnical Institute. Richard von Mises graduated from this Institute.

cialist in the X-ray structural analysis and physics of solids. According to Papalexy, Laue came to Strasbourg in 1913 and had a conversation with L.I. Mandelstam concerning his theory of optical images [1, Vol. 1, p. 22]. Otto Stern (1888–1970) is one of the authors of the molecular beams method.

4.7 “The Radiation of the Light Source, Located Very Close to the Boundary Between Two Transparent Media”

This article published at the beginning of 1914 is mentioned in passing by the biographers of L.I. Mandelstam. Let us ask L.I. Mandelstam himself to speak [1, Vol. 1, p. 261].

> The experiments described below concern a phenomenon which occurs when the source of light is very close to the boundary surface of two transparent bodies. The regular optics laws do not offer a satisfactory explanation of this phenomenon.
>
> The phenomenon consists in the following. Let us take a couple of infinite media separated from each other by a plane. Let a source of light is located in the medium which has less optical density. If we follow the rays emitted by this source and take under consideration the refraction of these rays on the interface, we shall have a regular result: In the more dense medium, the light is contained within a cone, the extent of which does not exceed the double maximum angle ϕ ($\sin \phi = n$, n is the relative refractive index).[4]
>
> The method to determine the refractive index (the method of the critical angle of refraction) is based on the fact that there exists a similar sharp frontier between light and shadow in more dense medium. However, if the source of light is located nearby an interface, the things are going in another way. The smaller the distance between them, the more blurred becomes the boundary of light. When this distance is small in comparison with the wavelength, light is radiated in every direction and the critical angle does not exist.

By formulating this problem, Mandelstam noticed that as an electrical problem, this problem was posed by A. Sommerfeld in his 1909 article which was ideologically connected with J. Zenneck’s article on surface waves (J. Zenneck considered the propagation of waves along the surface separating air and the Earth). In particular, A. Sommerfeld showed that the Zenneck surface waves could be generated by the Hertz dipole which would be vertically polarized and placed under the conductor surface.

In a popular article published already in Russia in 1916, Mandelstam explained in which way Sommerfeld’s article influenced him. Sommerfeld analyzed the radiation of an oscillator which is located nearby the Earth surface. He took an oscillator which is upright staying on the Earth surface. In contrast to his predecessors, Sommerfeld did not treat the Earth as an absolute conductor and the atmosphere as an absolute isolator. He endowed the Earth and its atmosphere with their conductivity and permittivity.

Mandelstam emphasized that in his 1914 article, he had translated Sommerfeld’s formulation into the language of optics. This means that he took the problem of

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[4] Angle of total internal reflection; critical angle of internal reflection.

refraction and reflection of the light waves in the case of two different media by admitting that the interface is plane. He replaced permittivity by the refraction index and conductivity by the absorption.

This resulted in the simple optical problem. However, Sommerfeld's article pointed to its nontrivial reinterpretation. In optics, we regularly admit that the source of light is located far from an interface that the distance between them is high in comparison with the wavelength. However, Sommerfeld considered the source of radiation which is close to the interface (to the Earth surface). The thing is that in optics and radio-engineering, we take different wavelengths into account. For example, the wavelength of yellow light is 0.0006 mm. The distance of 1 m is high for it. Radiowaves can have lengths equal to 100 m or even 1000 m. Their source is always close to the interface.

The translation of Sommerfeld's problem into optics language came not merely to the problem about the source of light, but to the problem of the source of light located nearby the interface. As L.I. Mandelstam writes, "new unknown optical phenomena arose here. I needed to somewhat generalize the Sommerfeld theory. Then I conducted experiments where one medium was glass, the other was water. The experiments showed that in the situation where the source of light is located nearby the interface, the usual laws of refraction are violated and should be replaced by the other laws" [1, Vol. 3, pp. 41–42].

E.L. Feinberg saw here something close to the quantum effect of the penetration of potential barrier. L.I. Mandelstam and his graduate student M.A. Leontovich were close to the description of this effect in their 1928 article. "This discovery, Feinberg writes, was for L.I. not accidental at all. In Strasbourg, when studying optics, he had already demonstrated both theoretically and experimentally that optical waves that should have experienced a so-called complete internal reflection from a border of a solid body, e.g. glass, in which they propagated, with air, did in fact partially jump through the gap with air if the same glass was placed nearby. Such integral understanding of classical and quantum physics was characteristic of Mandelstam" [103, p. 29].

4.8 The Radio-engineering Genesis of L.I. Mandelstam's Optics

G.S. Landsberg's statement on "radio-engineering genesis" of Mandelstam's research in optics has been cited several times above. The philosophy peculiar to Mandelstam's and some of his disciples is lying under this statement. This philosophy can be traced back to Mandelstam's and Papalexy's Teacher F. Braun, who meditated about the productivity of radio-engineering analogies in optics and optical analogies in radio-engineering.

In his 1934 lectures, Mandelstam generalized the method of analogies. He spoke about the mutual aid between different "oscillatory" areas of physics—optics, electricity, magnetism, acoustics. He constructed many analogies which provided such an aid: tuning fork and closed electric circuit, the Froude pendulum (friction pendulum—a pendulum with a sleeve mounted on a rotating axis) and the tube generator. Mandelstam said in his 32nd lecture, "The laws of interaction of oscillating systems are very specific. At the same time they are general for very different phenomena which take place in the electric circuits, in pendulums, in crystals. These are very different things, but the oscillatory laws are general for them" [2, p. 302].

In his 1944 "Lectures on some problems of the theory of oscillations", L.I. Mandelstam raised the oscillations theory, which he earlier called "a modest area", up to the level of an universal physical discipline. He again pointed to the "mutual aid". "Dark places, say, in optics are illuminated like by means of a floodlight in the process of studying oscillations in mechanics, etc." [ibid., p. 402].

As an example of the "radio-engineering genesis" (the ideological aid from the side of radio-engineering to optics) can be taken L.I. Mandelstam's article described in the previous section. In this research, Mandelstam proceeded from Sommerfeld's article concerning the problem which was put in wireless telegraphy.

Another example. In 1910, L.I. Mandelstam published an article about the damping of natural oscillations of luminous sodium vapors. This article followed the article which had been written Mandelstam in coauthorship with N.D. Papalexy "On the measurement of the logarithmic decrement and frequency of electromagnetic oscillatory systems" (see the previous chapter). In this article in optics, advantage was taken of the Vilhelm Bjerknes method of the determination of damping by means of reading of a resonance curve [1, Vol. 1, p. 18]. In their article, Mandelstam and Papalexy presented the method alternative to Bjerknes'. However, for the optical problem of the eigen-oscillations of sodium vapor Bjerknes' method turned out to be suitable.[5]

However, is the method of radio-engineering analogies always productive? Mandelstam's criticism of Max Planck shows that a positive answer here is problematic. Young L.I. Mandelstam uncritically took the radio-engineering idea of the interaction of oscillators through their radiation into optics. He proceeded from a regular structure of the system of oscillators: like in an antenna oscillators form a homogeneous medium. "This is physically obvious, Mandelstam wrote. Let we have two oscillators which are located within the wavelength distance. Let us give them identical but oppositely directed moments. Let they oscillate without any additional supply of energy. In this case, damping should be small as compared with the damping of oscillations of a single oscillator; the damping resulted from its radiation. This means that "the dissipative part of the force which an electron acts on itself is compensated by a corresponding part of the force which acts on this electron due to another electron" (it was cited in Sect. 4.3).

In this connection, Landsberg wrote that in essence in his research in dispersion, Mandelstam proceeded from his reflection on the damping of isolated oscillators and oscillators constituting a homogeneous medium. "By explaining this research, Mandelstam pointed to the analogy with the phenomenon which every radio-engineer

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[5] In Chap. 3, we mentioned that V. Bjerknes had described the extremely loose coupling.

can manage now. This phenomenon is the following: radiation resistance of every oscillator constituting an antenna can be considerably less than radiation resistance of such on oscillator taken separately" (it was cited in Sect. 4.3).

The simplest theory takes an antenna as an oscillator. A more sophisticated theory treats an antenna as a sequence of oscillators. This theory uses the concept of radiational resistance of an antenna. This is a fictitious (non-ohmic) resistance representing the energy loss in the antenna. In a radiating antenna, the oscillation damping is more intensive than in a lack of radiation (this was an usual point of view—see, for example: [241]).

In his polemical articles on dispersion, L.I. Mandelstam tacitly proceeded from the radio-engineering experience of his time, and in the words of Francis Bacon, this experience so besets his mind that "truth could hardly find entrance".

4.9 Mythology

The above discussion gives us a chance to speak about mythology in the history of science. The biographers of Mandelstam do not follow only empirical facts in describing Mandelstam–Rayleigh and Mandelstam–Planck controversies. They conduct what the philosophy of science often calls the "rational reconstruction" of these facts. But they do not fix their methodology: They treat their rational reconstruction as if it would be direct and naïve description of historical observations. Succinctly stated, they presented their description of the historical situation as a description of reality.

In essence, the methodology of Mandelstam's biographers is the following: Mandelstam is right in his criticism of Rayleigh and Planck. This methodology may be expressed by N.D. Papalexy's statement: "In this important research the cardinal question about the reason of turbidity of a homogeneous medium was posed. Leonid Isaakovich showed the fallacy (or as he preferred to say "insufficiency") of the Rayleigh theory of molecular scattering that was universally recognized….

In close connection with this research, Mandelstam wrote his papers "On the theory of dispersion" dedicated to the discussion of the explanation of the attenuation of light as it passes through a substance on the base of light scattering. Such an explanation was proposed by Max Planck in his theory of dispersion. L.I. showed that M. Planck's model is not able to give an explanation of the attenuation of a transmitted light wave. L.I. conducted calculations which showed that the point is the difference in damping of isolated oscillators and oscillators constituting a homogeneous medium. This difference escaped Planck's attention, but was demonstrated by L.I., who brilliantly managed with the all circle of problems in the theory of oscillations" [1, Vol. 1, pp. 15–16].

How was this history represented? As was noted (Sect. 4.2), this presentation was amusing. M. Planck's postcard was cited in the biography of L.I. Mandelstam. In this postcard, Planck agreed with Mandelstam and wrote that he made a corresponding correction.

The biographers admitted a lack of coordination. L.I. Mandelstam's article "On the theory of dispersion" dedicated to the criticism of M. Planck's theory was published in 1907. Planck's card is dated by 1904. Probably, it was M. Planck's reaction to some unpublished Mandelstam's address.

Mandelstam's biographers described how L.I. Mandelstam criticized M. Planck. However, they had not described, had not even mentioned two papers by M. Planck written in reply to Mandelstam. True, a reader of the first volume of L.I. Mandelstam's "Complete Works" would see Mandelstam's references to these papers. But he/she would judge on these papers by Mandelstam's quotations.

How to explain such an approach? First, Mandelstam's biographers were representatives of a scientific school, the scientific community which was engaged in competition with other communities. Especially, this competition was important for young scientists.

It is possible that the story with the Noble Prize for the discovery of combinational scattering of light told in favour of interpretation (see Chap. 7).

It is possible that the ideological situation in the USSR influenced such an approach: "our Soviet science" was treated as "progressive", "leading".

However, one more reason should be taken under consideration. Like all the Soviet citizens, Mandelstam's collaborators and disciples lived within a framework of the myth about socialism and communism. The present author does not think that they really believed that the Soviet people went to communism as to a happy future. The majority of them seem to take communism as an abstract idea. Nevertheless, mass media, meeting, conversations bore ideologically loaded expressions. The language, which is the "house of being", was penetrated by mythology. Mandelstam's biographers wrote in the belief that there exists a general line in the development of knowledge. They wrote in such a style because this style was in common use.

Let me cite several sentences which are constructed in the style of Soviet ideology:

"The most powerful elucidation of all area of the radio-interferential ideas has been given in the L.I. report delivered for the 28 April 1938 general meeting of the Academy of Sciences In these papers the irrefragable elucidation of the relation of radio—interference with optical interference was given" [1, Vol. 1, p. 38].

"The school of physicists which arose within this Moscow period of the L.I. activity is remarkable not only for its brilliant penetration into physics, but its skillful operation with the contemporary conceptual technique, its concrete applications, but also physical logical thought, correct formulation of the physical problems, the ability to separate essential from nonessential" [ibid., p. 26].

"The complete overall clearness and clarity in the interpretation of quantum theory" [ibid., p. 52].

"This difference escaped Planck's attention, but was demonstrated by L.I., who brilliantly managed with the all circle of problems in the theory of oscillations" (it was quoted at the beginning of this section).

"Myth is already enlightenment, and enlightenment reverts to mythology", T. Adorno wrote. Progress in science as it is expressed in everyday language which is understandable for public turns out to be a kind of mythological line directed to perfect knowledge. Homogeneity of key positions became the main value in the presentation of scientific results.

However, researchers themselves need to use the elements of such a picture to catch their place in the world. Mythology is located in the very self-consciousness of people of science. In the opinion of Mandelstam's community, Mandelstam had no right to make a mistake.

Additionally, common sense of Mandelstam's disciples was penetrated by the ideology of progressivity proper to Soviet ideology.

Chapter 5
The Years of Pilgrimage (1914–1925)

5.1 Petrograd: The Absolute Method of Calibration of Cymometers

It was mentioned in Chap. 2, that on 1.08.1914 Mandelstam together with his wife and son arrived in Odessa. N.D. Papalexy's "Short Outline" says that at the end of 1915 L.I. Mandelstam with his family, after a short period of work for Novorosiisk University, moved to Petrograd. As Mandelstam writes in his autobiography, "since December 1915 till September 1917 I worked as a consultant at Siemens and Halske radiotelegraph plant in Petrograd".

One of Mandelstam's results was the "absolute method of calibration of cymometers". Cymometers are calibrated by the tuning in resonance with some variable circuit of which the calibration is known. However, in which way to calibrate this circuit? Mandelstam proposed an aperiodic circuit regularly interrupted by a buzzer.

Russian engineer J. Tykocinski-Tykociner, who was a manager at Siemens and Halske,[1] wrote in "PhilosophicalMagazine" (Vol. 39, 6 Series, January–June 1920) [346]:

> On the Mandelstam Method of Absolute Measurement of Frequency of Electrical Oscillations.
>
> During the summer of 1915, a considerable number of wavemeters for radio stations had to be calibrated in the Radio Department of the Russian Siemens and Halske Works in Petrograd taken under the control of the Russian Government.
>
> To enable a larger number of stations to work with each other without interference, not only sharp tuning to be applied but the precise setting of the radio apparatus for a given wavelength is of paramount importance. A reliable method of wave measurement reduced to the use of the simplest standard becomes of great necessity.
>
> Dr. Mandelstam, Chief Expert of the Works' Research Department, investigating the behavior of high-toned buzzers used at that time for generating high-frequency oscillations, for measurements and testing purposes, found that oscillations in a circuit energized by a buzzer

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[1] Papalexy twice mentions him in his biography of L.I. Mandelstam. There is an article about him in Wikipedia. It says that J. Tykocinski-Tykociner is Polish engineer.

A. Pechenkin, *L.I. Mandelstam and His School in Physics*,
https://doi.org/10.1007/978-3-030-17685-3_5

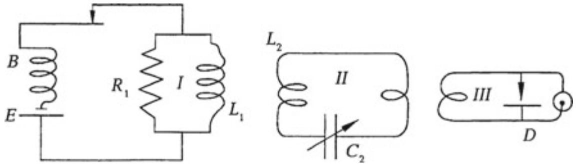

Fig. 5.1 Mandelstam's scheme of absolute measurement of frequencies in the Tykocinski-Tykociner's paper

> do not depend solely on its capacity and inductance, but depend also upon the frequency of the pulsating current delivered by the buzzer, and to a large extent upon the character of interruptions.
>
> Mathematical analysis discloses that a buzzer… can be made, in connection with another circuit, a source of trains of oscillations possessing a wide scale of frequencies. The amplitudes of the variety of oscillations obtained are not equal for all frequencies, but depend upon the ratio of the frequency of generated oscillations to a number of interruptions per a second the buzzer is operating. Those oscillations, the frequencies of which represent exact multiples of the number of buzzer interruptions, have the largest amplitudes.
>
> Basing himself on this result, Dr. Mandelstam devised (July 1915) and developed the following method of absolute measurements of frequencies used in radio work.
>
> The buzzer B giving regular interruptions and working from a battery of accumulators E excites an aperiodic circuit *I* consisting of a resistance R and an inductance L. This circuit is a source of oscillations of all possible frequencies in accordance with Fourier's analysis of the curve into sinusoidal components. Another circuit *II*, capable of performing free oscillations, with its variable capacity C_2 and inductance L_2 is inductively connected with the generating circuit *I* and with the circuit *III* containing an indicating instrument D, as for instance a thermoelement with a galvanometer or a detector with a telephone (Fig. 5.1).
>
> By variation of the capacity of the condenser C_2, a great number of maxima of the oscillating currents in *II* can be observed, arranged in definite positions all along the scale of the condenser C_2. Changing the number of interruptions per second of the buzzer produces the effect that the maxima come closer to each other, if the number of interruptions decreases, or become widely separated if the number of interruptions increases. The use of a detector with a telephone in the indicating circuit *III* coupled with *II* gives a means of hearing a pronounced musical tone, corresponding to the frequency of interruption of the buzzer only in position of the condenser C_2 which form circuits of multiple natural periods to that of the period of interruption of the buzzer. Every position of the maxima on the scale C_2 defines thus the frequency of a certain harmonic of an oscillation, whose fundamental is given by the number of the buzzer interruptions per second [346, pp. 289–291].

The circuit *II* represents the wave meter which should be calibrated.

We shall not go into further technical problems.

Engineer Evgenii Iakovlevich Shegolev (1893–1956), who made acquaintance with Mandelstam and came to corroborate with him in the prerevolutionary years, writes that Mandelstam also solved other problems which arose at Siemens and Halske plant.

> Along with the principal problems, Mandelstam solved a number of routine problems. For example, on the reason of the collapse of cooperation with Germany, the plant had not thermal wattmeters which were applied for cymometers as indicators and delivered from Germany. It was not possible to organize the production of such sophisticated apparatus then. On the other hand, the indicators of resonance were required, the indicators measuring

> not only frequency, but also the logarithmic decrement of damping. Leonid Isaakovich found the simplest way out: the lamp of a usual pocket torch turned out to way out. It was used as a bolometer.[2]
>
> It was included into one of the branches of Wheatstone bridge, balanced at a lack of oscillations. As a current appeared, the balance was broken and galvanometer needle deviated. This simple indicator, a small measurement apparatus in essence, made it possible to conduct all required measurements.
>
> Approximately at the same time, Leonid Isaakovich constructed the apparatus to increase noise immunity of radioapparatus [310, p. 177].

5.2 Tbilisi (Tiflis)

Mandelstam writes in his autobiography (the beginning of this was cited in the first paragraph of first chapter). “In summer 1917, I was elected as Professor of the Department of physics at the private Politechnical Institute in Ekaterinoslav (since 1924, this city is named Dnepropetrovsk) and on the same summer as Professor of the department of Physics at Tiflis Polytechnic Institute. On July, I was approved as Professor Ordinarius by Ministry of People Education. I hold this position now. In autumn, this year I was elected as Teacher of physics by the counsel of Tiflis higher woman courses”.

In Ekaterinoslav, Mandelstam had never worked. He was only in correspondence with the head of the private Jewish polytechnic institute which was formed there. In Tbilisi, Mandelstam remained until the autumn of 1918. However, next to nothing is known about his work there. There is a letter of Tbilisi Mayor, who was also the head of committee which run the formation of Tiflis polytechnikum (13.2.1918). Mayor asked which space was required for the Mandelstam chair. There is also Mandelstam’s short reply.

The October Revolution caught Mandelstam up in Tbilisi. However, in Autumn 1918 in Georgia Menshevist government came to power (Russian Social Democracy had two fractions: Menshevists were less radical than the Bolshevists, guided by Lenin and Trotsky.). This government existed till 1922.

5.3 Odessa

As N.D. Papalexy writes, Mandelstam had moved to Odessa in autumn 1918. Mandelstam participated in the organization of Polytechnicum, where he took the chair of physics. Mandelstam assembled a group of talented scientists among which there was Igor Evgenievich Tamm, Graduator of Moscow State University and the Nobel Prize winner in the future (Tamm was introduced to Mandelstam by his uncle A.G.

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[2] A bolometer is a device for measuring the power of incident electromagnetic radiation via the heating of a material with a temperature-dependent electrical resistance.

Gurvich, who was mentioned in Chap. 2). The above-mentioned E.Ia. Schegolev joined Mandelstam's team, too. Papalexy himself was in Odessa then and worked for the chair headed by Mandelstam. The vacuum laboratory was organized at the chair, and radiolamps were produced by this laboratory.

As is said in I.M. Volkova's commemorations,[3] "in 1919 in Odessa the radio telegraph plant was organized. The main target was to repair radio stations. However, at the same time research was conducted in the laboratory of this plant. In particular, lamp and arc transmitters were under studies. According to Mandelstam's and Papalexy's ideas the method of absolute calibration of wave meters was elaborated and applied in practice. It was organized the production of radiolamps (R-5 and more powerful till 10 w). The vacuum problems were studied by the people who worked for Polytechnicum" [364, p. 211].

The question who was in power in Odessa then is not articulated both in Volkova's commemorations and Mandelstam's biography published in the first volume of his "Complete Works". But what political situation had occurred in Odessa at that time?

Let us cite one of the fresh historical chronicles [311, pp. 29–30].

1918, March–December, The power of Ukrainian Sovereign Rada headed by Hetman Skoropadansky and Austria-German occupation.
1918, December–1919, April—military intervention of the Entente, the French area of influence,
1919, April–August—Soviet Power.
1919 August–1920 February—the power of armed forces under the command of General A.I. Denikin.
1920 February—the final establishment of Soviet Power.

Already in 1919, the Extraordinary Anticontrrevolution and Antisabotage Committee developed the great terror in Odessa. On the yard of the Committee building, 50 arrested people per night were executed. The Red Terror lasted through the 1920s.

The events of the Civil War were described by Ivan Bunin in his diary "Damned days" ("Okoiannye dni"), the 1920s Red Terror in Odessa is described by Bunin's disciple Valentin Kataev in his novel "Werther has already been written". (Kataev has entitled his novel by a line borrowed from Boris Pasternack's verse (1918): "Werther has already been written. Today the air smells of death")

As "Radenaska enciklopedìe istorıì Ukraìny" (Soviet Encyclopedia of the History of Ukraine) writes, Odessa Polytechnical Institute was established in September 1918 when Odessa was under the power of Ukrainian State Rada supported by German militaries. Certainly, an institute could not be organized within one month, and its formation took several years. It seems to be financed not only by Rada, but also by occupants, White Army and Soviet Power.

In which way Mandelstam and his family survived through these perturbations? The role of fortuity is great. However, there is another answer. Both White and Red, and both nationalists and internationalists felt the power of radio.

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[3] I.V. Volkova is a journalist who in coauthorship wrote a book on the Central Radio Laboratory in Leningrad.

The Reds got aware of the importance of radio very early: First, they took under consideration the military importance of radio, and then they became aware of its ideological function, the function as a "meeting of millions" (see [144]).

Odessa engineer and physicist B.A. Minkus (he was born in 1904) gave some information on the organization of Polytechnical Institute. Minkus points to the initiative of Odessa Technological Society and Union of Engineers. This initiative was supported by some groups of professor, engineers, and students who emigrated to Odessa in 1918. Minkus cites the list of members of the Soviet of Trusties. Mandelstam was among them.

B.A. Minkus describes the beginning of his studies at Odessa Polytechnic Institute and his first impression of Mandelstam as Lecturer [235, pp. 163–165]. He outlines a literary portrait of Mandelstam as Teacher.

> The first year of our studies consisted in study of elementary mathematics. However we (student Eugenie Bardakh and me) made our mind to attend the lecture of Professor Mandelstam. To prepare myself I got H. Lorentz' text- book. I bought the first volume at an absolutely deserted bookshop, the second at an empty apartment of the typhus teacher.
>
> Leonid Isaakovich delivered his lectures in the large hall which belonged to the Institute of Noble Girls before. By taking convenient seats among students sitting crowded together, we tensely anticipated the appearance of the legendary professor. A rather untall man with elegant grey suit on and pleasant face somehow imperceptibly appeared at the lecturing desk. The professor quietly took off his pincenez and carefully wiped it. The assistant, who followed him, came to the laboratory table which was staying in the middle of the scene. By calm, quiet voice the professor addressed himself to the audience. He proposed the audience to share his considerations concerning the status of physics among the sciences which we should study within the next few years. By speaking on initial concepts Leonid Isaakovich emphasized that they cannot be defined and cannot be explained in words. The students were impressed by the lucidity of his mind and proudly joined the ideas presented by the professor.
>
> In the course of lectures the feeling of the coparticipation with the famous scientist in the cognition of physics laws increased among students. By delivering his lectures Leonid Isaakovich sometimes left the desk and came to the blackboard to draw carefully a well thought-out formula. Assistant K.B. Romaniuk, who accompanied Leonid Isaakovich, demonstrated the experiments which required the great skill. Leonid Isaakovich called his lectures "talks". In these "talks" he included some stories from the life of Kepler, Newton and other great physicists. These stories were taken from the original writings rather than from textbooks. These original writings were well known for L.I. Mandelstam.
>
> Time passed. It was a very cold winter. In the nonheated auditorium students were sitting in greatcoats, padded jackets, caps on and were frozen. Once by delivering a lecture Leonid Isaakovich interrupted himself and suddenly loudly said: 'you have caps on, this is a lack of respect to the desk, audience, lecturer at last, take off your caps!" The professor was angry, he himself did not have his cap on. Undoubtedly such an episode was the first in the life of the great physicist, and, I think, the last. After a pause, Leonid Isaakovich turned to Molière's words which are well known for him:
>
> And as a learned man remarked one day
> Most aptly 'tis the Tower of Babylon,
> Where all, beyond all limit, babble on.

Leonid Isaakovich continued to deliver the lecture. At the end, Minkus writes that as Spring is coming closer, the feeling of attachment to this marvelous person and lecturer increased. Mandelstam became the favorite professor of our course.

5.4 The Central Radio Laboratory

In summer 1922, L.I. Mandelstam became a scientific consultant at the Central Radio Laboratory of Trust which run the mills of weak current. This laboratory is located in Moscow, but at the beginning of 1924 it moved to Leningrad.

The Central Radio Laboratory was an establishment that resulted from the scientific and technological policy of the young Soviet State. "The establishment of the State Electrotechnological Trust controlling the mills of weak currents marked the end of the period when instead of the central State radioindustry, there were uncoordinated local industrial and research enterprises which were oriented to narrow and local requirements and resulted from the "present situation". After a long break induced by the civil war and its consequences, it was necessary for the State to develop the self-supporting production of radioapparatus corresponding to the level of science and technology of that time. The trust was facing an acute problem to organize a powerful research laboratory corresponding its purpose and aim within its framework.

The new scientific research laboratory should formulate and solve a wide scope of theoretical and applied problems. This laboratory should provide the application of the achievements of science to production, the application demanding minimal cost and time. Eminently qualified specialists were needed for such laboratory, such specialists existed, but they were scattered along different regions and establishments" [347, p. 69].

L.I. Mandelstam and N.D. Papalexy who lived in Odessa, Dmitriy Apolinarievich Rozhansky (1882–1936), who lived in Nizhny Novgorod and worked for the radiolaboratory headed by M.A. Bonch-Bruevich, were such eminently qualified specialists. All of them were invited to the Central Radio Laboratory. Mandelstam and Papalexy started their research there in Summer 1922 when this laboratory was in Moscow (the laboratory attached to the work in Shabolovka street). D.A. Rozhansky joined them in Leningrad.

What was Mandelstam's subject in the Central Radio Laboratory in 1923–24? The biography of L.I. Mandelstam in Mandelstam's "Complete Works" tells next to nothing about it. However, there is I.G. Freiman book which says something about Central Radio Laboratory (first edition appeared in 1924). This book says that Mandelstam and Papalexy were in particular concerned with the problems of modulation. As is well known, radiobroadcast is provided by modulation of the carrier signal. As Mandelstam said, "without modulation we can only state that the broadcasting station either works or keeps silence" [1, Vol. 3, p. 158]. In broadcasting, modulation usually consists of changing the amplitude in the tempo of sound oscillations in broadcasting conversation.

From the point of physics, one distinguishes amplitude, phase, and frequency modulations. However, from the point of engineering it is important which process provides modulation in the transmitter. For example, one distinguishes anode and grid modulations (it is clear that the lamp transmitters rather than the spark ones are under consideration). “The invention of grid, Mandelstam said on delivering his lectures, was a step of great importance. First of all the moving of electrons is influenced by the field which is about the cathode (filament). The anode is charged positively. The grid is close to the cathode. It is enough to give small potential to the grid to obtain strong field nearby the cathode. By changing the grid voltage we strongly change the anode current” [1, Vol. 4, p. 119; 2, p. 221].

Usually to produce modulation, besides the generative lamps, the modulatory lamps are in usage.

The modulation of independent excitation is also under consideration. This is the modulation produced by a special driving generator. The main more powerful generator is excited by the modulated oscillations.

“The modulation by excitation, I.G. Freiman writes, can be provided by another way (besides this scheme of an independent excitation). For example, it is possible to introduce an iron-core coil into the grid circuit and to produce the different excitations by the direct current of biasing.

This method is a special case of the general one. Mandelstam and Papalexy proposed the method consisting in changing the parameters of the grid circuit for the high-frequency current. The voltage V_c, exiting the grid can be represented as V_c e_c $Z_c i_c$, where e_c is an internal electromotive force given to the grid circuit, $Z_c i_c$ is a voltage drop across it. The above modulation principle is based on the changes of an apparent resistance of the grid circuit Z_c. In general by changing Z_c one changes the tuning of the grid circuit” [121, p. 239].

The research by Mandelstam and Papalexy and their students in parametric resonance can be traced back to the above consideration of modulation. From a mathematical point of view, both modulation and parametric resonance are described by the differential equations with periodically alternating coefficients.

In 1923, L.I. Mandelstam was sent by the Trust of weak current on a mission to Germany. There he met Richard von Mises. The following section will tell something about it.

5.5 What We Learned About Mandelstam’s Life from His and His Wife Letters to Richard von Mises

As the present author promised (see the Introduction), Mandelstam’s and his wife’s letters to Richard von Mises are an invitation to elucidate Mandelstam’s life and creative work.

The first letter which is available for us was sent 30.10.1918 from Odessa, which was occupied by German troops then. Mandelstam writes: “There is next to nothing to say about us. Things were going almost good all the time. We lived about two years in Saint Petersburg, then in Tbilisi where I worked for Higher Technological School. Now I have been invited here to work for the recently formed polytechnic school". Let us recall that in Saint Petersburg since the end of 1915, Mandelstam worked for Siemens and Halske mill taken under control by the Russian government.

Mandelstam also writes that he has conducted almost no scientific research. There were many reasons. "The situation is bad with the books and especially magazines, he continues. I almost do not know anything, that during this time has been achieved. It is not pleasant to be without laboratory and literature".

Mandelstam wife's letter is written in another manner (1.11.1918):

> Dear Mis,
>
> I ask you to write to us from time to time. You will not believe how your April letter made us happy. We have received it when we came back here from Tbilisi. We spent one year in Tbilisi and we had not a single letter from here where I left Bubi.[4] Three years ago we were two months in Petrograd, where I passed through the State Exam to take with something time. Since that I work in surgery without big successes for me and my patients. Lenia will describe the serious page about our life, but I am writing about the inner. Now I have everything, but we lived very hard during this war. I became less sensitive, receptive. I think that this stupefied is not nice and I have not passed success fully through my humane exam. What I know about you (it is regrettably not much) evidences that you are energetic, as earlier. This makes me happy. If you have time, write me, what do you think about Strasbourg people. Lenia often wrote to Strasbourg, but I seem to want too much from people.[5] I find that Odessa's life is so-so. I liked Saint Petersburg very much at the beginning of 1917. However it got bad later there. It was hard time. In order not to be too lyrical, I am finishing I am afraid that my later will be similar to my usual letters to Strasbourg.

The 24.09.1921 letter was already sent by Mandelstam from Odessa where the Soviet power had been settled. He again writes that he does not have the ability to conduct research (both experimental and theoretical). However, since this letter the theme of departure abroad for the productive research work has appeared. "It is not conceivable to you which great interest has been aroused by your inquire and how I should be happy if your contacts with Einstein come to a positive result. One of my great desires would be granted".

The letter of Einstein of 27.1.1913 to Mandelstam evidences that he knew Mandelstam. This letter has been cited in Chap. 4.

The present author, however, has not been able to obtain information about whether Richard von Mises' conversation with Einstein about Mandelstam took place. The historians of science, who study Einstein's creative work (Boston University, USA), were not able shed light on this problem.

L.I. Mandelstam's 12.01.1922 letter (from Odessa) shows that he took steps toward the departure:

> According to our own estimation, the thing is going as follows. Once it's your letter, I immediately began looking for ways and has taken some steps to follow your friendly

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[4] Sergei Leonidovich Mandelstam, Mandelstam's son.

[5] L.I. Mandelstam probably wrote his Strasbourg friends and colleagues. Sometimes this communication had not justified hopes.

> advice, relevant to our own will. I spoke about it with a local German representative in the organization to help prisoners of war. He promised to help me. With all the formalities that must be met by the German side, he advised me to first perform all the local formalities. This is not over. It turns out that even if it is on the delegation, it is time-consuming, since it can not be solved on the spot. //But I still try to do all in my ability to achieve the goal, and I hope it succeeds, if not immediately, then sometime in the foreseeable future.
>
> Anyway I shall try… As I learn something definite, I let you know and in general I shall keep you informed.

L.I. Mandelstam also writes about his possible departure in his 18.06.1922 letter:

> I turn to the question, which for me is the main one now. It seems now the prospects of departure from here will be better, as the administration is easier to give the necessary permission, and our problem is becoming more visible and more concrete.
>
> Given the fact that since your letter much time has passed, please, explain if can I understand your letter so that I could expect an academic position in Germany, or just talking about the possibility of a job at a technical firm that knows that my interests lie in science and that I regard technology as an external condition.

In L.I. Mandelstam's letters to Richard von Mises, there is the theme of the availability of scientific literature. As early as 1918, Mandelstam wrote Richard von Mises about his shortage of books. However, at the beginning of the 1920s an opportunity occurred to post books to Russia. L.I. Mandelstam wrote Richard von Mises on 24.09.1921:

> As I could find out, there is the only opportunity to send books, namely, by post to Polytechnic Institute and addressing to my name. As I am not convinced that this way is reliable, I ask you to send a trial parcel. I should like to receive H. Weil "Space, Time, Matter" and H. Möller "Electronic tubes" I heard from a Moscow colleague that your various things—about the theory of probability, about the problems of flight—had been published. It is not necessary to explain that I am interested in these works and I would be very grateful if you send copies of them to me. If our test is successful, I would take journals first of all. Send me physical and mathematical literature according to your test. However, let's first see whether a trial parcel will reach me.

At the beginning of the 12.1.1922 letter, L.I. Mandelstam writes: "Hearty thanks for your friendly letter which I have received in proper time (two copies—this means that Mandelstam and Richard von Mises duplicated their letters since they did not trust the post service—A.P.). I am also very grateful for a copy of your journal which gave us much pleasure. I like very much your fine work about iterations".[6]

In a few lines concerning a job in Germany, Mandelstam's letter says:

> I should like to thank you for your troubles concerning journals and books.

First of all I should like to have the following books:

> Hermann Weyl. *Space, Time, Matter*;
>
> Max von Laue. *The theory of relativity*. Vol. 1 and 2 (especially I need in the second volume);
>
> Hans Georg Möller. *Electron tubes*;

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[6] Mandelstam had in mind the article "Das Problem der Iteration", published in: *Zeitschrift für angewandete Mathematik und Mechanik*, Bd. 1, 1921, S. 298–307.

> Arthur Haas. *Introduction into theoretical physics*;
>
> R. Fürth. *Oscillatory phenomena in physics*.
>
> I would be very grateful if you add something according to your choice.

L.I. Mandelstam would like to receive the following books:

Hermann Weyl. *Raum, Zeit, Materie*. Vorlesungen über allgemeine Relativitätstheorie. Berlin: Verlag von Julius Springer, 1919;

Max von Laue. *Die Relativitätstheorie. Erster Band. Das Relativitätsprinzip der Lorentz-transformation. Vierte vermehrte Auflage* (Friedr. Vieweg & Sohn, 1921); Zweiter Band: Die allgemeine Relativitätstheorie (F. Vieweg & Sohn, 1922);

Hans Georg Möller. *Die Elektronenröhren und ihre technischen Anwendungen*.

Braunschweig: Druck und Verlag von Friedr. Vieweg & Sohn, 1920;

Arthur Haas. *Einführung in der theoretische Physik*, Bd. 1, 2. Berlin [u.a.]: de Gruyter;

Reinhold Fürth. *Schwingungserscheinung in der Physik*. Braunschweig: Vieweg und Sohn, 1920.

In his 18.07.1922 letter, Mandelstam writes:

> I am very grateful for what you have done for us. I got the Sommerfeld and before that both of your papers, which I am very interested in.[7] Recently I took your theory of flight and now I am reading it with great pleasure.[8] I am sorry that the other books have not come. I think that the bookseller has sent them without an inventory. Sommerfeld's book came as a registered packet. The local experience shows that the registered parcels have been very rarely lost, but undocumented ones often.

Richard von Mises sent not only books. In 1922 when food supply was very poor, Richard von Mises sent food parcels to Mandelstam and organized parcels from the philanthropic organizations. The following extract from Mandelstam's 18.06.1922 letter says something about it:

> We got a 10 dollar package "ARA" from you, as well as two similar packages from Holland. These parcels were the great support for us at that difficult time. Only I doubt that your other parcels will arrive soon. I also got a nice letter from Mr. Linz. Once again, many thanks. Do not act now on in this direction. We are now fairly well provided of the products.[9]

In Mandelstam's 23.01.1923 letter, there is a penetrating phrase: "Now I see clearer than before: the fact that we all lived through these black times, we owe largely to you".

Mandelstam's letters show that Richard von Mises arranged the financial affairs of the family. He received money for them. Mandelstam asked to put the money in

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[7] One of them was about the frequency theory of probability.

[8] *Fluglehre. Vortrage über Theorie und Berechnung der Flugzeuge in elementarer Darstellung*, 1 Aufl., Berlin, 1918; 2. Aufl., 1922; 3. Aufl., 1926; 4. Aufl. 1933; 5. Auflage (with Hohenmesser). A Russian translation of the first edition was published under the title "The basic principles of aeronautics" in 1926 (Moscow). The collective of translators under the leadership of P.P. Sokolov [367]. There is a publication of the second edition (Leningrad, 1926). The editor is V.A. Rynin.

[9] ARA is decoded as American Administration of Assistance. About the agreement of the Soviet Government with ARA see [203].

the savings bank. Of these, he asked to pay the cost of the books. Whether it was money received for the property left in Strasbourg, or received for Mandelstam's patents and inventions, is not known.

In 1923, L.I. Mandelstam worked as a consultant for the Central Radio Laboratory belonging to the trust of works of weak current. As Mandelstam's letters show, for Mandelstam a reason to accept this position was a possibility to have a business trip to Germany. This trip occurred on March–May 1923. Mandelstam was planning to travel together with his wife and son, but he traveled alone. On 9.03.1923, Lydia Solomonovna wrote Richard von Mises:

> My husband and Papalexy will start for Berlin on 13 or 16.
>
> My husband is leaving in Berlin for 2 or 2,5 months. I think that everything will be good with an apartment, since he has friends in Berlin. But I think, that first he will go to a Sanatorium to have a rest. His nerve are weakened, he became a hard neurasthenic. I think that nearby Berlin there is something. Today I shall write Rohman and shall ask him to prepare. Without such a rest he will not able to work and to live in general. This is very serious, but I am afraid that he will not do it, although he promised to me.

It is not known whether L.I. Mandelstam visited a sanatorium.[10] However, judging on recollections, the trip was productive. N.D. Papalexy writes about Mandelstam's meeting with Einstein [1, Vol. 1, p. 26]. L.I. Mandelstam did not meet Richard von Mises on his arrival. When Mandelstam arrived in Berlin, Richard von Mises was visiting his ill mother in Wien. But later they met, and Richard von Mises helped Mandelstam to organize the scientific part of his visit.

On arrival, L.I. Mandelstam sent Richard von Mises the following letter (20.07.1923, Moscow):

> Dear Friend,
>
> Every day I wanted to write you, but here one never makes what he wants to do.
>
> In general I need to say that what I said about the paradise is true more than I thought.
>
> Hearty thanks for what you did for me. Till now inwardly I live there and mostly with you. Now I understand clearer how many efforts you devoted to me.
>
> Here the things have not practically changed. From Berlin I directly went to Odessa and stayed there for four weeks. I don't need to deliver lectures. Only a few. The institute by itself is very good. But now there is a lack of many things. No gas, etc. And almost no people with whom it is possible to speak about something.

In 1925, the tone of Mandelstam's letters to Richard von Mises changed. In 1925, he accepted an invitation of Moscow State University (there was the name then: First MSU—in contrast to Second MSU which became Lenin Pedagogical Institute later). He became Professor at Physico-Mathematical School (Department) and Acting Member of Scientific Research Institute of Physics belonging to this Department.

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[10] E.L. Feinberg writes about L.I. Mandelstam's hard neurasthenia. He also writes that Mandelstam had control over himself and his colleagues had not observed his neurasthenia [103, p. 19].

5.6 Mandelstam and I.E. Tamm

As was noted in Sect. 5.2, in Odessa Mandelstam became to collaborate with I.E. Tamm. Tamm's letters to his wife Natalia Vasilievna show that this was not merely cooperation, but that Mandelstam's and Tamm's personal trajectories became very close.

As was noted above, Mandelstam placed Tamm to teach at Odessa polytechnic school. Mandelstam and Tamm drew together not only on the base of teamwork, but also team catastrophe. In 1922, Mandelstam and Tamm shared one apartment. Tamm, whose awkwardness was noted in a number of recollections, committed an explosion of a big can of kerosene.

As Elena Solomonovna Billig, Mandelstam wife's sister, wrote, the main victim was L.I. Mandelstam, who received burns and was in bed for three weeks. Material losses were considerable, too. However, I.E. Tamm himself mostly suffered from the offence. To calm him down, people (first of all Leonid Isaakovich himself) merrily discussed the accident and tried to treat it as a deliberate crime committed by the cold-blooded malefactor [40, p. 34].

I.E. Tamm writes in his 20 January 1922 letter.

> Yesterday, it happened what could result in (and only by the concatenation of circumstances has not resulted in) a terrible catastrophe. Leonid Isaakovich and Alexander Solomonovich[11] were sitting in my room, and on my fall, the 8 pounds kerosene can has exploded. Leonid Is. got the burns of hands and foots. On blisters, he is not able to wear shoes.

In his 28 January 1922 letter, Tamm informs that he was dismissed from Odessa polytechnic school. "Farewell, the hated mill", Tamm wrote about the radio lamp mill which was mentioned above [171, p. 266].

In 1922 November in Moscow, Tamm was waiting for Mandelstam's arrival. "For 2,5 days I have received so many experiences that I could write a whole book. The main point, however, that I have two disappoints. First, Mandelstam not only has not come, but his flat has not been prepared, moreover he has not received the final invitation. According to the trust opinion, he will come in the middle of January. This means that he will at best arrive at the end of the month" [171, pp. 267–268].

I.E. Tamm literally counted days until Mandelstam's arrival.

Tamm also says that he is invited to teach at Sverdlov University and the demand of him to teach physics from the position of materialist philosophy. Tamm writes that he treats social matters from the position of materialism. "But physics is just a science, and I can not understand what is materialism in exact sciences" [171, p. 268].

In his 11 November letter by providing a brief survey of Moscow physics, Tamm claims that "except Leonid Isaakovich there is nobody here to train physics" [171, pp. 269–270].

In his 12.11.1922 letter, Tamm shares his impressions of N.P. Kasterin's lecture addressed to the colloquium in Institute of Physics and Biophysics belonging to the

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[11] Alexander Solomonovich Isaakovich is Lidia Solomonovna's brother.

Ministry of Public Health. Kasterin was Professor of Moscow State University. This institute was headed by P.P. Lazarev, who was a representative of P.N. Lebedev's school.

The lecture was dedicated to physics of either. Tamm writes that he attended such a lecture in Odessa. "Unfoundedness, elementary mistakes". "I had a conversation about it with Leonid Is. He completely agreed with me" [171, p. 271].[12]

Tamm's 14 December 1922 letter touched upon the same situation as about which L.I. Mandelstam and L.S. Mandelstam wrote to Richard von Mises [171, p. 273]:

> Mandelstam left for Peter for one week. Lidia Solomonovna says that his nerves became sickly, she worries and thinks about a sanatorium- preventorium. They have not come to the decision if they stay here, come back to Odessa, leave for Germany (perhaps already in January). By the way, Leonid Isaakovich's disquiets for any Bolshevistic became painful (although his personal situation is OK). According his own words, to be with a communist at table (in divers places and without any communication) produces strong headache for him, although this communist, as Leonid Isaakovich cer tified, was very polite. It would be very hard for me, if Leonid Is. is leaving. A day before yesterday I did not meet him, I became very upset because I had a number of questions.

By claiming that L.I. Mandelstam completely rejected the Soviet Power, E.L. Feinberg (see Introduction) referred to the above Tamm's words about Mandelstam's disgust for all things Bolshevistic.

In May 1925, Tamm and Mandelstam published their collaborative article "Electrodynamics of anisotropic bodies in the special theory of relativity". They cruelly and long wrote it. In 1923, just before his departure for Germany Mandelstam insisted that Tamm would not send their paper to the editors until "he would rewrite it in his own way" (23 March 1923 letter) [Ibid., p. 274]. Tamm was upset because he in essence had no publications which he could show to nominate himself for the position of docent.

In 1925, Tamm and Mandelstam became "Fellows". They came to work for the Institute of Physics belonging to Moscow State University and came to teach at Moscow State University.

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[12] Later in coauthorship, Tamm published an article criticizing Kasterin [342].

Chapter 6
Moscow State University (1925–1935)

6.1 Invitation

So, in 1925 Mandelstam became Professor of theoretical physics at Moscow State University (MSU). The protocol of the Subject Committee of Physics Department (15.05.1925) says that Prof. A.K. Timiriazev communicated that State Scientific Council had approved L.I. Mandelstam as Professor of theoretical physics [398, pp. 24–146].[1]

At the same time, Mandelstam became Full Member of Physics Institute at MSU (besides the full members, there were researchers of the first and second categories). This job was similar to that to which Mandelstam was engaged in Strasbourg. Mandelstam taught at the Physico-Mathematical School (Faculty) (in 1931 on the base of this School, the School of Physics and Mechanics was established, and in 1933 this School was transformed into the Physics School). At the same time, he organized research at the institute which was included into this School. MSU was organized on the German model: a research institute under the school.

As was noted in the previous chapter, since 1923 Mandelstam worked as Scientific Consultant at Central Radio Laboratory belonging to the Trust of the weak current works. In 1923, this laboratory was in Moscow. Afterward, this laboratory moved to Leningrad. In 1925, Mandelstam became full-time Professor at MSU, retaining for himself the position of Scientific Consultant to the Central Radio Laboratory concurrently.

Mandelstam was invited to take the position of Professor due to the decision of the Subject Committee in which students were actively involved. Among those students, there was Alexander Alexandrovich Andronov (1901–1952) who became

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[1] L.I. Mandelstam came to MSU, when there was a united Physico-Mathematical Faculty. This was a period of changes. At the beginning of the 1930s, the structure of the Moscow State University included departments: mechanical, astronomo-mathematical, physical, and zoological. In 1933, the faculty system was restored and Mandelstam again became Professor at Physics Faculty.

A. Pechenkin, *L.I. Mandelstam and His School in Physics*,
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Mandelstam's student. He represented the students who were not satisfied in the standards of teaching in theoretical disciplines at the Physics School (Faculty). In recollections, A.A. Andronov told about this invitation in the following way: "Three candidates have been identified. First was Paul Ehrenfest, in response to the invitation, he expressed his gratitude, but said he could not throw the department inherited by him from great Lorentz. Second was Epstein, he also declined. Third was L.I. Mandelstam.... We thought that Mandelstam should be the worst variant, but he turned out undoubtedly the best" (cit. on: [17, p. 116]).

As we see, the students took seriously the invitations sent to Ehrenfest and Epstein. G.S. Landsberg's 18.6.1924 letter to L.I. Mandelstam makes the situation clear. (Landsberg started to work for MSU in 1923, and he was Docent and then Professor.) Let me cite it as a whole [399, 1622-1-75]:

> Honorable, Leonid Isaakovich, I have long wanted to write you with this letter, but some vagueness of the situation kept me. Today, finally, the situation turned out so clear that I am able to write you. It is about your candidacy for the department of theoretical physics at Moscow University. You probably somehow know that your candidacy was put forward by us after the death of S.A. Boguslavskii, along with Epstein and Ehrenfest. However, till now we could not achieve the announcement of the competition: the administration supposedly for reasons of economy, refused to send to the State Scientific Counsel the request for the opening of the competition. Today, finally, in the Subject Committee meeting declared that if you, Epstein, Ehrenfest have agreed to head the department, the administration would not have objections. It is quite clear that Ehrenfest and Epstein will not come here. So the whole thing reduces to your agreement. You will probably get an official inquiry on the matter within a few days. By expressing my own opinion and the opinion of many of my colleagues at the university, I decided to write this letter to you. You certainly know the situation in the Moscow University, and you know people who play leading roles there. Therefore, the negative side of Moscow are well known to you. The other side of the case is as follows: by deep conviction of many of us, you are the last hope for improvement of the Physics Institute at Moscow University. Only the appearance of such person, as you, may initiate the formation of people willing and able to work, put an end to endless intrigue, completely permeated the entire soil of the institute. There is a considerable group of students which eager to have the real research supervisors. Despite their youth they already disillusioned with the present leaders of the Institute. The negative side of the case is, as you, of course, is well known, low payment. Perhaps you could also count on other sources, in particular, the State Publishing House. As for the apartment, then I think you could put a condition of providing you with an apartment, and I think that University is able to provide it. Sorry, I'm taking the liberty to write you all this: I am very afraid that you will immediately and resolutely refuse.

So, L.I. Mandelstam accepted the invitation. On 27 January 1925, I.E. Tamm, who became Docent at MSU in 1925, wrote a very optimistic letter to Mandelstam:

> Dear Leonid Isaakovich,
>
> I am very sorry that during your visit I was not in Moscow. Rumours about your intentions are very encouraging. I am only afraid that by coming to the decision, you will postpone its official publication….
>
> Do you know that it is assigned (already assigned, rather than it is planning to assign) 400,000 rubles to improve the status of professors? Professorial salary will be raised to 80 rubles.

With the coming of Mandelstam, the program has been corrected and the general situation has changed.

6.2 At the University

The following courses are scheduled to start in 1925–26 academic year: A.K. Timiriazev “The introduction to theoretical physics”, L.I. Mandelstam “The structure of substance”, L.I. Mandelstam “The special seminar on theoretical physics” [398].

L.I. Mandelstam delivered a lecture “The status of a theoretical physics bias” at the 5.6.1926 session of Subject Committee. Supplementary reports were delivered by I.E. Tamm, the Odessa colleague of Mandelstam, and G.S. Landsberg, who became the closest colleague of Mandelstam at MSU.

In 1926, the course “The structure of atom” was completed by G.S. Landsberg, I.E. Tamm delivered the course on the theory of electromagnetism.

In his biography of L.I. Mandelstam the following courses of lectures and seminars are indicated:

1925/26—the seminar on some questions of the theory of radiation, electromagnetic waves, and optics;
1926/27—the lectures on the theory of field and the seminar on theory of oscillations;
1927/28—the seminar on statistical physics;
1928/29—the seminar on electronic theory and the special theory relativity;
1930/31—the first part of the course on the theory of oscillations and the seminar on the theory of oscillations;
1931/32—the second part of the course on the theory of oscillations;
1932/33—the course on the selected chapters of optics (paradoxes);
1933/34—the course of the physical foundations of the relativity theory;
1935/36—the course on the theory of relativity (unfinished);
1936/37—the seminar on dispersion and adsorption;
1937/38—the seminar on some chapters of the theory of oscillations;
1938/39—the seminar on a fascicle of physical problems (Cherenkov radiation, mass–energy equivalence, etc.) and the lectures on the foundations of quantum mechanics;
1939/40—the seminar on some questions of optics [1, Vol. 1, pp. 63–64].

As early as in 1925, Mandelstam became the supervisor of four graduate students. Their names have been repeatedly mentioned in many books on the history of science. They are Semen Emanuilovich Chaikin (1901–1968), M.A. Leontovich (he was mentioned as one of the authors of the biography of L.I. Mandelstam), A.A. Andronov (one of the students who initiated the invitation of Mandelstam to teach at MSU), and Alexander Adolfovich Vitt (1902–1938).

L.I. Mandelstam (1930)

Besides S.E. Chaikin, all these graduate students graduated from MSU. S.E. Chaikin graduated from the Moscow Higher Technological School and passed through military service.

M.A. Leontovich wrote in his recollections: “Andronov, Chaikin and me are Mandelstam’s first graduate students…. This was in the autumn of 1925. I worked in different fields of physics: the theory of adiabatic invariance, the theory of oscillations, scattering by surface of liquid (this was my first work)” [204, p. 432].

M.A. Leontovich did not mention A.A. Vitt who was a graduate student of Alexander Savich Predvoditelev (1891–1973) in the beginning (Predvoditelev was Supervisor of his diploma work). Vitt joined the group of Mandelstam’s graduate students later by remaining to be A.S. Predvoditelev’s graduate student.

Leontovich was recommended by G.S. Landsberg in his 25 September 1925 letter to L.I. Mandelstam, who had not taken up the duties of MSU Professor yet. “Among our young people Mikhail Alexandrovich Leontovich is the most educated and talented. He is eager to enroll at Scientific Research Institute (Landsberg meant Institute of Physics). Apart from scientific reasons for him it is important for financial reasons: he would be able to concentrate himself on research. He could be relied upon as an active participant of your future seminars.

I am sending his Curriculum Vitae to you, in order to you would able to write a letter of recommendation in favour of him if you regard it as possible".

M.A. Leontovich was included to the staff as Researcher of second class (according to the terminology of that time this meant that he was a graduate student). On graduating (1929), he went on to be in staff of Scientific Research Institute of Physics, and in 1931 he became Researcher of first class.

By conducting his dissertation research, M.A. Leontovich closely cooperated with A.A. Andronov. In 1927, Mandelstam wrote one total reference to Andronov and Leontovich.

After working for a year for the All-Union Institute of Electrical Technology, A.A. Andronov was included in the staff of MSU Institute of Physics in 1930.

In 1929, L.I. Mandelstam wrote a letter of reference to A.A. Vitt in connection with his planned trip to Germany. By pointing out that he supervises Vitt's research together with A.S. Predvoditelev, L.I. Mandelstam claimed: "I consider that he is prepared for a foreign scientific trip. He has undoubted intellectual faculties for research, fundamental education, he can manage with mathematics and speaks good German. According to his work he is connected with radioengineering and some areas of modern physics. It is desirable that his studies would be guided by the prominent theoreticians (Sommerfeld in Munich, Born in Göttingen). Taking into consideration his mathematical talent I think that namely these first-rate physicist-mathematicians should be Vitt's scientific supervisors" [398, file 24, item 235].

The foreign trip has not taken place. In 1931, Vitt was included in the staff of MSU Institute of Physics as Researcher of first class.

New graduate students appeared. In 1934, G.S. Gorelik, who was mentioned above as one of the authors of the biography of L.I. Mandelstam, completed his graduate course under Mandelstam and became Researcher at MSU Institute of Physics. At the beginning of 1930 under L.I. Mandelstam prepared their dissertations S.M. Rytov (one of the authors of the biography of L.I. Mandelstam and the editor of the book about him), Maksim Anatolievich Divilkovskii (1904–1942), and Sergei Pavlovich Strelkov (1905–1974). In 1932, M.A. Divilkovskii wrote the following report (item 38): "I have solved three problems out of Mandelstam's problems. Leontovich's seminar has been worked out by 50 percent". In his report, S.M. Rytov wrote: "Leontovich's seminar has been worked out by 50 percent. I have solved four Mandelstam's problems" [398, 46-1-38].

In 1930, Boris Mikhailovich Hessen, the Communist Party official, who graduated from Institute of Red Professorship belonging to the Communist Academy, became Director of MSU Institute of Physics. In 1931, he was appointed as Dean of Physics Department (in 1933, when the faculty system has been restored he became Dean of Physics School). In a sense, B.M. Hessen was L.I. Mandelstam's student. Besides Institute of Red Professorship, B.M. Hessen attended University of Edinburgh, Natural Science Faculty (1913–14)[2] and Petrograd Higher Polytechnic School (1914–1916). Hessen studied statistical physics at those institutes. Under Mandel-

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[2] I.E. Tamm was a student of University of Edinburgh at the same year.

stam, he prepared the dissertation on the foundations of the probability theory and statistical mechanics.

As Director, the Marxist Philosopher B.M. Hessen superseded Viacheslav Mikhailovich Romanov (1880–1954), who was a specialist in electricity and radio-engineering (Romanov started in P.N. Lebedev laboratory). The other cadre shifts followed. S.E. Chaikin, who was Mandelstam's graduate student, became Deputy Director in 1932. In 1931, I.E. Tamm became Deputy Director.

In 1931, L.I. Mandelstam wrote in his report to the secretariat of the USSR Academy of Sciences [398, Fund 1622, list 1, number 57, item 2]: "Over the financial year I was Professor of theoretical physics at MSU-1, full member of MSU Institute of Physics, scientific consultant at Central Radio laboratory and Head of Physics Section at All-Union Institute of electrical engineering.[3]

Together both with the members of MSU-1 and the staff of All-Union In-t of Electr. worked out the problems of optics and electrical engineering. Together with N.D. Papalexy took the problems of non-linear oscillations in electrical systems under consideration and developed their application".

In this report, Mandelstam still calls himself Professor of theoretical physics. At the beginning of the 1930s, the structure of Institute of Physics has been changed. Instead of two divisions (theoretical and general physics), a system of laboratories has been established. In addition to the position "full member", the position of research supervisor has been established. In 1932, L.I. Mandelstam became Research Supervisor of the laboratory of oscillations and shortwaves. Head of this laboratory was S.E. Chaikin, who also was Deputy Director. Mandelstam also was Research Supervisor of the laboratory of optics. Its Head was G.S. Landsberg. In the same year 1932, the "shortwave part" of the laboratory headed by Mandelstam and Landsberg became a separate laboratory headed by V.I. Romanov.

Since 1932, N.D. Papalexy worked for MSU Institute of Physics by having the status of Full Member.

As was noted in the Introduction, in 1928 L.I. Mandelstam was elected as corresponding Member of the Soviet Academy of Sciences, and in 1929 he became Academician. From the very beginning of his work for MSU, he was Member of Scientific Counsel of Institute of Physics. In 1932, Mandelstam received a personal salary of 600 rubles. It was the only personal salary for employees of the institute [498, Fund 46]. For comparison, S.E. Chaikin, who combined the two posts—Deputy Director and Head of Laboratory—received a salary of 400 rubles, and G.S. Landsberg, Head of the laboratory of optics, had 450 rubles. In 1933, Mandelstam received 700 rubles for his work in Institute of Physics. As Teacher of the Faculty of Physics, he had 400 rubles, and as Academician of the USSR had also 400 rubles [498, Fund 46, list 1, file 67, Box 3].

According to the site *his.1september.ru* in 1933, an average salary of a worker was 125 rubles a month (according to the official course, this was $63.5 US). However, this figure shows lack of information. Only in 1935, the rationing system was completely

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[3] All-Union Institute of electrical engineering held an important position in Soviet technology and industry. It exists at present time. In 1929 A.A. Andronov worked for this institute.

abolished. True, foods were on sale not only by ration cards. One could also buy food at commercial shops.[4]

The beginning of the 1930s was a period of collectivization and dispossession of the kulaks, the organization of special exiles, starvation in Ukraine, Volga region, Ural.

6.3 The Characteristics of Basic Groups of Professors

In A.V. Andreev's book on the history of MSU Institute of Physics (for which Mandelstam worked since 1925), there is the following table concerning professors and researchers of this institute [12, pp. 36–38]. The date on the table is October 1929, the author is not known, but Andreev suggests that this could be a young teacher and a member of the Communist Party, A.A. Maksimov. The table is the main point of the letter, addressed to the Communist Party Central Committee (the other parties have long ceased to exist).

Some commentaries on the table. In the table prominent Russian physicists are mentioned. V.I. Romanov was mentioned in the previous chapter. Vladimir Konstantinovich Arkadiev (1884–1953) started his carrier at P.N. Lebedev laboratory where he described a number of properties of ferromagnetism in 1912–13. Kasterin Nikolai Petrovich (1869–1947), who was highly critically mentioned in Tamm's letter (see the end of previous chapter) started under A.G. Stoletov by conducting research in the field of electrodynamics and molecular physics. Vvedenskii Boris Alexseevich (1893–1969) was a specialist in radiophysics, and in 1943 he became Academician. Bachinsky Boris Iosofovich was Physicist–Experimentalist, A.I. Umov's follower.

There are biographies of V.K. Arkadiev, N.P. Kasterin, and V.I. Romanov in Andreev's book which contains the cited table. It is notable that the most loyal one, V.I. Romanov, was subjected to repression and was rehabilitated only posthumously.

As a criterion of the significance of a physicist, the table uses his international contacts. The physicist's loyalty is coordinated with his work for industry. As early as in 1929, L.I. Mandelstam was an influential figure in MSU: The table points to "The group of Mandelstam". Finally, in contrast to E.L. Feinberg's qualification of the political position of Mandelstam (see the Introduction), the table states that Mandelstam is loyal. With respect to loyalty, the group of Mandelstam as a whole is characterized rather favorably in the table.

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[4] In 1937 an artist of the Moscow Art Theater had 200 rubles a month, but the great actors had personal salaries equal to 1200 rubles.

In 1937, the director of a shop had 700–800 rubles a month, and a shop assistant had 500–600 rubles a month (according to [13, p. 502]).

The prices were the following: kilogram of wheat flour—4 ruble 60 kop., buckwheat—1 ruble 82 kop., a can of sardines—4 rubles 75 kop [13].

1. Romanov V.I.	Well-known physicist. Some of his researches are known abroad	Loyal. Member of Moscow Soviet. A member of the section of scientist
2. Iakovlev K.P.	Next to nothing as physicist	One of the Black Hundreds
3. Kasterin N.P.	Well-known physicist	One of the Black Hundreds. In 1922, he was expelled from the USSR together with the group of reactionary professors
4. Potapenko G.V.	Mediocre physicist	Loyal
5. Kaptzov N.A.	Mediocre physicist	Politically inert
6. Zernov V.D.	He is next to nothing as scientist	Anti-Sovietic. For political reason, he has been eliminated from Saratov University
7. Mlodsievsky A.B.	Mediocre physicist	Loyal
Group of Arkadiev		
1. Arkadiev V.K.	Prominent physicist	Loyal
2. Vvedensky B.A.	Good physicist	Loyal. He works for All-Union Institute of Electrotechnology
3. Bachinsky A.I.	Good physicist	One of the Black Hundreds
4. Teodorchik K.F.	Mediocre physicist	Disloyal
Group of Academician Mandelstam		
1. Mandelstam L.I.	Outstanding Physicist with European reputation. His recent discovery (the Mandelstam–Landsberg effect) is well known abroad	Loyal. He is principal Consultant of the Trust of weak currents. Recently, he was appointed as Head of theoretical laboratory belonging to All-Union Institute of Electrotechnology. Excellent university Teacher
2. Vavilov S.I.	Prominent Physicist. Many of his works are well known abroad	Right-wing trend, but recently he came to work with us. He delivered lectures for the community of physicist–materialist and for the courses at Communist Academy, he has written for "Revolution and Culture" and for "Natural Science and Marxism"
3. Tamm I.E.	Good young physicist	Loyal, but recently the hesitations appeared
4. Landsberg G.S.	Good physicist. His recent results are well known abroad	Right-wing orientation

"Mediocre Physicist" and "disloyal citizen" Kazimir Franzievich Teodorchik (1891–1968) took subsequently an active part in work which was initiated by L.I. Mandelstam and his colleagues. Since 1919, K.F. Teodorchik was a researcher at the laboratory of electromagnetism, and in the first half of the 1920s he collaborated with

B.V. Vvedensky, who was mentioned above. In 1930, he became Full Member of MSU Institute of Physics, and in 1931 he started to work for the laboratory of oscillations and shortwaves founded by Mandelstam and Chaikin. In 1939, he became Research Supervisor of this laboratory.[5]

6.4 Boris Mikhailovich Hessen

In Sect. 6.2, B.M. Hessen, who became Director of MSU Institute of Physics, was called "in a sense" L.I. Mandelstam's student. What does this mean? Let me cite B.M. Hessen's personal file which the Archives of Communist Academy keep [398, 364-3a-17]. His autobiography says: "I was born in 1893. In 1913 I finished 8 classes of gymnasium. In 1913–1914 I was a student of Edinburgh University (Faculty of Science, Department of Pure Science). I took "Introduction to mathematical analysis", the first part of differential calculus, analytical geometry, and algebra.

During the Imperialist War—because it was impossible to get to England—two years (1914 and 1916), I was a student at the Economics Division of Petrograd Polytechnic School. I worked there on statistics under A.A. Chuprov and was also engaged in mathematical statistics. At the same time, I attended the Faculty of Physics and Mathematics of Petrograd University where I entered as a Jew.[6]

In 1917, I was Secretary of the organization of internationalists in Elizabethgrad up to October. "From 1917 to 1923 I am in the Party and I am doing the Soviet work" [398, 364-3a-17].

In June 1924, Hessen entered Natural Sciences Department of Institute of Red Professorship (hereinafter—IRP), belonging to the Communist Academy. This department had been just opened.

"Professor Mandelstam gave his consent to propose a theme of research for me" wrote Hessen in his undated letter to the administration of IRP.

In 1928, Hessen, who was already in his final year, wrote the following letter to the administration:

"I ask for a trip to Germany. Now I work on the problem of foundations of statistical mechanics and the application of the statistical method in physics. My research supervisor is Prof. Mandelstam. With regard to mathematics and methodology my research proceeds from the ideas of Richard von Mises. Richard von Mises's works have appeared in recent years and little known and little developed. Richard von Mises is connected with Mandelstam by collaborative work, for this reason the conditions of my work with Richard von Mises' will be very favorable".

Judging from the personal file of Hessen, stored in the archives of Moscow State University, in 1928 he travelled to Germany. Although the Archives do not have any report about this trip, he likely met with Richard von Mises.

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[5] About K.F. Teodorchik see [36].

[6] Hessen meant a quota which is used in tsarist Russia.

B.M. Hessen developed Richard von Mises' ideas which he elaborated in Strasbourg and discussed with L.I. Mandelstam (see Chap. 2, Sect. 2.6). As the letters of Mandelstam to Richard von Mises testify, Mandelstam continued to be interested in the Richard von Mises calculus in the 1920s. It is possible that they have discussed this issue while L.I. Mandelstam was in Germany in 1923.

The essence of Richard von Mises' ideas runs as follows. Richard von Mises proposed the "rational" concept of probability diverging with its customary use (Math. Z., 1919). He did not accept the classical notion of probability, according to which this magnitude is the ratio of favorable outcomes to the number of equally probable. Richard von Mises assumed the concept of a collective as a basis of the theory of probability, and he proceeded from a set of events or phenomena that had a feature accessible to observation.

Richard von Mises treated the probability of an event as its relative frequency over time. More exactly, the probability is the limit of a sequence of relative frequencies. Richard von Mises meant the frequencies of appearance of any event in the collective of such events, for example, the appearance of "tail" in a series of tosses (tests) of a coin. The limit shows itself with an expansion of collective, when a number of its elements tend to infinity.

Richard von Mises' frequency (or empirical) definition of probability met the trends dominating in statistical physics and statistics in the first decades of the twentieth century. This definition received a response. Even B.V. Gnedenko's textbook (1969, fifth edition), which represents the probability from the fundamentally different points of view and characterizes Richard von Mises' conception as a "widely distributed especially among specialists in natural science" [134, p. 46]. This concept came to the philosophical literature, since it met the empirically critical spirit of that period. Such well-known Philosophers Hans Reichenbach, Karl Popper, and Wesley Salmon proposed their versions of the frequency conception (see a review of these versions in H.E. Kyburg [194]).

By developing the frequency concept of probability, B.M. Hessen emphasized what was mentioned by Richard von Mises, but was mentioned in passing. B.M. Hessen, like another protagonist of Richard von Mises' conception, the mathematician A.Ia. Chinchin (1894–1959), emphasized that this conception, in contrast to the classical one, treated probability as an objective feature of physical phenomena since it proceeded from the concept of collective. If the classical probability represents the degree of our ignorance concerning what happens, Richard von Mises' empirical conception exposes the empirically testable frequency of some event in the series of tests of which the purpose is to trace an objective tendency in the appearances of this event. "The subject of probability theory, Hessen writes, does not refer to our lack of knowledge, it is founded on objective properties of the process under study" [154, p. 34].

B.M. Hessen was concerned with the mathematical problems to which Richard von Mises' formulation of probability came. The thing is that this formulation is based on a couple of presumptions: (1) irregularity (randomness), that is, a lack of game system with respect to the game which produces a collective, and (2) existence of the limit of relative frequencies as a collective increases endlessly (the number of tests tends to infinity). Both postulates required a mathematical elaboration; moreover, it was observed that they are probably inconsistent.

However, the main point of Hessen's work was the interpretation of statistical physics proceeding from the frequency conception of probability. At the same time, this was the materialistic interpretation of statistical physics for him: The frequency conception treats the probability as an objective characteristic of physical systems. Hessen emphasized that the statistical regularity is not a "bypass route", and it is not a "temporal crutch which we use under a lack of knowledge". "The statistical regularity does not destroy dynamical laws and this regularity does not contradict to a dynamical regularity. The statistical regularities are valid in their field, which differs from the field of the dynamical regularities" [154, p. 37]. Statistical physics treats the processes provided by movement of a great amount of particles, molecules. Every particle behaves itself in accordance with the dynamical laws. However, the collective of particles as a whole is run by the statistical regularity. "In essence the statistical regularity can not guide the individual particles composing a collective. This is not its defect, it is its specific since it deals with the properties which characterize the whole collective, rather than its elements" [154, p. 37].[7] "The dynamical law is not adapted to study a collective. An instrument to study a collective is the statistical regularity" [153, p. 455].

By pushing a materialistic treatment of statistical physics, B.M. Hessen, in spite of all his respect for Richard von Mises, criticized Richard von Mises' Machism. B.M. Hessen was not able to accept Richard von Mises' interpretation of causality: Richard von Mises emphasized the mechanistic nature of causality. For Hessen, causality is "the corner stone of materialistic world view" [145, p. 159, 152]. Hessen posed a problem of the generalization of causality by means of application to it of the concept of chance.

This, however, did not save him from accusations of "semi-machist errors" [385, p. VII]. This happened already in 1930, when the newly produced Academician (1929) A.M. Deborin and his collaborators were subjected to an organized ostracism. Then probably only a few of Marxists felt that this charge also means the beginning of the end of the career of B.M. Hessen and portends a close decline of any meaningful debate in Marxist philosophy. A different philosophy had already been absent.

In 1928, however, the young scientists were still full of optimism and romanticism. B.M. Hessen studied the concept of Richard von Mises and went for this purpose on a business trip to Germany. In 1928, he enrolled in the Physics and Mathematics Department of Moscow State University, and he organized Chair of the history and philosophy of science. In the late 1920s at the Communist Academy, a community of physicists and mathematicians arose. The philosophical problems of science, particularly Richard von Mises' concept of probability, were under intensive discussions. For example, in 1929 this community discussed A.Ia. Chinchin's paper (February 1 [398, 350-2-397]). Hessen took the floor as an opponent.

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[7] For the first time Hessen's contribution to the philosophy of physics was probably described by K.H. Delakorov [88].

According to S.M. Rytov (in the interview conducted by the present author), Richard von Mises' conception of probability was under discussion in the Mandelstam seminars at MSU.

As was mentioned, both Chinchin and Hessen were enthusiasts of Richard von Mises' concept of probability. However, Chinchin's approach was another. Chinchin criticized Richard von Mises' conception from both methodological and mathematical points of view. Methodologically, Richard von Mises' concept of collective was too abstract to apply it to the real problems in physics. Chinchin also pointed to inconsistencies in the mathematical postulates which, according to Richard von Mises, the concept of collective should meet.

Chinchin's lecture corresponded to his paper published in the journal "Uspekhi Fizicheskikh Nauk" edited by Hessen and Shpolsky [78].

Chinchin's lecture was criticized by B.M. Hessen and A.A. Andronov. Moreover, the Subject Committee of the Chair of the history and philosophy of science (1.03.1929) came to the decision captivated by its naïve radicalism: "to protest against Chinchin's unjustified lecture delivered at the meeting of the phys.- maths" [398, 24-1-229].

As is well known, the second half of the 1930s was marked by the appearance of A.N. Kolmogorov's axiomatic theory of probability. In 1929, Kolmogorov presented some of his ideas in the *Proceedings of the Communist Academy*, in 1933 he published the axiomatic treatment of probability in German, and in 1936 Kolmogorov's book written in Russian appeared.

Due to Kolmogorov, a new controversy appeared: Kolmogorv's axiomatic versus Richard von Mises' frequency theory. Chinchin took the Kolmogorov's position. As a matter of fact, he cooperated with Kolmogorov by elaborating the theory of probability. Hessen no longer participated in the debates concerning the concept of probability.

B.M. Hessen's carrier was really dizzy. As was noted above, he became Dean of Physics School and Director of MSU Institute of Physics. At the same time, he was Head of the Department at Communist Academy. He was one of the editors of Soviet main journal on the foundations of physics "Uspekhi Fizicheskikh Nauk" ("Physics-Uspekhi"), and he was the editor of the division "Physics" in "Big Soviet Encyclopedia". In 1934, he obtained the status of corresponding member of the USSR Academy of Sciences, and in 1935 by avoiding the procedure of defense he together with A.K. Timiriazev and V.I. Romanov received the degree of Doctor of Physico-Mathematical Science.

As Editor of "Uspekhi Fizicheskikh Nauk", B.M. Hessen organized a number of publications of prominent European scientists. In particular, he initiated the publication of Richard von Mises' lecture "On causal and statistical laws in physics" at Fifth Congress of physicists and mathematicians in Prague (16 September 1929). B.M. Hessen wrote the Foreword for this publication. B.M. Hessen wrote the Foreword for this publication.

L.I. Mandelstam also contributed to the Russian publication of Richard von Mises' writings (see Chap. 2, Sect. 2.6). True, in contrast to Hessen, Mandelstam has never dissociated himself from Richard von Mises' philosophy in his articles and letters. Besides Mandelstam, Hessen had other Supervisors, namely Marx, Engels, Lenin.

Hessen became world-famed due to his lecture delivered at the International Congress on the History of Science in London (1931). This lecture was not dedicated to probability and statistics, but dedicated to the social and economic prerequisites of Newton's "Mathematical principle of natural philosophy". This lecture represents a Marxist approach to the genesis of modern science. Hessen emphasized the development of material production and class composition [148].

Hessen's lecture challenged the current methodology of the history of science. This lecture presented arguments in favor of the externalism in the history of science. Hessen's lecture is cited and discussed (see, e.g., "Science in context". Vol. 1, No. 1, 1987, pp. 105–108; "The Social and Economic Roots of the Scientific Revolution", Springer, 2009, 253–256).

"To dismiss Prof. Hessen" (the order from 5.9.1936), this order followed his arrest. In the same year, 1936, Hessen was executed.

Before his arrest, Hessen went on a six-month leave for research work. To fulfill the duties of Director of MSU Institute of Physics, S.E. Chaikin was appointed. Earlier, E. Chaikin was appointed as Dean of the Physics Faculty (December 1934).

* * *

In 2015, the Russian first book about B.M. Hessen was published [57]. See also the review of this book written by the present author [279].

Regrettably, there is little information about Hessen as a Mandelstam's student in this book. There is little information about Hessen's contacts with Mandelstam's students and coworkers. Probably, these contacts were not very significant. Hessen had his own sphere of communication. This sphere embraced the communist party activists, philosopher–Marxists, officials, authorities. There were important figures among them: one of the leading political figures of the Communist Party Nikolay Bukharin, the leader of the Soviet philosophers-marxists Academician A. Deborin (his election as an academics had two steps: at the beginning, the academicians voted against this candidacy, but then, under pressure from the authorities, he was elected), one of the coeditors-in- chief of the journal "Uspekhi Phizicheskikh Nauk" Shpolskii (the other was Hessen), the historian of science and philosopher E.Kolman.

True, this book informs us that Igor Tamm (Hessen's friend; they together went to gymnasium in Elizabethgrad) supported the publication of Hessen's book dedicated to the theory of relativity (1929). The second review in favor of this manuscript had been written by A. Deborin.

We learned also that on 6 August 1930 Presidium of the Communist Academy appointed its delegation to the First All-Union Conference of Physicists in Odessa (19–24 August 1930). Boris Hessen delivered a paper "Materialistic Dialectic and Modern Physics" at the plenary session.

In 1929, Hessen organized one-month course for the school teachers in mathematics and physics. He invited S.I. Vavilov, G.S. Landsberg, and I.E. Tamm to deliver their lectures.

The authors of the book on Hessen concentrate on the controversy which is not directly relevant to the main themes of the present book. This is controversial between the two schools (or directions) in Soviet Marxist philosophy. These two schools can be named "dialectics" and "materialists" correspondingly. The "dialectics" were headed by A. Deborin and B. Hessen, and the "materialists" were mainly represented by A. Timiriazev and A. Maksimov (A. Timiriazev was Physicist, and A. Maksimov mainly worked as State Official and Historian of science).

In the middle of the 1920s, the "dialectics" had the advantage of their rather good education. By the beginning of the 1930s, the aggressive line of "mechanicists" had been supported by the supreme leadership of the Soviet Union. Dialectics was characterized as the "Menshevik-idealist". This was rather a political accusation as the "Menshevik" was a political party which was forbidden by the authorities. Nevertheless, the competition between the "dialectics" and "mechanicists" can be observed in the 1930s and even after World War II. Deborin, say, held some of his positions in the Academy of Sciences until his death.

In the context of the present book, it is worth to emphasize that namely the "dialectic" Hessen supported (and even organized) a discussion concerning the concept of probability. In the reviewed book about Hessen, this discussion is mentioned passing by.

The reviewed book gives new details about famous Hessen's paper delivered on the International Congress on the History of Science in London (June, 4, 1931). First of all, it describes the political and ethical circumstances under which Hessen presented his paper. The Soviet delegation which was headed by N. Bukharin, one of the leaders of the Soviet communists communist party, was favorably met by the organizers. At the same time, Hessen was given 20 min only to deliver his paper.

The reviewed book contains a review of the response to Hessen's paper in the Western historical and philosophical literature. Here, the authors follow Loren Graham's and Vladimir Kirsanov's historical writings.

In the 1930s, Hessen concentrated on the problems of the social history of science. The methodology of modern physics became of secondary importance for him.

The reviewed book provided the description of Hessen's political trial. Such a description is provided for the first time. Hessen was arrested on August 21, 1936. From the very beginning, Hessen confessed that he was connected with the persons who were accused as the "enemies of people". He confessed that he spoke against the Soviet State and Stalin. However, he rejected the statement that he was Member of a terrorist organization. The investigators reached this "confession" by means of a deception: They added this "confession" to the documents which has already been signed by Hessen.

Hessen was sentenced to be shot on November 20, and he was executed on this day.

The authors came to the following important conclusion: "Hessen's blood is not on the conscience of ignorant NKVD functionaries (they themselves have been executed in 1939–1940) but on the soul of Stalin, on the soul of the stalinists and anti-Semites among Hessen's coworkers and collaborators at Moscow State University. They could not forgive him his talent and humanity".

In 1936, the Academy of Sciences' general assembly eliminated Hessen as corresponding Member of the Academy of Sciences, and in 1957 the Academy of Sciences' general assembly restored Hessen as its corresponding Member.

Б. Гессен

6.5 Mandelstam as Teacher

About L.I. Mandelstam as Teacher many enthusiastic words have been written. Let me refer to just a fact: He was one of the last scholars whose lectures and seminars were assembled and recorded by his students to become books (fourth and fifth volumes of his "Complete Works").

In Chap. 5, the verbal portrait of Mandelstam as a lecturer at Odessa Polytechnic Institute was cited. Here, extracts from the verbal portraits of Mandelstam as the MSU Teacher will follow [103, pp. 10–11]:

> Although he speaks with quite precise phrases, but he begins somewhat awkwardly. Something apologetic in his tone and even pose will also burst open later. However, he gradually warms up and reaches the state in which the only thing that is relevant for him in the world are the words spoken, the thought expressed. His voice is slightly nasal, not loud, and only the wonderful acoustics of the auditorium (subsequently reconstructed and now, unfortunately, non-existent), a clarity of the structure and contents of every phrase make this voice understandable, even for listeners in rear rows. Mandelstam does not make slips while speaking, does not need to correct himself, he pronounces only something he is sure of and has been reflected on. But, until the end of the lecture, he does not leave the saving spot between the end of the desk and the blackboard behind him. On the desk he places his lecture notes which he sometimes bends over or which he, having taken off the pince-nez and holding them with a hand somewhat aside, brings closer to near-sighted eyes. This combination of clarity and firmness in something important and softness of behavior is, as we shall see, characteristic of him. His entire appearance is a variant of that of a Russian-European intelligent of a pre-revolutionary epoch. His entire behavior is that of such an intelligent, unbending in important matters, understanding and yielding in minor. An extraordinary mind power and a high spiritual, moral culture allows him to understand, better and clearer than others, what is truly important, and what is not. Niels Bohr behaves in the same auditorium in the same way in a few years later. And, although the facial features of both are very different, and although in comparison to Mandelstam, Bohr is bigheaded with bushy eyebrows and looks somewhat like a clodhopper, common generic features are evident.
>
> This was one of the famous Mandelstam "optional courses" of the 1930s. They continued for many years—on the theory of relativity, physical optics, theory of oscillations, quantum mechanics. The very word "optional" always contains a shadow of being not really obligatory, not really useful. It was, however, sufficient to start thinking on what Mandelstam was talking about to understand its necessity for a physicist striving to "get to the very essence". Mandelstam lectured in a somewhat "chamber" manner. His formation as a person took place during an epoch in which science in general and physics in particular were the destiny of only a few.

Anna Livanova, who attended the seminars of L.I. Mandelstam at the end of the 1930s, says: "Here he enters to the big physical audience, he, as always, surrounded by people, tall, slightly stooping, with a thick brush mustache, with a smile full of kindness and charm. Now I do not remember who and what reported and what Mandelstam himself said, and I guess that for me, for an aspiring student, many things were not clear, but it's a feeling of light and a very significant celebration, which covered thee with the advent of Mandelstam, preserved for life" [219, p. 157].

The biography of L.I. Mandelstam, which the first volume of his "Complete Works" contains, says the following: "L.I. Mandelstam's lectures and seminars were a notable event in the scientific life of our country. His lectures' audience was extensive: it consisted not only of students, graduate students, young scientists, but also of prominent physicists. The secret of their success consisted in what he was able to teach how to think in physics. Mandelstam did not simply inform about facts, he did not simply construct the chain of postulates, definitions and deductions. His lectures opposed a formal, "smooth" presentation, even if this presentation is perfect in a sense. Leonid Isaakovich never avoided and shaded difficulties. Contrary he always emphasized difficulties, did them distinct, as he liked to speak. After that he disposed of them, eliminated them by attacking them within the framework his subtle and clear thought. Mandelstam's lectures were the demonstration of the very process of thinking in physics" [1, Vol. 1, pp. 63–65].

However, the process was not so smooth. According to N.L. Kaidanovsky, who attended Mandelstam's lectures in 1930–31, these lectures attracted physicists and radio-engineers from every corner of Moscow. The main physical auditorium was overfilled. "Along with a group of students one could see famous professors and even academicians" [169, p. 9]. However, the students had not been prepared for learning the content of Mandelstam's lectures. They almost did not understand anything. In addition, teaching in mathematics was behindhand with respect to teaching in physics. By feeling a lack of understanding, the students appealed to the dean's office with a request to organize the corresponding exercises. A. Vitt, one of the best of Mandelstam's students, was assigned to teach. By being brilliant Mathematician, Vitt could not even understand what the students wanted. He considered that Mandelstam's lectures were not enough complete and introduced new problems and theories with which he was fascinated. Besides, Vitt was laconic and speaking thickly. We were entangled due to Vitt" (Ibidem).

N.L. Kaidanovsky points to the reason of this contradiction: The first-year students had been poorly trained. Besides a small group of those who had intelligentsia parents (and 4–5 years of seniority), the great bulk consisted of workers' faculty graduators. There was a group of party-tysiachniks (the members of the communist party sent to lead the student body). They were trained even worse. The entrance examination was formal. The biographical particulars had decisive importance.

N.L. Kaidanovsky writes further that the crisis had been partially overcome, when S.E. Chaikin, who had pedagogical talent, came to conduct seminars.

Kaidanovsky's recollections are interesting since they characterize the participation of Mandelstam's former students in teaching.

Let us ask a question: Were lectures similar to Mandelstam's lectures popular now at the end of the 2010 years? Should the lecture saturated by historical reminiscences, philosophical digressions, the analysis of paradoxes which are interesting, but useless from a practical point of view, and attract all of the Moscow audience? To answer this question would mean to characterize the contemporary situation in science and education. This is a difficult question. It is only clear that Mandelstam's lectures were relevant to their time of enthusiasm and romanticism.

6.6 Teaching as an Ethical Principle

In the interview conducted by A. Livanova, I.E. Tamm said that Mandelstam devoted much time to the preparation of his lectures and seminars. "When a seminar was planned, only a range of problems and of main speakers was outlined. Later, however, new literature was reviewed and new papers were delivered. Mandelstam always provided an initial pulse. In seldom cases when Mandelstam was not present, students said “Tea without sugar”. Every seminar had Mandelstam’s opening address. Often this address looked like an improvisation, since Mandelstam did not use any notes and preparatory materials. Only a few people knew that it took plenty of time for Mandelstam to prepare his lectures and seminars. Especially he did everything in his power to teach his graduate students and young researchers to speak clearly and distinctly by emphasizing main points” (see [219, p. 158]).

In the previous section, the literary portrait of Mandelstam delivering his lectures pictured by E.L. Feinberg was reproduced. In Feinberg’s recollections, this portrait is complemented by the following discussion [8, pp. 237–238].

> Only after about ten years since that time did I learn how these lectures had been prepared. When working on a five-volume edition of Mandelstam’s scientific work I was offered a honorable task of preparing for print a text of his lectures on theory of measurement in quantum mechanics. The original material was in the form of lecture notes, taken by different listeners, of all five lectures given in 1939, in the first place the especially thorough notes by S.M. Rytov (I found my own notes long after these lectures were published). One lecture (the fourth) was taken down in shorthand. Leonid Isaakovich had never seen neither the notes, nor the shorthand and had not checked them. However his extant working notebooks of the period of 1938– 1939 were given to me. These were usual thick (relatively disorderly) school notebooks containing much relating to his work in these years: fragments of calculations without comments, some notes with formulae without clarification and, amongst all this, disjoined pieces of the first three lectures that I needed. Leonid Isaakovich wrote them with full phrases, as if preparing them for print. Each such piece existed in several not too distinct variants. One saw that he was essentially writing with ease, with complete, literary perfect sentences. Very little was crossed out or written between the lines. At the same time a multiple reiteration and variation of the whole excerpts, sometimes with mutually exchanging positions in the text, reflected some sort of indecision, a constant doubt in the readiness, in the finality of the written, a constant care on its improvement. A closeness of these texts to the notes made by listeners allowed to trust the notes of other lectures, for which nothing could be found in the extant notebooks, as well.

By retelling I.L. Fabelinsky’s recollections, E.L. Feinberg described how Mandelstam worked with his students after the lectures and seminars (cited in [8, pp. 228–229]).

> L.I. asked students to put their questions during the break or at the end of the lecture and seminar. Usually, there were only a few questions. The majority of students was ashamed to show their incompetence in the presence of well-known physicists attending Mandelstam’s lectures. Leonid Isaakovich understood this, and he invited students to come to the physics office which was nearby the big physics auditorium where he delivered his lectures. There one can speak with him tête-à-tête.
>
> As I was fourth-year student, I learned that nobody had calculated the inner field of liquid acting on a molecule. I thought that I had an idea of how to solve this problem and I came to Leonid Isaakovich to discuss my “finding”. My idea was silly. I comprehended this after my conversation with L.I. However, it is important how L.I. received me and how he conducted a conversation.
>
> When I came to the desk at which L.I. sat, he stood up, made several steps to me, and offered his hand. With deep attention, he listened me. He never interrupted me. Then, he started to speak with me and spoke with me as an equal but on such a level that I could participate. Our conversation lasted half an hour. At the first stage, L.I. succeeded to put the problem in such a way that it received a clear sense.

"L.I. Mandelstam liked to teach in the direct sense of the word, A.A. Andronov wrote, he liked to put and explain different and tricky problems, different paradoxes" [17, p. 105]. Let me to state that situation cannot be only described by "he liked...". Teaching implying a transfer of knowledge from a professor to a student was a kind of the ethical principle for Mandelstam. By preparing his lectures and seminars Mandelstam laid out the time which he could use for work on scientific articles and on the scientific results. A lot has been written that Mandelstam combined teaching and research and that Mandelstam's lectures led to scientific problems. It is true for special lectures and seminars, say on special problems of optics. But Mandelstam made efforts to prepare the lectures and seminars describing the foundation of science. Here, he had to solve didactic, methodic, and pedagogical problems.

Some of Mandelstam's former students followed these ethical maxima. According to recollections, A.A. Andronov spent much time to teach students. Miller recalls how Andronov communicated with audience. "You can put any questions before me. However, it is possible that I shall not be able to answer them immediately. Sometimes I shall answer your question later by discussing the corresponding piece, sometimes I shall do it within the next lecture after thinking over. At last in some situations I should recognize that I don't know an answer" [234].

Many prominent scientists do not take teaching as a moral obligation. Let me cite a piece from another part of the history of Soviet science.

Institute of Physics and Technology (MFTI) was established on the base of Physico-Technological Faculty of MSU in 1951. Now, I am not going to describe the history of this institute (this history is described in [170, 172], for example). The characteristic message of this Institute was the invitation of the prominent working scientists to teach. However this program encountered the resistance proceeding from the very prominent scientists. In 1963, one of the founders of MFTI, Peter Leonidovich Kapitsa, said at the meeting of professors: "It is important that we have serious problems with teaching in the fundamental disciplines—general physics, mathematics, mechanics. Earlier the great scientists delivered these courses. Now the situation has changed. It is difficult to say, why" [170, p. 134]. Kapitza also said "While a scientist teaches, he learns himself" [Ibid., p. 135].

To teach the general courses is hard work. Nevertheless, Mandelstam taught such courses.

6.7 Dear Friend, Dear Mis! Again on Mandelstam's and His Wife's Letters to Richard von Mises

In the previous chapter, it is said by the end of the review of L.I. Mandelstam's letters to Richard von Mises that since 1925 these letters' tone changed. Quite recently (10.03.1924, Leningrad), Mandelstam skeptically wrote about the Soviet higher school. "I want to continue my work for industry, because academic work does not gladden me. On the other hand, I shall take responsibility for a laboratory where I shall probably be able to work. This not entirely satisfies me, but work for higher school has its dark side which I am not able to completely accept".

However, on 29.06.1926 he writes in another manner:

> As you know, we have already lived seven months in Moscow. In general I am satisfied my work here. With respect of teaching I am glad to have to do with senior students who are prepared for perception.
>
> I have also several students who are able and want to be engaged in research work. In last term we considered some theoretical problems including the problem of the principle of electromagnetic buzzer about which I said to you (this problem I put before one of young men).[8] Now I don't know if it is possible to publish this article in German in any journal. I had in mind *Zeitschrift für Physik*. For your journal I have an article which is interesting from a mathematical point of view. Could you please allow me to send a manuscript to you? I should be thankful if you let me know of your opinion. Regrettably I personally could not did much during this term because I was busy with many other things. I hope that soon the situation will be better and I shall have a possibility to do experimental work. The thing is that I have been invited to work in theoretical physics and I have no a laboratory. True, in the course of talk I stipulated for organizing such a laboratory. There is a special room for it. But now under common deficit it is difficult to get essential apparatus and equipment. True, I hope that it would be better with it in the next term. I am still connected with industry and I often need to travel to Petrograd. Now we have a vacation and we are leaving for Odessa and then for the Crimea.

Step by step things come out right. About this Lydia Solomonovna's 24.01.1928 letter says the following.

> Dear Mis,
>
> Today I have received your 17.1 letter and want to answer immediately. I don't want to postpone. You will not believe how I am sorry that you have problems with our things. I ask you to send them to us. I hope that it is possible to obtain a permission to receive it without customs. Your proposal does not convenient for me. I think that you will understand the reasons. I only ask to allow me to pay here all these great expenses. To post money is difficult now. Once more I thank you for your cares. You have not had any information from me for a long time because we had troubles with our relatives. We were not in a mood to write. I hope very much that my husband will be able to travel abroad on summer. For me it is impossible. I should be very glad to meet your friends. I am very sorry that you missed a chance to arrive. My husband swears that he will write you and will tell about everything. In turn, I shall stop a flow of my German words. I only say that Sergei became a student at Mathematical Faculty of MSU. He has health problems. He is very thin and nervous. I already have not worked for several years, I have forgotten my medicine. You say: it is not a pity… No, I say to you, time presses. Sometimes I come to the conclusion that I am a good house wife only… You say: it is not worth to be sorry. May be.
>
> Many—many heartfelt greetings from Sergei and me.

---

[8] M.A. Leontovich.

In his next letter, Mandelstam shared his impressions of his work for MSU. There is an alarming news: the trouble with the relatives (see Chap. 7, Sect. 7.4)

> 9.02.1928 Moscow
>
> Dear Friend,
>
> I shall not excuse that I have not written you for a long time. I don't know why it happened in such a way, but I have often and even twice often experienced a need to speak with you in letters. A lack of your letters means the privations for me. You know how I happy receiving letters from you.
>
> We recently survived hard times, we had much family troubles. We three are OK (at least at the first sight). I am much working especially with the students of fifth course, some of them are intelligent enough.
>
> One of my assistances and former student Mr. Tamm was invited by H. Lorentz to Leiden for several months. I am very glad.
>
> Here we are busy with the development different small theoretical problems. I am sorry that I have not a progress with the organization of a laboratory. Recently I became to be interested in quantum mechanics. I should like to discuss different issues with you. It is possible that a chance will arise to travel to Germany. True, till now I had not results. But I don't lose hope. Probably during the next summer vacation I shall see you.

In this letter Mandelstam again discusses literature, but it is rather to share his emotions. As early he is full of recollections about his 1923 trip to Germany and the meetings with Richard von Mises there.

> The books which I have received from you became one of the great pleasure for me. I don't know how to thank you. The second volume of Riemann- Weber contains very valuable material.[9] However, I could not to read through attentively. Your first volume is required here. When reading Gauss-Garling correspondence I experienced fear and joyful trembling.[10] All the book makes an joyful and sedative impression. And the book about Ohm is very interesting. When I am writing you, I experience a desire to see you again. It is difficult to fancy how a meeting with you is very important for me.

The cited letter shows that L.I. Mandelstam felt a lack of laboratory as a burden. He complained he did not have an opportunity to conduct real experiments in his 9.02.1928 letter. At last, in his 27.4.1929 letter he says two nice things new: He received an apartment which is very good under the present circumstances. He also received a room to organize a laboratory, and he received an amount of money to buy equipment. In this letter, he explains the situation with a Russian translation of the Richard von Mises book (see Chap. 2).

Mandelstam did his best to go to Germany once more, but he succeeded in 1930 only. A postcard, which was sent by a group of participants of First Congress of Soviet Physicists, evidences this. The postcard is signed by Papalexy, Sommerfeld, Ehrenfest, and others. This postcard is addressed to Richard von Mises for L.I. Mandelstam [8, p. 64].

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[9] Mandelstam means the two volumes by Philipp Frank and Richard von Mises which arose on the base of the Riemann lectures. See [313] and (http://www.springerlink.com/content/51w200619038k141/).

[10] See [69, 71].

True, Richard von Mises had been to Russia before. He participated in Sixth Congress of Russian Physicists (1928). Since the participants travelled by ship along Volga, Sixth Congress has been called the Moscow-Volga congress.

In 19.04.1934, Mandelstam writes to Richard von Mises, who emigrated to Turkey in 1934.

> No news from you for a long time, but we should like to know how are you doing in new circumstances. I should like to know anything about you and I should be grateful for any message. I am sorry that we have not met, but I hope that we shall meet on September. I am sorry that I am not able to arrive. My trip should be legitimated as a business trip with a definite goal. Perhaps you will come to the Crimea or Odessa on September? We should be very glad.
>
> How are things with Frau Dr.? Heartfelt greetings to her.
>
> A postscript:
>
> Heartfelt greetings. Lidia Mandelstam.
>
> In the 22.09.1934 letter there is a phrase:
>
> We are disappointed very much that you were not able to arrive.

Here is L.I. Mandelstam's 7.06.1935 letter (Moscow):

> Dear Friend,
>
> The director of the local higher engineering school asked me to ask you if you wish to visit Odessa on September and to deliver a lecture (a topic is of your choice). If you agree, this institute would take steps to organize the entry visa for you. Does the second half of September suit you? If the visa are not ready, could you put off your trip till October?
>
> Institute of Physics of Moscow University is very interested in your visit and asks me to invite you to arrive to Moscow, if your visit to Odessa takes place. The Ministry of Higher Education would cover all your travel expenses. I don't need to speak you, how much we are glad because of the forth coming communication with you. I shall impatiently wait for your answer.
>
> Please, write as soon as possible in order to I shall get a possibility to pass your affirmative reply to the corresponding officials.
>
> We have not news from you and we don't know how things are with you.
>
> How are things with Frau Dr.?
>
> Please, write about her.
>
> Heartfelt greetings.
>
> Yours L. Mandelstam.

Let us turn to the letter written in the terrible 1937.

> 15.03.1937. Moscow
>
> Dear Friend,
>
> I foolishly missed an opportunity to immediately write you on receiving your card which pleased us very much. Soon after I became ill. Pangs came again. Probably it is gall-bladder. Physicians cannot determine.
>
> My wife and me were at sanatorium nearby Moscow. Now everything is OK and I am shall soon start to work. No more news. Sergei worked much at the Institute. I also have many things to do. I am grateful to you for a copy of the second edition of your book. However, I

> already confirmed receiving it and receiving your off-print.[11] With great pleasure I have read your book. There are many questions which I should gladly discuss with you. Thank you also for off-print. When does your "Small textbook of positivism" appear? I wait impatiently for it. Great greetings to Frau Dr.
>
> With warm wishes,
>
> Yours Mandelstam.
>
> Dear Ms! Thank you very much for your card. I am glad that your things are good. How are things with your mother.
>
> Lidia Mandelstam.

This is the last letter which the Harvard University Archives keep. A little earlier, Mandelstam's name had been eliminated from the list according to which Richard von Mises sent his offprints. Let us pay attention that Mandelstam asked "When do your textbook on positivism appear?" And he wrote, "I wait impatiently for it".

Mandelstam also writes about his son Ssergei Leonidovich. In 1932, Sergei Mandelstam came to work for MSU Institute of Physics as Researcher of second class (like Leontovich started; see Sect. 6.2).

Let us sum up. The letters from Mandelstam to Richard von Mises do not carry important scientific information. There is a little of physics in them, practically no philosophical comments. Domestic demands, plans, emotions go. However, it is important not only what is said in them, but also what is thought in connection with them. The letters sent by Mandelstam contain the following maxims: "Hearty thanks for what you did for me. Till now inwardly I live there and mostly with you" (10.07.1923). "I have often and even twice often need to speak with you be means of letters. It was hardship that I have not received your letters" (9.02.1928). "Recently I became interested in quantum mechanics. I should like to discuss different issues with you" (9.02.1928).

"Soliloquy, John Dewey wrote, is the product and reflex of converse with others" [89, p. 170]. It is probable that Mandelstam formulated some of his ideas and developed his scientific language in the process of his tacit communication with Richard von Mises.

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[11] Mandelstam referred to the second edition of "Probability, Statistics, Truth".

Chapter 7
Research in Optics: Odessa, Moscow

7.1 The Brillouin–Mandelstam Effect

There is something like a complementarity between the Brillouin–Mandelstam effect (which is usually called the Brillouin effect) and the molecular scattering of light, the phenomenon which, as Chap. 4 said, was described by Lord Rayleigh and Max Planck and was studied by L.I. Mandelstam in Strasbourg. M. Smoluchowski and A. Einstein showed that the Rayleigh scattering of light resulted from inhomogeneity produced by the fluctuations of the density of the medium. The Brillouin–Mandelstam effect consists in splitting of the Rayleigh scattering lines resulting from the time evolution of thermal fluctuations. The Rayleigh scattering considers only random and incoherent thermal fluctuations, whereas the Brillouin–Mandelstam effect is produced by the time-dependent optical density variations.

As N.D. Papalexy and G.S. Landsberg write, this effect was theoretically predicted by L.I. Mandelstam as early as 1918 in Odessa in the course of his optical research started in Strasbourg. But Mandelstam published this result only in 1926. An experimental confirmation of this prediction was achieved by Mandelstam and Landsberg in Moscow in 1930 (their article "The scattering of light in crystals at high temperature") and by E.F. Gross who conducted experiments initiated by Mandelstam. Gross worked for State Optical Institute (Leningrad).

Since the present author does not have any archival material concerning Mandelstam's research in this field, he follows two descriptions, the first one belonging to G.S. Landsberg and the other belonging to E.L. Feinberg. They put some accents in different ways.

Landsberg again turns to the above-mentioned idea of the radio-engineering genesis of L.I. Mandelstam's research in optics. "In the decade from 1914 to 1925, between the Strasbourg and Moscow periods of his activity, Landsberg writes, Mandelstam was unable to carry out his experimental projects in optics. However, this period has not been useless for optical problems. It was during this period that one of the most fruitful ideas of L.I. has taken shape—to apply the idea of the modulation of the oscillations into optics".

A. Pechenkin, *L.I. Mandelstam and His School in Physics*,
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In its simplest form, the phenomenon of modulation is a periodical change in the intensity of a simple (sinusoidal) oscillation, which is thus converted into a more complicated form which is equivalent to a manifold of a few simple vibrations of different periods. This phenomenon is well known in radio, where the telephone transmitter frequency ν, modulated n times per second, emits radiation, consisting of a carrier wave of frequency ν and the two sidebands with frequencies $\nu + n$ and $\nu - n$. As early as in his Strasbourg lecture, Mandelstam illustrated this phenomenon by an excellent demonstration which has entered the textbooks. He interrupted, for example, 3 times per second, the municipal alternating current and showed that such a modulated alternating current forces not only the frequency meter read corresponding to the main frequency of 50 cycles per second to vibrate, but also two other reads responding to the frequencies of $50 - 3 = 47$ and $50 + 3 = 53$ to vibrate.

L.I. saw here the “leading idea” which extremely deepened the question of the molecular scattering of light. His train of thought was the following. The molecular scattering of light is scattering by optical inhomogeneities arising in the medium due to local fluctuations of the refraction index. In turn, these fluctuations arise as a result of fluctuations of the density, concentration, temperature, etc. But the magnitude of the fluctuations varies with time. Consequently, the intensity of scattered light should change over time. In other words, the scattered light undergoes modulation. If monochromatic light falls on the substance, the scattered light must have changed the spectral composition, and the character of the change shows the process of resorption of the fluctuations, that is, a corresponding molecular process. This idea has been fully traced by L.I. as early as in 1918, although a note “On the scattering of light by a molecular medium” has appeared much later, in 1926, when a part of the results had already been published by Brillouin.

A more detailed examination of the process of resorption of fluctuations shows that the main role is played by the phenomenon of alignment of seals and rarefactions, the phenomenon which occurs due to the elastic properties of the substance. This alignment goes with the help of elastic waves diverging from the compaction, and the attenuation of these elastic waves can be essential here, especially if the waves are short. It would be difficult to take into account this attenuation for liquids. For crystalline substances, there can be made more precise conclusions. In this case, L.I. Mandelstam’s theory leads to the conclusion that, under scattering, a line of the primary monochromatic light transforms into a doublet, i.e., into two closely spaced spectral lines whose frequency differs from the frequency of the primary line only on a few thousandths of a percent” [200, p. 92].

Here is a piece from E.L. Feinberg’s historical book. “It is easy to understand that a prolonged absence of regular scientific work tormented L.I. It is however equally clear that it was impossible to fully stop his head from working. As we saw, already in Strasbourg he expanded his scientific interests to the fundamental issues of optics (a remarkable mathematical theory of optical images, etc.). It was in these starving years (N.D. Papalexy writes, in 1918) that he understood that light scattering in the medium can occur on elastic waves creating a necessary inhomogeneity. Then it should be accompanied by a small shift in the frequency of the scattered light. In other words, a spectrum of this light should contain two lines, not one. This work was however published only in 1926. Clearly, such pause could not be explained only by the civil war conditions. Probably, the L.I.'s hesitation played a role here. He was sure that calculations were correct but could also doubt a necessity of publishing an article predicting a very small effect (the frequency change should have constituted 0.003%) before it could be verified in an experiment. This turned out possible much later, in a completely new period of L.I.'s life. Here we just note that independently this effect was predicted in 1922 in France by a French physicist L. Brillouin. Therefore in physics it is known as a Brillouin–Mandelstam effect. Its studies continue even nowadays. Using lasers it turned out possible to explore it in more detail and use it in other studies, also for applied purpose" [103, p. 25].

In Mandelstam's 1926 article, there is no reference to the Debye theory of the heat capacity of solid bodies. However, in the Mandelstam–Landsberg publications, there are such references. In the biography of Mandelstam, the Brillouin–Mandelstam effect is traced back to the Debye theory in which the heat motion in a solid body is taken as a totality of acoustic waves. Figuratively saying, Landsberg writes light is scattered by sound.

The Brillouin–Mandelstam effect had already been observed experimentally by Landsberg and Mandelstam after their experimental penetration into the combinational scattering, which will be discussed in the next section, and experimental confirmation of this effect was published together with data relating to the combinational scattering.

7.2 The Combinational Scattering of Light

As was noted in the Introduction, the combinational scattering belongs to those achievements which are treated at the Nobel level of physics. In 1930, the Nobel Prize for physics was awarded to the Indian scientist Sir Chandrasekhara Venkata Raman (1888–1970) who observed and described this effect in liquids, whereas L.I. Mandelstam and G.S. Landsberg observed it in crystals.

The history of combinational scattering of light has been traced by G.S. Landsberg's disciple I.L. Fabelinsky [97–102] (one of his papers was written in coauthorship with the Nobel Prize winner V.L. Ginzburg [133]). This book follows his interpretation. Fabelinsky also discussed the history of the Nobel Prize which was awarded to R. Raman only (after him this effect is also called).

In the present section, we also cite the recollections of L.I. Mandelstam's son Sergei Leonidovich. In the next section, the Indian version of the history of the combinational scattering will be cited.

What is the combinational scattering of light (Raman scattering)? "The effect can be summarized as follows. The spectrum of the scattered light contains, in addition to the Rayleigh scattering at the frequencies of the exciting light, additional lines, which lie on the long-wavelength and short-wavelength sides of each spectral line of the exciting light. These satellites (the "Stokes" satellites on the long-wavelength side and the "anti-Stokes" satellites on the short-wavelength side) are characteristic of intermolecular, or lattice vibrations, which are sometimes also manifested in infrared absorption spectra" [136, p. 604].

Like the Brillouin–Mandelstam effect, this is a modulation of the scattered light, but a modulation of a more rapid process than the elastic waves diverging from the compaction. The combinational scattering of light originates from the molecular oscillations, the higher frequency infrared vibrations, rather than from intermolecular forces providing an equalizing of the random compactions. The spectrum of the combinational scattering consists of the carrier frequency lines and additional lines which are characterized by the combinational frequencies (this is why the phenomenon is named "the combinational scattering of light").

As a matter of fact, the structure of the spectrum of the combinational scattering is similar to that of the Brillouin–Mandelstam effect. However, in the case of the Brillouin–Mandelstam effect, one speaks on the fine structure of the Rayleigh wings; in the case of combinational scattering, one speaks on the satellites. The satellites with the frequency $\nu - \Lambda\nu_{\mathrm{comb}}$ are called "red satellites", and the satellites with the frequency $\nu + \Lambda\nu_{\mathrm{comb}}$ are called "violet satellites".

By addressing First All-Union Conference on Oscillations, L.I. Mandelstam used the radio-engineering terminology in his description of his discovery, made together with G.S. Landsberg. "In its essential features the spectrum of scattered light reproduces the spectrum of a modulated telephone transmitter.... Speaking schematically here we have nothing else than the modulation of an incident wave by the natural oscillations of a molecule and of molecular aggregates. It is clear then that, like the spectrum of a telephone transmitter carries all our talk, the spectrum of scattered light carries what a molecule speaks about itself. By studying this spectrum you study the structure of a molecule" [1, Vol. 3, p. 60].

L.I. Mandelstam's statement about the "conversation of a molecule" became very popular. It was reproduced in many different contexts. G.S. Gorelik, who is one of the authors of the biography of L.I. Mandelstam in the first volume of his "Complete Works", wrote that in the combinational scattering spectrum, the "oscillations of atoms speak", whereas in the line spectrum of a rarefied gas, the "electron shell speaks" [136, p. 645].

The history of combinational scattering of light can be traced back to the first years of L.I. Mandelstam's work for Moscow State University. As I.L. Fabelinsky, who worked together with G.S. Landsberg during 20 years, writes: "the collaboration between Mandelstam and Landsberg began the moment Mandelstam arrived in Moscow, and it continued essentially to Mandelstam's death in 1944. Landsberg became not only a colleague but also a friend of Mandelstam. For the first joint effort, Landsberg and Mandelstam formulated a problem for studying the spectrum of molecular light scattering in solids. Solids were chosen because it was believed that the damping of high-frequency elastic waves in liquids would be severe" [98, p. 130].

Further Fabelinsky writes as follows [98, pp. 130–134]:

> The problem was difficult for several reasons. First, the magnitude of the displacement is proportional to the ratio of the sound velocity to the speed of light, i.e., of the order of $10^{-6}\nu$ (ν is the fundamental frequency of the exciting light). It was very difficult to detect

a frequency change of one-thousandth of 1%, but not absolutely impossible. The second difficulty was that the intensity of the light scattered by a good crystal was expected to amount to only a small fraction of the intensity of the primary light (a relative intensity of the order of 10^{-8}), and it was necessary not only to detect but also to study the spectrum of this light. Finally, no one knew at the time whether it would be possible to find a crystal, or even a region of a crystal, in which most of the scattered light would be molecular scattering, rather than parasitic scattering due to various inclusions or various other defects.

The first sample chosen for study was crystalline quartz, which is very common in nature and which has excellent optical properties. At the time, very little study had been made of this crystal, but even so there was already a fair amount of confusion. The first physicist to study light scattering in quartz had been Lord Rayleigh. He reached the conclusion that the scattered light which he observed was due to impurities. In a brief note in *Nature*, Raman asserted that Rayleigh's data referred to molecular scattering. The first task was thus to reliably determine the actual relationship between the parasitic light and the molecular scattering in a good quartz crystal. This problem was resolved by Landsberg, in work beginning in 1926. In the Soviet Union at that time there was no optical instrumentation industry, and nobody needed quartz crystals (perhaps some might be found in the Mineralogical Museum).

The investigators were thus confronted with a serious problem: to find a quartz single crystal of good quality. Landsberg and Mandelstam knew that former families of Russian nobility had not only their own coats of arms but also their own seals, which were generally made of the better-quality pieces of rock crystal, in other words, crystalline quartz. It is difficult to trace the movements of these quartz seals, but they ended up in antique shops and pawn shops. That was where Landsberg looked for them and bought them, raising a few eyebrows among the salespeople and others who might see his purchases. (Who needed these crystal seals, especially someone else's?) Landsberg had to keep on shopping, however, because it was only after he returned to the laboratory, placed his most recent purchase in immersion fluid, and illuminated it with an intense light beam in a dark room that he was able to make out the coarse defects. Putting this unsuccessful purchase aside, he continued his search and purchases. All these purchases, of course, were at his own expense. The first quantitative results on the study of light scattering in quartz were reported by Landsberg to the Fifth All-Russian Congress of Physicists in late 1926. In 1927 Landsberg published two papers in *Zeitschrift für Physik*. The basic result of this work was Landsberg's determination, on the basis of a study of the temperature dependence of the scattering intensity, that, in the better samples, only 25% of the scattered light was independent of the temperature and thus due to foreign inclusions, while the other 75% was a linear function of the temperature and by implication the result of molecular scattering. At this point it became possible to start the spectroscopic experiments; suitable samples were already available. In 1927, Landsberg and Mandelstam started their spectroscopic work on molecular light scattering in the better quartz samples at their disposal.

The goal of the first spectroscopic experiments was to detect spectral components which were shifted from the exciting line and which were due to a modulation of the scattered light by thermal elastic waves—the effect predicted by Mandelstam.

Landsberg and Mandelstam began their preliminary experiments with a study of the spectrum of the light scattered in two quartz samples. One of the quartz samples exhibited fluorescence, while the other did not. The scattered light was excited by light from a mercury lamp and analyzed with a quartz spectrograph.

Near several lines, after long exposures, they observed additional lines, or satellites, which were not in the spectrum of the exciting light. These satellites were particularly apparent near the most intense resonant line of the mercury spectrum. The shift of these satellites was much larger than that expected on the basis of modulation of the scattered light by thermal elastic waves. The experimental results were unexpected and surprising. It was necessary to prove that the observed satellites were real. At the beginning, only the "red" or "Stokes"

> satellites, which are strong, were observed in exposures of 15 hours. Many experiments of various kinds were carried out. They proved that the experimentalists were dealing with real spectral lines, rather than spurious signals produced by the many reflecting surfaces of the optics system used. Finally, Mandelstam and Landsberg felt it necessary to carry out a decisive experiment. In the path of the scattered light before the spectrograph the placed a resonant filter: a quartz vessel filled with nonemitting mercury vapor. By adjusting the temperature, they found the density at which the light in the resonant line was completely absorbed after passing through the entire length of the filter. When this resonant filter was then placed in the path of the scattered light, the light corresponding to the resonant line would be completely removed, leaving only the light at the new wavelength.
>
> This experiment was carried out, and the satellites remained. All these many, tedious, and complicated (especially at the time) experiments convinced Landsberg and Mandelstam that they were dealing with real additional spectral lines and thus a new optical effect. They had spent a lot of time on the subject, but they did not begrudge the time spent to prove that the effect was authentic. As soon as this new effect had been firmly established, it was correctly understood by Landsberg and Mandelstam, and their simple quantum explanation of the effect remains valid today, as does their original classical interpretation, which they offered somewhat later.

The "simple quantum explanation" is used the analogy with the Compton effect discovered in 1922. This effect consists in the following. In the scattering of X-rays, photons of both the original energy and of a lower energy can be detected in the scattered flux. In the case of scattering by light elements (aluminum, boron, etc.), i.e., in the case of essentially free electron scattering, the increase in the wavelength is independent of the substance but dependent on the scattering angle.

The Compton effect speaks in favor of the corpuscular properties of light: Scattering of light is treated as a collision of a photon with an electron, the collision under the laws of the conservation of energy and momentum. From this point of view, combinational scattering is a scattering of a photon by a molecule: A photon can either transfer a part of its energy to the molecule, or it can receive a portion of energy from the excited molecule.

I.L. Fabelinsky, however, insists that Mandelstam and Landsberg came to a deeper explanation than that which proceeded from an analogy with the Compton effect and was used as the positive heuristic by Ch. Raman and his collaborators. He cited the following statement in the Mandelstam–Landsberg 1928 article: "The analogy with the Compton shift (in the red direction) is striking, but the mechanism for the change in wavelength should, in all probability, be different" [1, Vol. 1, p. 295]. However, in the cited article, the "mechanism" is rather a problem. The Mandelstam–Landsberg subsequent publications show that these physicists were inclined to explain the combinational scattering on the base of the Kramers–Heisenberg theory which was published in 1925 (in the same year Werner Heisenberg published his first article containing matrix mechanics). Kramers and Heisenberg described the oscillations in atoms, the oscillations which are in resonance with the incident radiation. They also pointed to the "nonresonance terms" which represented oscillations leading to the noncoherent radiation.

To visualize the Kramers–Heisenberg ideas, it is useful to turn to the concept of virtual oscillators in H. Kramers 1924 article. Kramers treated an atom in its connection with the incident radiation as a set of the "virtual oscillators" symbolizing the atom transitions from one stationary state to another.

The recollections of Mandelstam's son Sergei Leonidovich make the history more comprehensive. This recollection provides a sight from the inside [201].

> The Institute of Spectroscopy, Academy of Sciences of the USSR keeps some of the first original spectrograms on which the lines are clearly vis ible. All the experimental conditions are marked on these spectrograms by the hand of Grigorii Samuilovich. The first plate is dated February 21, 1928. On the February 23–24 spectrogram obtained with 15 hours exposition new lines have already been seen quite clearly. However, the first observations of a new effect have been made, as far as I can judge, a little earlier. A lot of checking and rechecking of the observed phenomena had been performed, and G.S. Landsberg met with my father several times a day The nature of the new phenomenon was soon understood by them, and I remember my father's words, spoken in German and belonging, I think, to Helmholtz: "Eine Grosse Entdeckung oder eine Schweinerei".
>
> I also remember that the spectrum required a very long exposition. It was necessary to maintain the mercury lamp light. My mother did it, in particular at nights. We lived at Physics Institute then and our apartment's front door faced the laboratory where the experiments were conducted…

The building about which S.L. Mandelstam wrote is on Fig. 7.1 (the view of the 1980s).

In his essay dedicated to M.A. Leontovich (he was mentioned above as one of the first graduate students of L.I. Mandelstam), I.L. Fabelinsky recalled how he prepared his first review of the discovery of the combinational scattering of light. He showed the prepared text to M.A. Leontovich, who directly participated in the Mandelstam–Landsberg research and published several articles concerning this phenomenon. Leontovich was highly critical with respect to this text. He cried: "You had no right to write that they were looking for the one thing, but have found the other!". Fabelinsky explained that Leontovich so loved L.I. Mandelstam that he could not admit that he did not do something in a direct way. Nevertheless, he took the comment into account. He came to a flexible formulation that L.I. Mandelstam set the problem to study the spectrum of molecular scattering of light in the case of solids, but first of all, he was looking for the experimental confirmation of the Brillouin-Mandelstam effect.

7.3 Indian Approach to the Discovery of the Combinational Scattering of Light

So, in 1930, the Nobel Prize in physics was awarded to C. Raman alone for the discovery of combinational scattering of light. In this section, we are not going to describe the history of Raman's research. We shall only follow how this research was elucidated in Indian literature. In the following, we shall cite the introductory article from the first volume of Raman's six volumes "Complete Works".

In 1922, Raman published a book entitled "The molecular diffraction of light" dedicated to Vice-Chancellor of Calcutta University, who proposed to Raman to take

Fig. 7.1 Building where the Institute of Physics was located

the chair of physics. The concluding chapter was entitled "The scattering of light and quantum mechanics" . Raman came to the conclusion that the scattering of light should be taken as a discrete process.

This chapter contained the following discussion (cit. [293, p. XI]):

> The belief in the validity of Newtonian dynamics applied to the ultimate particles of matter has, however, received a rude shock from the success of the quantum theory as applied to the theory of specific heats, and there seems no particular reason why we should necessary cling to Newtonian dynamics, in constructing the mathematical framework of the field-equation from the kernel of Maxwell's theory. Rather, to be consistent, it is necessary that the field-equations should be modified so as to introduce the concept of the quantum action. In other words, the electrical and magnetic circuits should be conceived not as continuously distributed in the field but as discrete units each representing a quantum of action, and possessing an independent existence.

Further, the introductory article with which Raman's "Complete Works" starts says the following [ibidem, pp. XI–XII]:

> Students, many of whom were university teachers (who came as vacation workers) were put on problems connected with the scattering of light. In 1923, the study of the scattering of light in water was taken by K.R. Ramanathan. Sunlight was focused on the light contained in a flask, and the scattered light was seen as a track in the transverse direction. Even from the beginning, Raman's intuition seems to have told him to look for a change in color in scattering. By the proper use of a system of complementary filters, a "weak fluorescence" was detected in the scattered track. This was attributed to impurities in the liquid. Ramanathan wrote much latter: "Raman was not satisfied with the explanation that it was due to the fluorescence. He felt that it was characteristic of the substance and wondered whether it might not be akin to the Compton effect in X-ray scattering" (which had been just discovered that year). At the insistence of Raman, the liquid was purified again and again but the effect persisted. The "weak fluorescence" also showed polarization effect, but Raman did not, for some strange reason, follow up this important clue as he did later in 1928. In 1924, the "weak fluorescence" was again observed by K.S. Krishnan, and in 1925, Raman asked S. Venkateswaran to try to obtain a spectrum of this "weak fluorescence", but no spectrum could be recorded. Raman saw this "feeble fluorescence" as a disturbing effect superposed on the classical scattering of light. It is interesting that Compton too attributed the softening of X-rays by scattering to what he called a "general fluorescence radiation" almost in the manner Raman labeled the phenomenon he observed as a "special type of feeble fluorescence". Because of the close analogy with the Compton effect, Raman became interested in X-ray scattering again.
>
> Raman (along with Ramanathan) had broken new ground in the field of X-rays scattering in liquids in 1923. He showed that scattering at very low angles was governed by the Einstein–Smoluchowski fluctuations. For explaining the scattering at larger angles, the discrete structure of medium must be taken into account. For this, the distribution of matter in the fluid must be analyzed into a continuous "structural spectrum" which has its peak of intensity at a wavelength equal to the mean distance between the neighboring molecules. Raman once said: "We were so preoccupied with light scattering that we did not apply the idea of Fourier transformations to X-ray scattering of liquids although we were so close to it". This was done later by Zernicke and Prins.
>
> Raman attempted to understand the Compton effect from the point of view of the classical wave theory. In this process, he derived what is now known as the Raman–Compton formula. It was then that the true nature of the "feeble fluorescence" phenomenon became evident to him. The Compton effect could be considered as due to kind of "fluctuation" in the state of the scattering atom in the field of radiation. If much milder fluctuations were possible, they should give rise to a change in wavelength in the light scattered by the molecule. He was more convinced than ever that the "weak fluorescence" phenomenon was the optical analogue of the Compton effect.
>
> So he pressed on with the experimental study of this phenomenon. S. Venkateswaran, a part time worker in his laboratory succeeded in purifying many organic liquids by slow distillation in vacuo and observed a greenish blue track in pure glycerin and the fluorescence was strongly polarized. This clearly indicated to Raman that this phenomenon could not be the conventional fluorescence—a point of view he had always taken and for which he was seeking proof. Venkateswaran was a part time worker who could work only after working hours and on holidays. Raman wanted some one to use the sunlight available all through the day…. And so he persuaded K.S. Krish nan, the best student he had at that time, to get on to these experiments. With Krishnan, Raman observed that all pure organic liquids available in the laboratory showed this "feeble fluorescence" and he was convinced that this was the modified scattering of altered wavelength corresponding to his "milder fluctuations" in the state of the scattering molecule and in fact due to the Kramers-Heisenberg process. The real discovery of the Raman Effect took place on 28th of February 1928 when Raman pointed a

> direct vision spectroscope on the scattered track and saw that the scattered light contained not only the incident colour but at least one another, separated by a dark space.

As we see, from the beginning, Raman proceeded from an analogy with the Compton effect. In turn, Mandelstam and Landsberg proceeded from the wave considerations, from the concept of modulation. Finally, both groups came to the explanation on the base of the Kramers–Heisenberg theory.

We also see that Raman was the leader of a group of Indian physicists, and he had to overcome the organizational problems.

Although Russian literature gave Raman his due, he was constantly criticized that he kept silence on Soviet physicists' achievements. This is true. Raman has not mentioned Mandelstam and Landsberg in his Nobel Lecture. Moreover, he has not mentioned Mandelstam and Landsberg in his scientific articles. Their names are absent in the author index of the first volume of Raman's "Complete Works" (this volume has been dedicated to the scattering of light).

It is possible, however, to find arguments in "defense" of Raman. In his Nobel Lecture, he mentioned his collaborators. In his articles, he referred to those writings which influenced him.

7.4 The Nobel Prize

So, combinational scattering (in the majority of countries called Raman scattering) was discovered in 1928 by Indian and Russian (Soviet) scientists, at almost the same time. In 1930, the Nobel Prize for physics was awarded to the Indian scientist Raman, while the Russian scientists were rejected.

This Nobel Committee's decision has been discussed by Russian scientists I.L. Fabelinsky and E.L. Feinberg (their writings were cited many times above) and by R. Singh and F. Riess in their historical article in 2001 [316]. I.L. Fabelinsky and E.L. Feinberg emphasized the moral aspect of the decision of the Nobel Committee, R. Singh and F. Riess treated the problem from the point of view of the technology of such decisions.

I.L. Fabelinsky pointed out that "any research carried out by Landsberg and Mandelstam was always very careful and thorough, guided by a clear understanding of the effect under study, and they did not rush to publish their results". As a result, Raman promptly published a report of his discovery on March 31, 1928. The Russian scientists, unfortunately, were in no hurry to report their discovery of the effect. News of their discovery reached print only in July.

N.D. Papalexy and E.L. Feinberg said approximately the same [1, Vol. 1, p. 28, 103, p. 26].

I.L. Fabelinsky pointed to the political aspect of the Nobel Committee's decision. I.E. Tamm also emphasized the "political reasons" [336]. The combinational scattering of light is the great achievement of Soviet Physics that has never been recognized with an international prize. A.M. Blokh provides a short review of the

Soviet scientists' statements that the Nobel Committee's decision showed a lack of respect for Soviet science in some Western circles [49].

E.L. Feinberg also referred to the following event (which he described from what others had told): Once L.I. returned home from Landsberg laboratory holding in his hands a photographic plate, still wet after development, and told his wife with a squirming smile: "Imagine, Mizya (an interfamily name of Lydia Solomonovna), one gets Nobel Prizes for such things". L.S. replied indignantly: "How could you think about such things when Uncle Lyova is in jail and they have already stopped taking parcels!"

"Uncle Lyova" is L.I. Gurvich, a close relative of A.G. Gurvich, who was mentioned in Chap. 2.

L.I. Mandelstam took some steps; he visited Vyshinsky who was Rector of MSU then.

"Raman did not have such problems, hence his unrestrained strive for a Nobel Prize", E.L. Feinberg concluded [103, p. 31].

In their article, R. Singh and F. Riess tried to restore the chain of events by basing themselves on documents. Raman was nominated by a number of physicists; among them, there were so great physicists as N. Bohr, E. Rutherford. Raman was known among Swedish physicists.

Mandelstam and Landsberg were nominated by O.D. Chvolson, the Russian prominent physicist, an author of a five-volume course on physics (he was mentioned in Chaps. 2 and 3). Raman was also nominated by Chvolson.

Mandelstam was also nominated by N.D. Papalexy (Landsberg for some reason had not been nominated by Papalexy).

In his nominative letter, Papalexy emphasized that Mandelstam had theoretically predicted the combinational scattering of light in his early optical writings. Papalexy wrote "Starting from theoretical considerations about the necessity of the occurrence of scattered radiation caused by thermal motion with frequencies which are different from the frequency of the incident monochromatic radiation, L. Mandelstam looked for a possibility to ascertain this effect experimentally since 1918… Afterwards these attempts had led to the discovery of combinational scattering".

The Nobel Committee rejected what is implied by Papalexy's claim cited: It rejected that Mandelstam had predicted the new effect.

R. Singh and F. Riess write as a conclusion: "Raman's example shows that to be nominated for the Nobel Prize, contacts with renowned scientists play a decisive role. Raman's nomination by the renowned physicists and Nobel Laureates like Rutherford, Bohr, Stark strengthened his case, whereas the prospects of Landsberg and Mandelstam (who were nominated by their own countrymen only) were poor" [316, p. 279].

Chapter 8
The Scientific School of Mandelstam. The Early Steps and Results

8.1 What Is a Scientific School?

In the Introduction, it was said that the Mandelstam School is one of the basic scientific schools of Soviet physics. The preceding chapter described how the community which can be called the Mandelstam School started to manifest itself. It was noted that in 1925, Mandelstam had already got clever and intellectual graduate students: M.A. Leontovich, A.A. Andronov, S.E. Chaikin, and A.A. Vitt. This chapter will follow some aspects of the interaction of Mandelstam with his students: It will follow how Mandelstam would formulate problems to put them before his students, how the students consulted Mandelstam in the course of their work, how they together corrected the formulations of the problems, and how this collaborative work led to new problems.

However, first of all, the author would like to make more clear the concept “scientific school”. The word “school” usually means a scientific community constructed according to the principle “a teacher, the teacher’s students (“students”, i.e., the graduate students who perform research under the guidance of the teacher), and the students of the teacher’s students”. Usually, this is an informal community: The teacher may be an administrative chief, but this is not necessary. As a matter of fact, the teacher and his disciples can work for different scientific institutes.

M.A. Leontovich, A.A. Andronov, S.E. Chaikin, A.A. Vitt and subsequent graduate students G.S. Gorelik, S.M. Rytov, M.A. Divilkovskii, and S.P. Strelkov form together the historical phenomenon which is called the Mandelstam School.

Nevertheless, the term “scientific school” should be used cautiously. Too many evaluative, journalistic, declarative connotations have been accumulated in this term. Let us take the following definition: “A nonformal creative community of very qualified researchers who are integrated by the common approach to scientific problems and by the common style of work and thinking. A scientific

A. Pechenkin, *L.I. Mandelstam and His School in Physics*,
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school has its own scientific program and its scientific results have public acknowledgement" [80, pp. 3–4]. [1]

Such a definition does not take into consideration the historical dynamics of scientific schools. What do such qualifications mean—"very qualified", "having public acknowledgment"? Nobody can beforehand be doomed to high qualification and public acknowledgment. The "common approach" and "scientific program" should be elaborated in the process of cooperative research. Besides, the "common approach" is not always essential. Sometimes, the moral authority and erudition of the teacher are more important. The school can be based on the discussions and mutual criticism. The former graduate students may argue with their teacher.

By considering the phenomenon of a scientific school, it is worth to turn to the German science of the nineteenth century. There are reasons for this. In the nineteenth century in Germany, science mainly came unwound within universities. The German universities basically were research universities: From the beginning of the nineteenth century, they contained research laboratories and studies. In 1860–70, the phenomenon which is called by the American historian D. Cahan as the "institutional revolution" took place: In the process of the development of industry and science, the research institutes arose in the structure of German universities (we wrote about it in Chap. 2). In the 1860s, the big research institute arose within Berlin University and then in Munich University. As was noted, the Strasbourg University had not passed through an "institutional revolution". From the very beginning, it corresponded to the pattern of a "research university" which contained research institutes in its structure. The research institute, for which Mandelstam and Papalexy worked, was one of them.

Teaching is a transfer of knowledge from a teacher to students. It is a characteristic message of German universities: transfer knowledge in the process of research, a professor and his student together solve a research problem. A textbook in this process has its place, but its function is secondary.[2]

In Germany, a special scientific research institute did not exist until the end of the nineteenth century. In 1887, the Physikalisch-Technische Reichsanstalt was established in Berlin. Herman von Helmholtz, who was a professor of physics at Berlin University, became Director of this Institute. This was the first important scientific establishment oriented to research rather than to education.

So, the very organization of research in Germany led to the development of scientific schools. In Chap. 2, we spoke about the scientific school founded by Ferdinand Braun. However, this was not a paradigmatic scientific school. Let us recall that the author of the book about F. Braun is rather skeptical with respect to very existence of F. Braun's school. Let us turn to the widely recognized school, the school which was founded by Hermann von Helmholtz. Among Helmholtz' doctoral students, there

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[1] Mikulinskii et al. [233] provides a very extensive conception of scientific schools. This book aims to embrace a wide variety of what can be called "scientific school".

[2] "Development of German universities in the first half of the XIX century can be treated as the first important step on a way of emergence of the appropriate organizational basis for scientific professions. Training and research function, especially in postgraduate studies, appeared combined" [328, p. 106].

were Albert Michelson, Wilhelm Wien, Ronald Wundt. Let us concentrate on the relations of Helmholtz with Heinrich Hertz (1857–1894, these relations can be followed along their correspondence). The Helmholtz-Hertz creative communication had not stopped in 1880, when Hertz completed his dissertation and passed through the final examination. This communication had not stopped, too, in 1883, when Hertz left Berlin University, where he worked under Helmholtz at Physics Institute [157].

Hertz's 1884 letters to Helmholtz "made two things clear: Hertz still retaining his admiration for Helmholtz's ideas, but on the question of electromagnetic waves, he much preferred Maxwell's theory; his conviction was growing that electromagnetic waves traveled at the speed of light. As a consequence, he felt the need for a definitive measurement of the speed of electromagnetic waves at nonoptical frequencies, and he devoted much thought to ways of carrying out such a measurement if the necessary equipment became available…".

During the course of Hertz's electromagnetic research in the years 1885–1888, letters were frequently exchanged between Hertz in Karlsruhe and Helmholtz in Berlin. Of the nine research papers published in *Annalen* between 1885 and 1888, four were sent by Hertz to Helmholtz to be first presented by him to the Berlin Academy of Sciences….

Hertz's letters to Helmholtz during this period are very differential, as might be expected of a former student writing to his mentor. They frequently point out how grateful Hertz is for Helmholtz's contribution to his research success. Thus in a letter of 5 December 1886, detailing some of the results contained in his paper "On very rapid electric oscillations", Hertz writes:

"I should like to take this opportunity to let you know about some experiments that I have recently successfully completed, since I had hoped from the beginning that they might interest you".

A very interesting letter arrived from Hertz dated 5 November 1887. It contained a request for Helmholtz to submit to the Academy the paper in which Hertz was finally able to show the effect of electromagnetic field on the dielectric polarization of insulators. This was the same research topic Helmholtz had suggested to him at the beginning of his university studies in Berlin.

Helmholtz was always equally gracious in his replies and enthusiastic about transmitting Hertz's paper to the Academy. To his letter mailed on Friday, November 5, Hertz received a postcard reply on Tuesday, November 9, that reads: "Manuscript received. Bravo! Will hand it on to be printed on Thursday. H.v.Htz" [243, pp. 44–45].

On 6 August 1889, Helmholtz's son Robert, died, leaving his father devastated with grief. Robert had also been a physicist, and his father had expected great things of him. It must have consoled Helmholtz a little to travel to Heidelberg the following month to hear his "second son", Hertz, delivering his famous address, "on the relations between light and electricity". The evening of the talk Helmholtz wrote to his wife:

"Today we had the address from Professor Hertz; it really was extraordinarily good, very polished in style, tactful, and tasteful, and called out a storm of applause" [243, pp. 45–46].

Let me stop quoting. The above history shows that a scientific school does not imply a conformity of ideas. The ideological contradictions between the teacher and his former student may be productive for research. It is important moral and even existential teacher–student interaction. The teacher helps his former student in his career. The teacher is a source of the positive emotion for his former student.

The Helmholtz-Hertz relations could be considered on the level of their philosophy of science. Their positions are very different, but there is a continuity between their world views. Let me cite the conclusion of M. Heidelberger's article: "Hertz's philosophy of science seems to be a consistent extension of elements already present in Helmholtz's philosophical conceptions. Hertz's contemporaries viewed his philosophy of science from various vantage-points. First of all, they saw the student of Helmholtz who wanted to reduce contiguous action to the motion of concealed masses. Then they saw Hertz as an empiricist opponent of metaphysics who insisted that theoretical concepts can have no a priori and metaphysical justification; they have to be based on experience in order to have meaning at all. And then, there is Hertz, the anti-mechanist who did always with distance force in electrodynamics as an old fetish of mechanicist physics and provided a clean and comprehensive picture of pure electromagnetic field" [233, p. 33] (in the last sentence N. Wise' article is cited [383, p. 354]).

Thus, by characterizing a scientific school, it is useful to follow the philosophical conceptions which unified the teacher with his students. This helps us to make clear the ideological connections between them. In Chap. 2, we started to do it by describing F. Braun's philosophy of science. In the following chapters, we shall continue: We shall outline how the philosophical positions of Mandelstam and Papalexy are represented and reproduced in the philosophical positions of their graduate students and then collaborators. We shall follow the ideological sequence: F. Braun, Mandelstam and Papalexy, Mandelstam's graduate students.

"Scientific school" is rather blurred conception. But it allows us to notice such details of historical reality that would be unnoticed if this concept is absent. Besides, by designating the different types of scientific schools, we make this concept more interesting.

Scientific schools became the widespread form of organization in Russian science and then in Soviet science, since in the second half of the nineteenth century, the German system of a research university was applied in Russia. As early as the first half of the nineteenth century in Russia, the main success in physics was provided by Physics Laboratory of the Academy of Sciences. At the beginning of the second half of the nineteenth century, a number of energetic reforms promoted the development of university physics: A number of talented students were sent abroad to improve their education and scientific qualification, new chairs of physics were established, and financial maintenance was increased. According to the new University Law (1863), the positions of Ordinarius and Extraordinarius (ordinary and extraordinary professors) implied Doctor Degree which could be reached through a research work.

The first Russian scientific schools in physics were founded by A.G. Stoletov and P.N. Lebedev who both started in Germany [233].

The Soviet organization of science also promoted the formation of scientific schools. A.B. Kojevnikov's observations should be taken into account. Kojevnikov believes that under the condition of totalitarian power, it is natural for scientists to be amalgamated with each other around an authoritative professor.

> "The authoritative scientist, Kojevnikov writes, if he occupied a stable position, created a protective area around him. He was able to defend his disciples and help his disciple in his career. Due to such a style of life, a number of fields of science (for example, theoretical physics) have survived the period of isolation and preserved the high standards of scientific activity for the following generations of scientists. Nevertheless, there was the reverse of the medal. Too many things depended on the personality of the leader, too much was determined by his status, his preferences which could become out of date played a decisive role" [179, p. 236].

L.I. Mandelstam came to MSU in 1925. This was time of enthusiasm and romanticism. Young people came to science in order to serve for the truth which was associated with Revolution and with the materialistic world view. Scientific research was considered by them as a part of the great work of Soviet people.

Science is impossible without discussions and sharp criticism. Such discussions were typical for the end of the 1920s and the beginning of the 1930s. Some scientific discussions got the status of philosophical ones. An example was the discussion concerning the Richard von Mises frequency conception of probability (see Chap. 7).

True, the governmental power became to direct philosophical discussions at the end of the 1920s. This power first supported the "dialecticians" guided by A.M. Deborin and B.M. Hessen. Later, this power supported the "materialists" who were treated as the proponents of "mechanist physics" by the "dialecticians" (the main figure among the "materialists" was A.K. Timiriazev—he was mentioned at the beginning of Chap. 6).

The Mandelstam School was formed as an open scientific community. To enter this community, one needed to show his scientific qualification and ability to communicate with the other members of the community.

Not only L.I. Mandelstam's ideas were in the centrum of discussions and theoretical activity in this community. As we shall see, Mandelstam's former students put forward their own ideas and Mandelstam participated in the development of these ideas.

L.I. Mandelstam helped his graduate students and his former graduate students. In particular, he wrote the references for them. It is possible that he recommended some of them in his verbal communications. However, he was open for people who did not belong to the community which was called the Mandelstam School. For example, the Mandelstam file in the Archives of the Academy of Sciences keeps a letter of reference written in favor of a person who was ideologically very distant from the Mandelstam School. This was A.S. Predvoditelev, the supervisor of A.A. Vitt's diploma work. In 1936, I.E. Tamm sharply criticized A.S. Predvoditelev's theoretical writings. Tamm's criticism was published in "Journal of experimental and theoretical physics" directed by Mandelstam and A.F. Ioffe as editors in chief [332]. True, this letter of reference was probably written at the beginning of the 1930s, but Predvoditelev did not belong to the Mandelstam circle in those years, either. Mandelstam's letter of reference emphasized Predvoditelev's experimental achievements (this letter had not written by Mandelstam's hand, but other letters of reference, say for G. Gorelik, had not written by Mandelstam's hand, either).

During the second half of 1930, when L.I. Mandelstam and the majority of his former students moved to Academy of Science Institute of Physics (FIAN), the character of Soviet science changed: Soviet science became more bureaucratic, a break between research and education arose (see [343, 359, 360, 361]). A reinterpretation of moral norms took place. However, we find ourselves in the second half of the 1920s, yet. G.S. Landsberg conducts his great experiments near Mandelstam's apartment. Lidia Solomonovna Mandelstam maintains the mercury lamp luminescence. Andronov and Leontovich made friends and discuss their collaborative work. A.A. Vitt sits on the bench in the public garden and smokes half a pack of cigarettes in the process of solving a problem.[3]

8.2 Mandelstam L.I., Landsberg G.S., Leontovich M.A. Research in Optics, 1925–1929

M.A. Leontovich, who became a researcher of second class at MSU Institute of Physics in 1925 (as was noted, this position in essence meant that he was a graduate student), helped Landsberg and Mandelstam in their experiments which led to the discovery of the combinational scattering of light. His work exceeded the laborant duties. The Mandelstam-Landsberg-Leontovich article published in "Zeitschrift für Physik" in 1929 makes this clear. The article was theoretical, and L.I. Mandelstam had played the leading role in this project. The experimental articles on the combinational scattering were signed in the following order—Landsberg and Mandelstam.

Landsberg and Leontovich published an article on their experimental research in the same year [201].

The Mandelstam-Landsberg-Leontovich article is dedicated to the classical explanation of the combinational scattering of light. In this article, the positive heuristics which led Mandelstam and Landsberg to their discovery had been developed. As was noted (Chap. 7), they proceeded from the wave picture of the scattering of light, and in their discussions, the effect of modulation played the decisive role. In contrast, C. Raman proceeded from an analogy with the Compton effect.

Here is the summary written by the authors [1, Vol. 1, p. 324]:

> The scattering of light in crystals is interpreted as the diffraction by elastic oscillations of a crystal. Under scattering the frequency of light changes by an amount ω_0 (ω_0 is the frequency of elastic oscillations which are decisive for the process of scattering). If the oscillations belong to the acoustic spectrum of a crystal, this change in frequency is small and depends on the direction of scattering. If this oscillation belongs to infrared spectrum, we deal with the combinational scattering (the change in frequency is relatively large and does not depend on the direction of scattering).

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[3] On S.P. Strelkov's recollections (cited in [37]).

Fabelinsky, whose description of the Landsberg-Mandelstam discovery was cited in Chap. 7, traced the following line: “In the first half of 1928 Landsberg and Mandelstam had carried out a complete experimental study, that they understood their results correctly, and that their quantitative results were explained quantitatively in quite a modern manner. With regard to the frequencies and positions of the lines, the problem had been completely solved. In their article 1928 Landsberg and Mandelstam discussed the intensities of the red and violet satellites and offered a correct qualitative explanation, which subsequently found quantitative theoretical development in the work of Mandelstam, Landsberg, and Leontovich” [99, p. 72].

The collaborative Mandelstam-Landsberg-Leontovich article was not the first article on optics by M.A. Leontovich. His first article on optics was written by him together with A.A. Andronov (1926). The topic was suggested by L.I. Mandelstam who went on to think over his Strasbourg problem—the scattering of light by the rough surface of a liquid (this roughness resulted from fluctuational inhomogeneity). As Fabelinsky wrote, “this article contains the most general formulae for the intensity of the scattering of light, the scattering at any angle” [99, p. 72].

About his article, written collaboratively with M.A. Leontovich, A.A. Andronov wrote the following: “Our work contains the calculation of the intensity of light scattered by the surface of liquid and the comparison of our results with the results obtained by Raman and Radames. Our work is a generalization of Mandelstam’s work published in 1913” [17, p. 156].

M.A. Leontovich became a famous theoretician. However, in the course of his research together with Mandelstam and Landsberg, he also worked as an experimentalist. Having published his article collaboratively with Andronov, Leontovich “came to the idea that the formulae obtained for the intensity of light scattered by the fluctuational inhomogeneities can be applicable in the case of light scattered by statistical inhomogeneities, for example, by a fine matt surface of glass.

I had to prepare such matt surfaces in order that inhomogeneity would be less than the length of light wave” (as it is cited by I.L. Fabelinskii [99, p. 73]).

M.A. Leontovich’s research on molecular optics (under Mandelstam and partially together with Mandelstam) became the base of his subsequent work in molecular acoustics and statistical physics (partially with Mandelstam [1, Vol. 2]).

8.3 The L.I. Mandelstam and M.A. Leontovich Article on Quantum Mechanics (1928)

The Mandelstam-Leontovich article “On the theory of Schrödinger equation” was published in “Zeitschrift für Physik”. This was one of the first Soviet articles on “new” quantum mechanics. Now, the results of this article have been absorbed by textbooks, and they became classics. It is useful therefore to take an original formulation of the problem under consideration. As the authors emphasized, the article put the fundamental problem—the behavior of the potential energy $V(x)$ under $x \to \infty$.

And they deal with a simple case: a system with one degree of freedom. To describe the behavior of potential energy means to solve the Schrödinger equation with such a behavior of the potential energy and to find the corresponding wave functions.

However, two contradictions arose. Mandelstam and Leontovich took an oscillator whose potential energy had a parabola form, $V(x) = \alpha x^2$, where x is a spatial coordinate. Here, Schrödinger equation gave a discrete spectrum. But a small change—a replacement $V(x)\alpha x^2$ by $V(x) = \alpha x^2 \mathrm{e}^{-kx^2}$, where k is positive and small, drastically affected the situation: The Schrödinger equation gave a continuous spectrum. From the physics point of view, this is not the case. "From a physics consideration one should expect that the behavior of $V(x)$ at infinity (if we don't take too high energy levels) should influence the phenomena only a little".

Mandelstam and Leontovich used the particle picture to interpret this solution. If a particle is located far on the left or on the right from the origin of coordinates, a particle can have any positive value of energy. They came to the conclusion that their Schrödinger equation "embraces two problems: an oscillator and a free particle, and the discrete spectrum of the first problem partially overlaps the continuous spectrum of the second problem" [1, Vol. 1, p. 289].

Mandelstam and Leontovich described the structure of wave functions of the system. If the system behaves itself like an oscillator, the conventional waves are considerable in the area of the origin of coordinates, within the bounds of parabola $V(x)$. If it is not the case, we "see" the trains of waves located either on the right or on the left from the potential barrier (from $x : V(x) = 0$). These waves become weaker by passing through the potential barrier. If waves with a big amplitude arise on the right, then on the left their amplitude is less, and vice versa.

I.E. Tamm wrote that the Mandelstam-Leontovich article contains, in essence, the foundations of the theory of how a particle goes through a "potential barrier", through the domain, in which its kinetic energy is smaller than its potential one. Mandelstam and Leontovich came to a phenomenon which is completely impossible in classical physics [333, p. 133].

This statement became standard for Russian history of physics. Let us, however, pay attention to the citations which Tamm's article contains. "The oscillator which is characterized by the potential energy in the form $V(x) = \alpha x^2$ has discrete energy levels. If in the beginning the potential energy increases proportionally to x^2, but after that, far from the origin of coordinates, starts to decrease and tends to zero under x, then the energy of such a system (Mandelstam was the first who emphasized this) can have any positive value".

Further, Tamm cited Mandelstam's statement which is absent in the Mandelstam-Leontovich article, but is directly related to this article: "On the other hand, from the point of view of physics, it is clear that the behavior of the potential energy at infinity cannot be essential for a particle which does not have a large energy. Moreover, a physical theory can be applied to reality if and only if its results do not in essence depend on how we extrapolate to infinity (for example, how we extrapolate the dependence of the potential energy from a spatial coordinate). The thing is that the realistic value of the potential energy depends on the accidental position of foreign bodies".

Tamm probably cited his notes of Mandelstam's lectures and seminars. He commented: L.I. Mandelstam gave an explanation of this contradiction. "I cannot go into details".

Which details? The Mandelstam-Leontovich article was the starting point of Mandelstam's reflections on the concept of energy, the reflections to which Tamm had joined. These reflections led to the Mandelstam-Tamm article on the energy-time uncertainty relation. The behavior of the potential energy far from the bound of the potential well is not essential? Here, the factor of time takes effect.

The Mandelstam-Tamm article was published in 1945 after Mandelstam's death.

This article is presented in Wikipedia's article "I.E. Tamm".

The Mandelstam-Leontovich article is not mentioned in the main books on the history of quantum mechanics (Van der Waerden. Introduction. In: "Sources of quantum mechanics"; M. Jammer. "The conceptual development of quantum mechanics" [165], F. Hund. "Geschichte der Quanten-Theorie"). This article is mentioned in M.A. Eliashevich's historical review of the papers following the basic articles of W. Heisenberg, M. Born, P. Jordan, E. Schrödinger [96, p. 703]. However, the historians of quantum mechanics usually referred to George Gamov's theory of the alpha decay of a nucleus via tunneling through "the wall of the potential well" (1928).

I.E. Tamm recalled that G. Gamov, who immigrated from the USSR in 1933 (the Institute of Physics regulation to dismiss Gamov in connection with his departure abroad is dated by 13 November 1933), said to him that he was completely based on the Mandelstam-Leontovich article in his theory of the alpha decay. Gamov, however, does not refer to this article. In his autobiography "My world line", he respectfully writes about Mandelstam, but does not provide comments about the contribution of Mandelstam to his own problems and achievements [126].

In 1929, Tamm and Leontovich published an article on Einstein's unified field theory [341].

8.4 Andronov–Leontovich and Mandelstam–Andronov–Leontovich

As was noted, two Mandelstam's graduate students, A.A. Andronov and M.A. Leontovich, met, became friends, and started to collaborate. As was also noted, their first collaborative article dealt with the problems of scattering of light. After Leontovich was engaged in research on the theory of a buzzer—electric bell, Mandelstam worked with the buzzer by elaborating the absolute method of calibration of cymometers—Chap. 5, Sect. 5.1). The next Leontovich research was conducted together with Andronov, and it was dedicated to the phenomenon of parametric resonance (1928). The parametric resonance is a resonance in a generalized sense of the word. This is not a resonance under the action of the periodical outside force. An oscillatory system shows such a resonance if some of its parameters are varying periodically in time. The paradigmatic example: a child pumping a swing by periodically standing and squatting to increase the size of the swing's oscillations. The varying of the parameters drives the system.

Andronov and Leontovich took the Mathieu equation under study. A mathematical pendulum with the vertically oscillating point of suspension was taken as a model. Notation: x is the angle of deflection, l is the pendulum length, and the point of suspension is oscillating according to the law $z_0 = a \cos pt$.

The equation ϕ of the pendulum oscillations is the following:

$$\ddot{x} + \frac{1}{l}\left(g - ap^2 \cos pt\right)x = o$$

or, after a simple transformation,

$$\ddot{x} + \left(\omega^2 - \frac{a}{l}p^2 \cos pt\right)x = 0$$

where ω is the eigenfrequency of the system.

A similar equation describes an action of the circuit with the periodically varying capacity of the condenser. For example, such variation can be reached by mechanical shifts of the condensator plates.

In essence, the Andronov-Leontovich article was concerned with the theory of equations of mathematical physics (the equations with alternating parameters). The general solution of the Mathieu equation was well known. However, to find the physical sense of this solution, one should take the problem of its stability under consideration. Andronov and Leontovich used the Lyapunov theory of stability. In the perspective of Andronov's further research, this turn to the Lyapunov theory was very significant.

Andronov and Leontovich found out how the stability changed if ω/p is under variation. They described the areas of stability and nonstability. "What sense do the areas of nonstability have?" N.D. Papalexy asks and writes: "if one periodically varies a parameter of the real oscillatory system (say, the length of a pendulum), and if the frequency of eigen oscillations of a system adjusts on the frequency of parameter variations (or on half, quarter,… of this frequency), divergent oscillations arise in the system at an initial perturbation. In other words, the system responds to an external action and a resonance, which can be called 'parametric,' arises in it" [265, p. 350] (see also [264]).

From the point of view of the parametric resonance, the procedure of modulation can be interpreted. As was noted in Chap. 6, the Mandelstam-Papalexy research on the problem of modulation methodologically influenced Mandelstam's graduate students subsequent research on parametric resonance. Really, the frequency modulation can be treated in the terminology of parametric resonance: Information, which is broadcast, is taken as a periodically varying parameter. The carrier frequency is taken as the natural frequency of an oscillatory system.

The article on the parametric resonance was followed by the three authors' paper on adiabatic invariants (1928). These three authors were A.A. Andronov, M.A. Leontovich, and L.I. Mandelstam.

In thermodynamics, as is well known, a process going on without heat exchange with the environment is called adiabatic. The adiabatic process is an abstraction, but processes which are going on with high velocity approach the adiabatic process. The article "On the theory of adiabatic invariants", however, was about the concept of adiabatic invariance, which has developed in quantum theory. Here, the term "adiabatic" is used in a "shifted" sense of the word. We call processes adiabatic, if they occur under a slow change in the external environment or a slow change in the parameters that characterize the system. "Slow" is slow compared to the periodic changes of the system; for example, pendulum swing is adiabatic in the case of a slow change of the parameter—the suspension point of the pendulum. Under an adiabatic change, the system does not lose stability.

G.S. Gorelik explained the situation with the three authors' article as follows. "To some extent the problem arose in the old quantum mechanics. For this theory it was important to find out which processes are developing in the system (in particular, in an oscillator) under a slow change of its parameters. With his subtlety of mind L.I. Mandelstam suspected.

L.I. Mandelstam suspected that the problem is not so simple as it is usual considered. Even under a very slow variation of the parameters the system may show a resonant increase of its oscillations. He connected this problem with Melde's experiments (by periodically changing the tension of a string, it is possible to excite transversal oscillations in it with frequencies half as large as those due to changes of the tension frequencies) which were considered by Lord Rayleigh" [138, pp. 143–144].

A.A. Andronov published the following summary of his article written together with Mandelstam and Leontovich: "The behavior of a pendulum in the field of periodically varying force of gravity is considered, a need to make some formulations of the theory of adiabatic invariants more precise is established, the definitions of temporal and stationary adiabatic invariants are formulated" [17, p. 526].

Roughly speaking, a temporal adiabatic invariant is sensitive with respect to the rate of variation of parameters. If such a sensibility is absent, an invariant is called a stationary one.

As was noted, in 1927, L.I. Mandelstam wrote a common report about the work of his graduate students—Andronov and Leontovich in 1926/27. After Mandelstam begun to write individual reports on Andronov and individual reports about Leontovich, Andronov and Leontovich began to work separately. Andronov concentrated himself on the general problems of the theory of oscillations. Leontovich took optics and molecular physics as his subject matters.

8.5 Parametric Generators

In the development of the idea of parametric resonance, Mandelstam and Papalexy produced a number of engineering innovations. "At the beginning of 1931 L.I. Mandelstam and N.D. Papalexy, the biography of Mandelstam says, constructed the first parametric machine—a parametric alternator, which was fundamentally different from well-known ones. The difference consists in the following: for a parametric machine both inductance and capacitor are essential. It is also essential to maintain the definite relation between the period of variation of these parameters and their values.

The first parametric machine was constructed on the principle of the periodically varying inductance. In 1932 the machine in which the capacity periodically oscillated was constructed. A little later, under the guidance of Papalexy samples of the industrial generator have been constructed. On a number of characteristics these generators surpassed the standard generators of electrical engineering" [1, Vol. 1, p. 46].

For an English translation of the Mandelstam-Papalexy 1933 article "Oscillations in an Electrical System Energized by Means of Periodically Varying Capacities", see: http://www.tuks.nl/pdf/Reference_Material/.

In his textbook, G.S. Gorelik provides the following description of parametric machines [138, p. 120]:

If the capacity periodically changes with the frequency which is close to the doubled frequency of the circuit, the oscillations start to increase in this circuit.

However, it is possible to obtain the regime of a controlled stationary amplitude, if a resistance which strongly increases with the increase of current strength is connected in parallel with the circuit.

A contemporary textbook written by Papalexy's former student V.V. Migulin and his collaborators traces the subsequent development of the parametric machines and specifies what the biographers of Mandelstam write: "The first experiments on parametric resonance were carried on in the 1930s by mechanically displacing a ferromagnetic core inside the induction coil of an oscillatory circuit. Making use of the nonlinear dependence of the core magnetization on the current flowing in an auxiliary winding, it was possible to change a reactive parameter electrically. The world's first parametric machines (generators) based on these principles were proposed by Mandelstam and Papalexy. However, because of inevitable high losses due to hysteresis loop and low mechanical frequency of displacing a core, it was impracticable in those years to realize parametric regeneration within the radio frequency range.

Considerable progress in this field and in the theory of parametric phenomena was made in the 1950s when high-strength magnetic materials (ferrites) and parametric semiconductor diodes appeared" [232, p. 148].

Chapter 9
The Mandelstam School: Theory of Non-linear Oscillations

9.1 Mandelstam and the Theory of Non-linear Oscillations

The title of this section reproduces the title of A.A. Andronov's article included into the book dedicated 100 Anniversary of L.I. Mandelstam. The article had been written on the base of the lecture delivered by A.A. Andronov for the joint funeral meeting of the USSR Academy of Sciences and Professors of Lomonosov Moscow State University (22 December 1944).

The exact sense of the word "non-linearity" is explained within the framework of the theory of differential equations. A differential equation is called "non-linear", if it contains the non-linear combinations of an unknown function (or its derivatives). The non-linear ordinary differential equations with which Mandelstam's students originally dealt were ordinary differential equations containing non-linear functions of unknown function or its time derivative.

In this book, the term "the theory of non-linear oscillations" will be used to outline a historical phenomenon. In Mandelstam's school, this theory was regarded as an important branch of the general theory of oscillations. In turn, the theory of oscillations, driving toward the "oscillatory" unification of science, belongs to the characteristic intellectual messages of Mandelstam's school. To some extent, this idea was parallel to H. Barkhausen's "Schwingungslehre" [33] (see Sect. 2.5). However, L.I. Mandelstam and his disciples tended to take this theory more broadly (see Chap. 4, Sect. 4.8, here we reproduce some of the quotations). They included into the theory of oscillations not only study in electricity, radio physics, and mechanics of a point but also study in optics, acoustics, hydromechanics, chemical kinetics, and biophysics. One can follow how L.I. Mandelstam, who in his 1930–1932 *Lectures on oscillations* showed the "oscillatory mutual assistance" between different areas, which were concerned with oscillations, and regarded the theory of oscillations as an "important and specific branch of physics" [1, Vol. 4, p. 329, 2, p. 302], radicalized his approach in *Lectures on Some Issues in the Theory of Oscillations*, which he delivered but did not finish in 1944. In 1944, Mandelstam proclaimed that besides "national

A. Pechenkin, *L.I. Mandelstam and His School in Physics*,
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languages" of optics, electromagnetism, acoustics; etc., there is an "international" language of the theory of oscillations in physics (here "national" and "international" are metaphors: "national" means "restricted by a scientific discipline", "international" means "covering all physics"). He said that the theory of oscillations provided uniform understanding of a great variety of physical phenomena including quantum ones [1, Vol. 5, p. 408, 3, p. 402]. In referring to the English philosopher Alfred North Whitehead, Mandelstam claimed that the rise of theoretical physics was provided by applying the concept of periodicity to different phenomena (incidentally, Whitehead was the only philosopher who Mandelstam referred to in his published writings). As an extension of the Whitehead thesis, Mandelstam claimed that all the main important discoveries in physics were related to oscillations [3, p. 409].

The "oscillatory mutual assistance" was a metaphor expressing that approaches and methods from an advanced area of study in oscillations could be used to gain progress in another area of oscillatory phenomena. This metaphor can be traced back to F. Braun's emphasis on the unity of electrical and optical phenomena taken as oscillatory phenomena (Chap. 2, Sect. 2.5) and L.I. Mandelstam's practical methodology to apply radio-engineering analogies in optics (Chap. 4, Sect. 4.8 and Chap. 7, Sects. 7.1, 7.2).

Mandelstam's school made use of the idea of the general theory of oscillations in its struggle for social prestige. In 1931, the new chair of the theory of oscillations was established at the Physics Faculty of Lomonosov Moscow University, and Mandelstam held this chair. In 1934, the laboratories of oscillations and optics were organized in the Physics Institute of the Academy of Science, which had just moved to Moscow from Leningrad. These laboratories were headed by two physicists belonging to Mandelstam's community, by N.D. Papalexy and G.S. Landsberg. In 1931, the All-Union (National) Conference on oscillations, where Mandelstam's community of physicists also played the main part, was held [280].

In outlining the general theory of oscillations as a fundamental theory, L.I. Mandelstam and his collaborators and disciples attached great significance to the non-linear phenomena that were taken into consideration in the theory. Thus, the idea of the general theory of oscillations led Mandelstam's community of scientists to the claim that the theory of non-linear oscillations was becoming a fundamental part of theoretical physics. At the end of the 1920s and the beginning of the 1930s, the majority of physicists treated the non-linearity in the equations describing physical phenomena as a disturbance of their natural courses or deviation from them. In Mandelstam's community of physicists, the strong tendency arose to regard the non-linearity as a fundamental property of some important physical phenomena. Mandelstam and his postgraduate students (A.A. Andronov and A.A. Vitt, in the first place) turned the relation between linearity and non-linearity around: according to A.A. Andronov's lecture at the 1931 First All-Union Conference on Oscillations, "the linear problems in their relation to the nonlinear ones cover an extraordinary narrow particular case", "it is necessary to provide a restructuring of mathematical technique in usage, it is necessary to find the technique, which would be adequate to represent non-linear processes and, moreover, effective enough…" [16, pp. 85–86].

9.2 The Rise of the Concept of Self-oscillations

The concept of self-oscillations arose in the course of attempts to overcome the stereotype of linearity in science and state a rigorous non-linear theory of oscillations which took place in a transmitter. As Andronov himself wrote, in 1927 Mandelstam put before him the problem to analyze mathematically the stability of motions (trajectories of differential equations describing non-linear oscillatory systems) yielded by the method of "matching" ("pripasovyvania") and place under this method a reliable mathematical basis [16, p. 454, 18, p. 112]. As is well known, the problem of an exact integration of non-linear differential equations is very difficult and has been solved only for some particular cases. The method of "matching" was popular in the 1920s: rough approximation methods to calculate trajectories of non-linear differential equations. This method runs as follows. An original non-linear equation is replaced by a number of corresponding linear equations; the non-linearity in the original differential equation is taken into account as "integration constants" which are "matching" in the points of conjugation of the different linear equations. This "matching" proceeds from the requirement of continuity of a solution of the original equation.

To make it clear, let us discuss a very simple case. Let the system be described by the following linear ordinary differential equation:

$$m\frac{\mathrm{d}^2x}{\mathrm{d}t^2} - kx = f,$$

where $f < 0$, if $\mathrm{d}x/\mathrm{d}t > 0$ and $f > 0$, if $\mathrm{d}x/\mathrm{d}t < 0$.

Let us solve this equation by employing "matching". Let at an initial time $\mathrm{d}x/\mathrm{d}t > 0$ and hence $f < 0$. Up to a time $t = t_1$, when the velocity decreases to zero, the system is described by the equation with $f < 0$. The return motion of the system is described by the equation with $f > 0$, with the initial condition being the value of the position and velocity at the end of the previous motion. Thus, we are "matching" the initial condition; that is, we take as the initial state of the motion the final state of the previous motion.

"Until now everybody, who applied this method (N.D. Papalexy, A. Sommerfeld, et al.)", Mandelstam said, "solved some specific particular problems leaving outside the problem of stability of the solution and its behavior at the initial values which differed from those corresponding to the periodic solution". He also said that in 1922 by applying "matching" Papalexy found the periodic solution for the differential equation describing a tube generator.

Although the problem of a rigorous mathematical treatment of "matching" had not been solved in those years, Andronov's work resulted in very important conceptual innovations: in his 1928–1929 papers and in his subsequent Ph.D. thesis a rigorous mathematical theory of the oscillations typical for a tube generator and manifest in many customary engines (say, a clock) and in living beings (say, beats of the heart) had been elaborated upon. Andronov had described these oscillations as essentially non-linear ones which could not be represented by adjustments to the linear theory. He had adopted the Poincaré-Lyapunov mathematical methods, the methods of the qualitative theory of differential equations, which were applied in celestial mechanics before (H. Poincaré's geometrical methods of describing the properties of the solutions of differential equations together with A.M. Lyapunov's methods for describing the stable motions). These methods permitted one to interpret non-linear oscillations rigorously and at the same time pictorially, to interpret them as geometrical figures on phase plane.

By combining a Greek "avto" ("auto") and a Russian "kolebania" ("oscillations"), Andronov referred to undamped oscillations in a dissipative non-linear system, which were maintained by external non-periodic energy source, that is, oscillations evident in tube generators, clocks, etc., as "avtokolebania" ("self-oscillations"). He related them to the Poincaré limit cycles on the phase plane: according to Andronov the mathematical image of self-oscillations was a stable limit cycle, an isolated path to which all neighboring ones tend in the course of time (Fig. 9.1).

A.A. Andronov's article "Predel'nye tsikly Puankare i teoriia kolebanii" ("The Poincaré Limit Cycles and the Theory of Oscillations") was published in *Comptes Rendus Acad. Sci.* (Vol. 189, N. 15, 1929, pp. 559–561) as communicated by Jacques Hadamard [15]. In 1928, a summary of A.A. Andronov's article was published in: *V s'ezd ruskih fizikov* [14]. (This conference was mentioned in Chap. 6, Sect. 6.7. Richard von Mises participated in this conference).

It is interesting that Mandelstam was firstly somewhat surprised by that mathematical development of method of matching that gave Andronov's results. Andronov writes in his recollections: "Mandelstam very attentively accepted my statement that the non-damping oscillations in the system with one degree of freedom turned out to be the Poincaré limit cycles. When my further mobilization of mathematical information has directed me to the Lyapunov theory of stability and to the Poincaré method

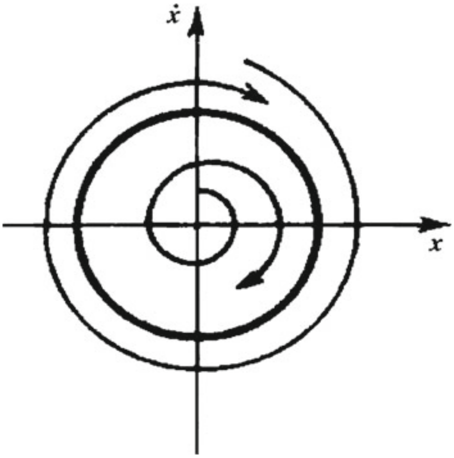

Fig. 9.1 A stable limit cycle on the x, $\dot{x}$ plane

of a small parameter, Mandelstam was a bit surprised… He wanted to have a clear understanding about the genesis of that work, its position in mathematics, its connection (and in some cases a lack of direct connection) with mechanics, astronomy and physics. He easily succeeded here, understood strong and weak points of the new approach and started to be at the head of applications of the new weapon" [18, p. 113].

To assess Andronov's discovery, one needs at least a short journey to the history of this conception. The phenomenon, which was called self-oscillation by Andronov, was described by Lord Rayleigh in the case of the Froude (frictional) pendulum [294, p. 169]. Before Andronov, the phenomenon of self-oscillations was taken in essence into consideration by the German radio-physicist and electrical engineer Heinrich Barkhausen in his dissertation (1907) and succeeding works. Barkhausen had shown that self-oscillations arise in mechanical and electrical systems provided with a feedback mechanism and pumping with free energy. He also underlined the role of non-linearities in the differential equations describing self-oscillations.

It should be noted that the concept of Selbst-Erregten Schwingungen (self-excited oscillations), which was introduced by Barkhausen and was extended over a German translation of Andronov, Vitt, and Khaikin's book [27] differed from Andronov's self-oscillations not only linguistically but also in its "horizon-intentions" (Andronov and his coauthor used the word "Autoschwingungen" in their German publications). While Barkhausen's term emphasized the principle of an engine which produced self-oscillations, Andronov's term placed emphasis on stability of the phenomenon which it designated.

Balthasar Van der Pol, a Dutch radio-physicist and mathematician (1889–1959), contributed a lot to the theory of phenomenon which would be called self-oscillations. However, Van der Pol's approach differed from Andronov's. Van der Pol developed the theory of non-linear oscillations as a number of approximations. "Unfortunately", he wrote in his 1934 review on non-linear oscillations, "our knowledge of the solution of non-linear differential equations has not yet reached that transparent state which is so characteristic of linear equations (and it is doubtful whether this aim will ever be reached). It was, therefore, considered appropriate (and in this way we also follow the historical development) to treat our subject as regards the approximations used from a more physical point of view without primarily laying stress on mathematical rigor" [350, p. 796].

Although Van der Pol was informed about Andronov's work and referred to Mandelstam's and Mandelstam's colleagues' writings, he did not use the concept of self-oscillations in his 1934 review on the non-linear theory of electric oscillations (the term "self-oscillations" appeared in a Russian translation of this review). By applying the method of a slowly variable amplitude, he described a particular case of self-oscillations, the "free oscillations of a regenerative triod oscillator" (called the Van der Pol generator, too), as the final stationary state which a system reached when it lost its unstable initial state. Although Van der Pol expressed a satisfaction that "our physical expectation is completely confirmed by approximations" [ibid., p. 803] he, in contrast to Andronov, did not give an adequate mathematical interpretation of the essential particularity of self-oscillations: He did not express mathematically the fact that the parameters of these oscillations did not depend on variations in the initial conditions and were determined by the nature of a system.

Andronov, Mandelstam, and their colleagues intensively used Van der Pol's approximations in their calculations. However, they tended to adopt those methods within the context of a rigorous non-linear theory based on the qualitative theory of differential equations. In this respect, Mandelstam and Papalexy's article on Van der Pol's calculative method is characteristic: They discussed the geometrical interpretation of the solutions of Van der Pol's approximate "abbreviated" equations in connection with the "phase portrait" of the rigorous equation and underlined the conditions of validity of Van der Pol's approximation.[1]

9.3 Abraham–Bloch's Multivibrator: A.A. Andronov and A.A. Vitt

In 1927–1928, Andronov worked out the concept of self-oscillations by studying very simple systems with one degree of freedom (the tube generator, the Froude pendulum, etc.). The phase portrait of these self-oscillations fitted in a phase plane. Along with this in his early writings, Andronov indicated the wide applicability of the concept of self-oscillations: a string excited by the bow, periodical chemical reactions, and population oscillations. At once, the problem arose of how to justify such a far-reaching claim. Nevertheless, the first problems which had been solved with the help of the concept of self-oscillations were rather simple, too. These were a tube generator with two degrees of freedom (containing two coupled LC circuits), self-oscillations in systems influenced by external periodic force, etc. True, these problems required new methods. However, these were methods within the framework of what T. Kuhn called "normal science". Andronov's 1927–1928 problems became the paradigm of the theory of non-linear oscillations. This meant that at the empirical level those oscillatory motions were taken into consideration, which looked like self-oscillatory ones, their theoretical treatment was conditioned by the search for limit cycles and the study how they behaved.

The paradigmatic function of the concept of self-oscillations becomes clearly evident in the history of how oscillations were studied in Abraham–Bloch's multivibrator consisting of two identical RC circuits connected across. At the empirical level, Andronov described this system as self-oscillatory. But he confronted theoretical obstacles. Having used a natural idealization (having neglected a small "parasitic" inductance), Andronov arrived at a differential equation of the first order, which did

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[1] Mandelstam L.I., Papalexy N.D., On the Justification of a Method of Approximative Solution of Differential Equations, *Zhurnal eksperimental'noi i teoreticheskoi fiziki*, Vol. 4, 1934, pp. 117–125 [1, Vol. 2].

not give a limit cycle. Moreover, this equation implied that such a cycle could not exist. To solve this problem, Andronov had to commit his idealization to an additional hypothesis and to construct a proper analogy of the Poincaré limit cycle. This meant that the concept of self-oscillations had been developed: along with regular, "Thomsonian" self-oscillations evident in a simple tube generator, relaxation self-oscillations, in which the "slow" and "fast" motions alternated, were described [23].

Andronov himself provided the following description of this history.

In 1929, I found myself on the track that the mathematical image of undamped oscillations, self-oscillations is the Poincaré limit cycle. I considered different systems and looked for limit cycles everywhere. However, I took into consideration the usual idealized scheme of Abraham–Bloch's multivibrator, containing only capacities, but showing self-oscillations. I was writing differential equations of its dynamics and looking for a cycle, but without results. Moreover, I could manage to prove that the differential equations into consideration could not have a limit cycle. Instead of a cycle, I found a specific curve which shows that the phase velocity becomes infinite. This meant that it is impossible to fix unambiguously the motion of a representative point. This resulted in a paradox: Self-oscillations mean cycles, there are no cycles, but a system shows self-oscillations. With this paradox, I came to Mandelstam who immediately comprehended what the matter was. After some discussion, he summed up: "If it has been proved that there is no cycle, this is something. Since the system executes oscillations, either your idealized scheme is unsuitable, or you don't know how to work with it". He added that he was leaving Moscow for Leningrad and he would try there to think my scheme over. On his arrival back to Moscow he said "I, together with Nikolay Dmitrievich (Papalexy), think that it is possible to work with your idealized scheme and find a periodical solution which is interesting from a physical point of view. But this solution will not belong to the continuous solutions which you are looking for. It will be a discontinuous solution, that is, a corresponding motion of a representative point will undergo discontinuous changes. We think that one can find a periodic solution on the hypothesis that under these changes the electrical energy stored in capacities is continuously varied".

Soon I and Vitt attempted to realize those Mandelstam's considerations. Having overcome some calculating problems, we found a discontinuous periodic solution [18, pp. 122–123].

So, one can distinguish between two steps of Andronov's struggle with Abraham–Bloch's multivibrator: 1. Andronov attempted to treat an oscillation in the multivibrator "in the image of" self-oscillations evident in the simple tube generator. Having used a natural idealization (having neglected a small inductance) he, however, arrived at the differential equation which did not give a limit cycle.

2. Having discussed this problem with Mandelstam, Andronov held the same idealization but drastically developed the idea of self-oscillations. Along with the Thomsonian self-oscillations which were represented by the Poincaré limit cycles, he admitted the non-Thomsonian relaxation self-oscillations which consisted of the "fast" and "slow" motions. On the hypothesis that the energy stored in a capacity continuously changed, he with Vitt constructed an analogy of the Poincaré limit

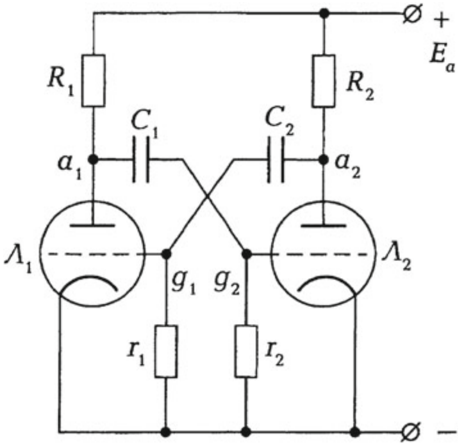

Fig. 9.2 Abraham–Bloch multivibrator. K_1 and K_2 are triodes, g_1 and g_2 are their grids, r_1, r_2, R_1, and R_2 active resistances, C_1, C_2 are capacities

cycle for such oscillations. This analogy consisted of two smooth curves which the equation resulting from the idealization gave and two straight lines *x const* which symbolized the "fast" motions and were justified by the hypothesis (Fig. 9.2).

In order to explain the Andronov–Vitt conception, it is useful to turn to N.D. Papalexy's paper (1931) treating a more simple problem [262] (reprinted in [260]).

In contrast to Andronov and Vitt, who took a two-dimensional problem (their system had two degrees of freedom), Papalexy considered a multivibrator with one RC circuit and represented its operation by using one differential equation of first order. It is clear that the integral curve of its equation is not closed. Papalexy, however, constructed a periodic process consisting of "slow" and "fast" motions (Fig. 9.3). A representative point "slowly" moves along the integral curve of the differential equation of first order. However, this curve has three branches: two of them are stable (*ab* and *dc*) and one of them, located in the middle, is non-stable (*bd*). As soon as a representative point reaches the unstable branch, it makes a jump to another stable branch. This is the "fast" motion. Along the stable branch, it moves "slowly" and in the opposite direction. Again, by reaching the unstable branch, it makes a jump. Papalexy writes that the points onto which a representative point makes jumps are adjusted by the special "hypothesis of jump": The voltage on the plates of the condenser does not change under a jump of the current strength.

At the end of Chap. 8, we noted the variety of cooperation forms in the community called the Mandelstam School. This section provides an example: Papalexy participated in the development of the theory of relaxational self-oscillations initiated by Andronov and Vitt.

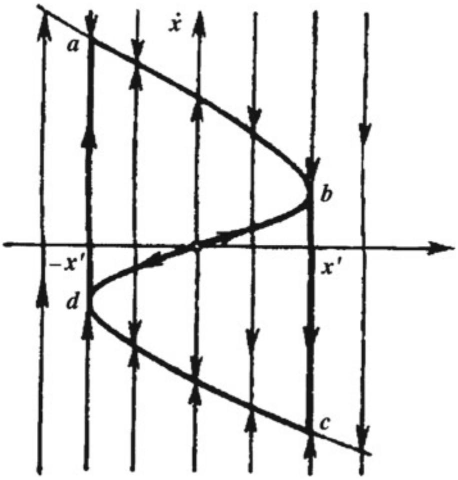

Fig. 9.3 Closed curve *abcd*, representing relaxation self-oscillations (from Papalexy’s paper)

9.4 The “Entrainment” and “Retarded Action”: B. Van der Pol, A.A. Andronov, A.A. Vitt

In 1930, A.A. Andronov and A.A. Vitt published a couple of articles about the phenomenon of entrainment [17, 22, 25, pp. 51–64, 70–84]. On one hand, their research was a development of the L.I. Mandelstam and N.D. Papalexy attack on the different phenomena of resonance. From the other hand, the phenomenon of entrainment of frequency was “a manifestation of the non-linearity of the system and can not be accounted for by linear theory” [238, p. 341].

Let us turn to the equation which was a starting point of Andronov’s reflections:

$$\mathrm{d}^2x/\mathrm{d}t^2 + 2\delta \mathrm{d}x/\mathrm{d}t + \omega^2 x = f(x, \mathrm{d}x/\mathrm{d}t), \tag{9.1}$$

where x is a generalized coordinate (for example, the current strength), t is time, δ is the factor of damping, ω is the natural frequency of the system (the frequency of oscillations which would have taken place, if the friction and outside force would be equal to zero), $f(x, \mathrm{d}x/\mathrm{d}t)$ is a non-linear function describing the action of the energy source which provides the undamped oscillations.

To explain the “entrainment”, we modify the right part of (9.1): The function $f(x, \mathrm{d}x/\mathrm{d}t)$ should be replaced by a periodical function of time. The “entrainment” takes place when the frequency of an extraneous *emf* approaches the frequency of natural (eigen) oscillations of a self-oscillatory system (or it tends to be divisible by it). If the frequency of an extraneous *emf* is far from the natural frequency, the system shows quasiperiodical oscillations which are characterized by two frequencies. One observes the “beats” of two frequencies. If the frequency of the extraneous *emf* approaches the frequency of natural oscillations, the frequency of the beats decreases and after a threshold value the beats disappear and there remains only the frequency of an extraneous *emf*. It looks like as if the "natural" frequency were "entrained" by the extraneous frequency.

The other name of "entrainment" is the forced synchronization.

The "entrainment" has been discovered and rediscovered by a number of physicists starting with Lord Rayleigh. However, Andronov and Vitt proceeded from the analysis of this phenomenon provided by Balthasar Van der Pol. Van der Pol introduced the concept of a locking band. This is the band of frequencies of an extraneous force, the frequencies at which this force "entrains" the natural frequency of the system.

Andronov and Vitt analyzed some of Van der Pol's formulations and (this is the main point) interpreted the "entrainment" from the point of view of Andronov's self-oscillations.

Van der Pol, and Andronov and Vitt after him, described another non-linear phenomenon which has been called the "retarded action". In order to explain this phenomenon, we need to distinguish between the "hard" and "soft" excitation of the valve (tube) generator. The "hard" excitation is reached by a push which brings a system into the state of self-oscillations; this is a jump of the system to the regime of self-oscillations under a variation of its parameter (for example, the coefficient of mutual inductance between the anode and the gird circuits). The "soft" excitation is reached by a smooth evolution of the system under the parameter variation. The amplitude smoothly reaches its stationary value.

N.D. Papalexy writes in his review of the theory of oscillations: "Usually the "hard" excitation is connected with the phenomenon of "retarded action". This phenomenon consists in that under an opposite variation of the parameter the regime of self-oscillations remains after the point of excitation has been crossed (the process under an opposite variation of the parameter does not repeat the process of excitation in the reverse sequence). The self-oscillations has retained over some interval of the parameter variation and after an excitation point has been crossed. Self-oscillations cease under a value which preceded to those at which they have been excited" [262, 265, p. 93].

Andronov and Vitt discussed the oscillations of two coupled circuits, one of them being excited. They write the following [24, p. 176]:

"As is well-known, the oscillations of such a non-linear system have the following property: our non-linear system "selects" one of two normal frequencies in which the corresponding linear system is able to oscillate. Its selection depends not only on a state of the system in the present instant of time, but also on the history of the system.

For some area of tuning—for an area of the variation of a parameter (for example, of the capacity)—it is possible to reach one of two normal frequencies depending on the direction of tuning (depending on the value of the parameter from which the tuning starts). On reaching the frontiers of the area of the retarded action the oscillatory regime is sharply varied: the system "jumps" from one frequency into another.

This phenomenon of retarded action has been discussed by a number of physicists. However, a rigorous mathematical theory has not been elaborated. In the present paper the formulae for amplitudes and conditions of stability have been obtained. These formulae provide the complete representation of the retarded action when the normal frequencies differ enough from each other".

In his works conducted partially together with Vitt and partially together with his graduate student A.G. Liubina, Andronov demonstrated that the "hard" excitation can be described by means of a portrait with two limit cycles (the external one is stable, the internal one is nonstable) and with the stable zero point. He also demonstrated that the "soft" excitation can be described by means of a picture with the nonstable zero point and the stable limit cycle around it.

In these writings, the term "bifurcation" has appeared.

In 2001, the present author asked A.G. Liubina when she came across the term "bifurcation" for the first time. "I was a student, "Liubina said". At the end of the 1920s together with A.A. Andronov, I went for a walk along Leninskie Gory (nearby the place where the new building of MSU has been constructed in the 1950s). He told me about the limit cycles and bifurcations".

9.5 The Method of a Small Parameter: A Slowly Variable Amplitude

In order to understand the Andronov–Vitt articles about the phenomena of "entrainment" and "retarded action", one needs to turn to the approximative methods which they used, the approximative methods of the theory of non-linear differential equtions. These methods were applicable before Andronov's article about the Poincaré limit cycles appeared. They were applied then and afterwards. In Sect. 9.2, the method of "matching" has been described. We have also mentioned the method of a slowly variable amplitude (slow amplitude) which had been invented by Van der Pol and the method of a small parameter (the main contribution here is due to Poincaré). In the present section, we gave some details on the role of these methods (small parameter and slow amplitude) in the conceptual development due to Mandelstam's former students.

These methods take non-linear processes as close to the linear ones. In other words, these methods imply that (9.1) takes the form

$$\mathrm{d}^2x/\mathrm{d}t^2 + \omega^2 x = \mu f(x, \mathrm{d}x/\mathrm{d}t), \tag{9.2}$$

where μ is a small parameter. "The first method gives an opportunity to find asymptotic solutions of (9.2) (the more exact, the less the parameter μ) for periodic processes and for the processes of becoming. The second method allows us to find the periodic solutions of (9.2) in the form of power series. This means with any degree of accuracy, if only these series converge" [26, pp. 652–653].

In the Andronov–Vitt articles, the method of a slowly variable amplitude was supplemented with the discussion of the phase portrait. In such a way, Andronov and Vitt made the problem more visual, explained its physical content. According to the method of slow amplitude, one looks for a solution of (9.2) with the help of the so-called abbreviated (in an English translation of Andronov-Vitt-Chaikin book the word "truncated" is used) equations, the equations for a slowly variable amplitude. Andronov and Vitt gave a geometric interpretation of the "abbreviated equations". In the case of an autonomous oscillatory system, the stable limit cycle on the x and dx/dt plane corresponds to a circle of the stable states on the plane of amplitudes [(9.2) is concerned with autonomous systems, which are not subjected to a periodical external factor].

To explain let us go a little into details. Let us rewrite (9.2) as a system of equations:

$$\begin{aligned} \mathrm{d}x/\mathrm{d}t &= y, \\ \mathrm{d}y/\mathrm{d}t &= -x + \mu f(x, y). \end{aligned} \tag{9.3}$$

Under $\mu = 0$, this system is the equation for an usual harmonic oscillator. As is well known, its solution has the following form:

$$\begin{aligned} x &= a \cos t + b \sin t, \\ y &= -a \sin t + b \cos t. \end{aligned}$$

Van der Pol assumes the solution of (9.3) in the form of the above system, where a and b are slowly varying functions of time. If one substitutes this solution into (9.3), one obtains the system of differential equations with the right sides explicitly depending on time. By neglecting the "oscillating" terms one comes to the approximative equations which are called Van der Pol's "abbreviated" equations:

$$\mathrm{d}a/\mathrm{d}t = \mu F_1(a, b), \quad \mathrm{d}b/\mathrm{d}t = \mu F_2(a, b).$$

Andronov and Vitt interpreted the change of variables x and y for variables a and b as a transformation of the phase plane x, y to the plane of the Van der Pol variables a and b, to the plane which rotates with unit angular velocity relative to the x, y phase plane in a clockwise direction. Equilibrium states are states (points on the plane a, b) where da/dt and db/dt together equal zero. The equilibrium state is stable if its small perturbation does not grow; if it remains small.

Andronov and Vitt demonstrated that under the change of variables x, y by the variables a, b the Poincaré limit cycle can be approximated by a circle of the stable states of equilibrium on the plane a, b.

Mandelstam and Papalexy developed the Andronov–Vitt consideration. They demonstrated that by proceeding from the picture on the plane a, b one can analyze the fragmentation of the plane x, y into phase trajectories. "On the base of our work", the Mandelstam–Papalexy review says, "it is possible to clarify when and to what extent the Van der Pol equations provide a satisfactory approximation for analysis of the unsteady oscillations which are important, for example, for telegraphy" [1, Vol. 3, p. 101].

The Poincaré small parameter method was applied by Andronov and Vitt in their paper about "entrainment". Andronov wrote in this connection [17, pp. 115–116]:

> In the beginning, Mandelstam was rather skeptical with the respect of this method. Mandelstam said about it "This is a kind of the asymptotic method, a kind of Korrespondenzprinzip". Mandelstam bore in mind that we could not evaluate how small should be the small parameter to provide a reliable calculation…
>
> Nevertheless, he considered that it would be important to adopt this method for the solution of non-linear radio-engineering problems and in general to find out what resources of the small parameter method.
>
> Vitt and me succeeded to elaborate the proper procedure which gave a possibility to reproduce a number of well-known radio-engineering results with the help of the small parameter method. These were results concerning the theory of the ordinary lamp generator, the theory of generator with the gird current, the theory of retarded action in the case of strong and loose coupling, the theory of "entrainment" and forced synchronization…
>
> The great heuristic role of the method of small parameter was, however, decisive for Mandelstam's estimation of this method. The small parameter method is able to predict new phenomena. This allowed Mandelstam and Papalexy to apply this method in their research in resonance of the second order.

The resonance of order n appears in the following situation when the system of its eigen frequency is sufficiently close to ω/n, n being an integer, then there can appear intense oscillations of frequency exactly equal to ω/n. In particular, such a situation can be described by differential equations of the type

$$\ddot{x} + x = \mu f(x, \dot{x}) + \lambda \sin nt,$$

where μ is the small parameter. If $\mu = 0$, we come to the equation which has infinite number of solutions. We are looking for a solution to which the solution of our equation tends under $\mu \to 0$. This solution is called the "zero" or "main" solution, and often, it is sufficient from a practical point of view. The "zero" solution consists of two parts. The first part is rather trivial: It represents the oscillations of the period which is equal with the period of the forcing emf. The second part says something about possibility of the resonance of order n. It corresponds to a periodic process the frequency of which is a fractional part $(1/n)$ of the forcing frequency.

9.6 The First All-Union Conference

As was noted in Sect. 9.1, the First All-Union Conference on oscillations showed the social significance of the Mandelstam School. Mandelstam's research, research of his colleagues, and students' science were strongly connected with practice, with industry. Such connection was very important through all the country there was a fight against "pure science" (see: addresses of Indispensable Scientific Secretary of Academy [7, 260]).

This conference directly followed First All-Union Meeting of physicists in Odessa (1930) where the oscillatory orientation was already inaugurated. The conference was held on November 10–14, 1931 in Moscow. 146 people participated. The organizational committee consisted of the following scientists: N.N. Andereev, a specialist in acoustics (later he became Academician), B.M. Hessen (chairman), Academician L.I. Mandelstam, Professor E.L. Nikolai (the Leningrad specialist in theoretical mechanics), D.A. Rozhanskii (who worked for Central Radiolaboratory—see Chap. 5), Professor V.I. Smirnov (a famous mathematician), M.V. Shuleikin (Leningrad radio-engineer, who became later Corresponding Member of the Academy of Sciences).

The main papers were delivered by L.I. Mandelstam, N.D. Papalexy, A.A. Andronov, and M.V. Shuleikin [280].

As was noted, L.I. Mandelstam called the theory of oscillations the "modest area"; it has a general importance, however. He outlined the important transformations which took place in this area. In particular, Mandelstam emphasized the wide applicability of spectral method in the analysis of oscillations. He referred to Schrödinger's application of this method in his wave mechanics.

N.D. Papalexy's paper was a continuation of his paper addressed to the Odessa All-Union Meeting. By applying the political dictionary of that time, Papalexy urged physicists "not to persist in a linear bias". Papalexy told about the radio-engineering problems and about Andronov's work.

In Odessa, Papalexy mainly told about the relaxation self-oscillations, then by addressing to All-Union Conference he told about the resonance of *n*th order.

Andronov delivered a paper about the mathematical problems of the theory of oscillations [16, reprinted in 17]. He proposed an extensive project of the theory of oscillations based on the qualitative theory of differential equations. In his paper, he told about his articles which had already been published and about those which he was planning to publish (in particular about [24]). He not only told about the Poincaré limit cycles, but also about different types of stability, about the behavior of singular points under parameter variations, about bifurcations.

Andronov's paper was replete with mathematical romanticism. "Andronov told about the general classification of motions of dynamic system, about the Birkhoff central motions which embrace the recurrent and Poisson stable motions—today we call them chaotic and stochastic—and about the moderate position of his theory of self-oscillations" [260, p. 15]. Andronov showed that abstract mathematics may expose important and interesting physical phenomena.

Andronov also emphasized that the theory of oscillations needed new mathematical means. "The Poincaré-Birkhoff methods are not effective for $n \succ 2$. They provide an idea about the type of motion, but don't have any tool to analyse a particular differential equation to which we come. It is necessary therefore to fill somehow a break between topology and physical practice" [17, p. 101].

The Andronov disciple Yu. Neymark wrote about First All-Union conference on oscillations: “The fact of the organization of such a conference spoke in favour of the theory of oscillations as a science, and Andronov’s broad and detailed lecture corresponded such a position of the this theory” [253, p. 132].

In their 1936 review on the theory of non-linear oscillations [1, Vol. 3, p. 89], Mandelstam and his coauthors listed six big scientific establishments [located in Moscow, Leningrad (now—Saint Petersburg), and Gorkii (now Nizhni Novgorod)] whose results were presented in the review and where research was being conducted within the framework of a “unified scientific policy”.

This review was written by the “oscillatory” branch of the Mandelstam School. This was Mandelstam himself, Papalexy, all Mandelstam’s graduate students of the first generation (except Leontovich). Gorelik and some of his collaborators participated, too.

Initially, this review was planned as a response to Van der Pol’s request (Van der Pol was Chairman of International Radio-Engineering Union then); however, it became an independent work.

“The present brochure contains a short review of works on non-linear oscillations (and some connected with the research concerning linear systems) that were mainly conducted at Physics Institute of Moscow State University, Leningrad Central Radio Laboratory, Leningrad electro-physical institute, and Gorkii physico-technological institute. Some of works were conducted at Physics Institute of the Academy of Sciences (FIAN) and scientific research center which belongs to Industrial Institute in Leningrad.

Researches in non-linear oscillations are conjointly conducted at all these scientific establishments, they are strongly connected with each other and comprise a research area” [1].

Andronov-Vitt-Chaikin’s book “The theory of oscillations” became the main result of the Mandelstam school in this area. This book has been three times published in the USSR (1937, 1959, 1981) [19, 22]. In 1944 a detailed review of this book performed by Nikolai Minorsky was published in USA (a rotaprint publication supported by the US Ministry of Navy). After the World War II, English translations were published [20, 24]. The first had been performed by the famous American mathematician Solomon Lefschetz. Lefschetz also wrote a review where he represented Soviet works at length [202].

In the 1960s, a German translation was published [27].

Minorsky, who published several books on non-linear dynamics after World War II (see, for example, [236–238]), wrote that “almost completely the early period of codification of the new science took place in the USSR” [260, p. 112]. True, Minorsky meant not only the Mandelstam School’s work, but also Krylov-Bogoliubov’s writings (see Chap. 10, Sect. 10.2).

According to S.M. Rytov (interviewed by G.E. Gorelik), the book “The theory of oscillations” was completed in 1935. However, its publication met difficulties (it is not known what difficulties). Difficulties were overcome only by 1937. However, in 1937 A.A. Vitt was arrested (the order to eliminate Vitt from the staff of the Moscow State University was issued on August 1, 1937) and to publish the book it was necessary to eliminate Vitt as an author (this piece of the history is touched upon in [37, 41, 168]). Therefore, the first edition of “The theory of oscillations” had two authors—Andronov, Chaikin.

So, by interacting with his graduate students L.I. Mandelstam conducted the following works: (1) he formulated the problem and made it more precise in the course of research, (2) he permanently discussed the results of research, (3) he was anxious for practical application of his graduate students’ results, and he learned from his former students how to formulate new problems, and (4) he took care of the social status of their research.

Chapter 10
A Continuation—The Mandelstam-Andronov School

10.1 Terminology

The term “Mandelstam-Andronov school” is not so widespread as the term “Mandelstam school” which has been in the center of our discussion till now. However, the term “Mandelstam-Andronov school” is present in the literature. The former Andronov’s graduate student Y.I. Neymark, who considers himself to be a representative of the Mandelstam-Andronov school, used this term by emphasizing the conceptual contribution of A.A. Andronov to the theory of oscillations and his organizational role in the formation of physics in Nizhny Novgorod. Neymark also emphasized Andronov’s high moral standards, which corresponded to the moral standards of Mandelstam and his disciples but appearing in a new situation—in the situation of provincial science [247–249].

Along with the term “Mandelstam-Andronov school”, the term “Andronov school” is present in the historical literature. The Andronov school is treated as an extension of L.I. Mandelstam’s school [54, 56, 289].

About the Andronov school, Chriss Bissell writes by emphasizing its contribution to the development of control engineering [42–46].

The present author asked M.I. Rabinovich, whether he feels himself as a member of the Mandelstam-Andronov school. M.I. Rabinovich was a graduate student of A.V. Gaponov–Grekhov, who in turn was a graduate student of A.A. Andronov. M.I. Rabinovich contributed to the theory of dynamic systems and of dynamic chaos [128, 130, 291].

M.I. Rabinovich answered that he preferred to consider himself as a representative of the Mandelstam school. The concept “Mandelstam-Andronov school” is too narrow. It is too strongly emphasized one of the directions in contemporary physics and mechanics.

The present author conversed about this statement with Y.I. Neymark and asked his opinion. Neymark answered by putting a rhetoric question: “Say to me, please, what theoretically importance the Mandelstam school could represent, if Andronov’s

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conceptions are neglected? The method of matching which Papalexy applied? The program of oscillatory mutual assistance?"

Thus, Neymark emphasized that the concept of the Mandelstam-Andronov school carries important philosophical connotations. This concept accentuates not only human relations and methodology of research, but also the powerful area of theoretical conceptions.

As was mentioned in Chap. 8 Sect. 8.1, "scientific school" is a rather blurred conception. But it allows us to notice such details of historical reality that would be unnoticed if this concept is absent. The term "Mandelstam-Andronov school" "allows us to trace the stream of ideas decisive for theoretical evolution of the theory of oscillations and non-linear dynamics".

10.2 The Competition

One of the reasons to treat the Mandelstam-Andronov school as a historical phenomenon is the presence of another (in a sense, rival) school in non-linear science in the USSR, namely the Krylov-Bogoliubov school of non-linear mechanics.

In contrast to the Mandelstam-Andronov school, the Krylov-Bogoliubov had a "chamber" character. It consisted of two persons: the Kiev Professor N.M. Krylov (1879–1955) and his disciple N.N. Bogoliubov (1909–1992), who afterward became the great figure in statistical physics and in quantum field theory.

Mandelstam, Papalexy, Andronov, and their collaborators used the term "the theory of non-linear oscillations" to designate their field of research. Krylov and Bogoliubov preferred the term "non-linear mechanics" which was close to the concept of non-linear dynamics popular in the West [182–184]. As Y.I. Neymark said to the present author, A.A. Andronov mocked in this connection: "All mechanics is non-linear".

To some extent, Krylov's-Bogoliubov's non-linear mechanics can be traced back to Van der Pol's approximate calculations. "It should be noted", Krylov and Bogoliubov wrote, "that Van der Pol's non-rigorous methods which he applied ad hoc yield some clues to how to analyze semi-periodic oscillations which cannot be studied by Poincaré-Lyapunov methods (at least in their contemporary state)" [183, p. 477].

N.N. Moiseev, however, wrote in favor of Krylov and Bogoliubov: "In the beginning of the 20th century the Dutch engineer Van der Pol discovered a new method of how to study oscillations… Krylov and Bogoliubov, however, were able to treat this matter from the principally new point of view. They elaborated an asymptotic method which resulted in the Van der Pol's solution as a particular case" [240, p. 11].

What about the competition between the Mandelstam-Andronov school, from one side, and the creators and proponents of non-linear mechanics, from the other side? Although these two schools "tried not to pay attention to each other" (as M.I. Rabinovich said to the present author), at their early stage, Krylov and Bogoliubov stressed their disagreement with the Mandelstam school. "The school of Academician Mandelstam and Professor Papalexy", Krylov and Bogoliubov wrote, "has first called the attention of radio-engineers to Poincaré'-Lyapunov's methods in study of periodic oscillations. In spite of their importance, Poincaré-Lyapunov's celebrated methods are applicable (at least in the contemporary form) for studying periodic oscillations, whereas in radio-engineering (like in many other sciences) we have to deal with quasi-periodic oscillations, that is, with oscillations which have (at least) two frequencies independent from each other" [183, p. 478].

Although the mathematical technique (asymptotic methods) and the terminology of Krylov and Bogoliubov drastically differed from those of the Mandelstam school, they used the concept of self-oscillations. But their concept of self-oscillation, which was technically equivalent to Andronov's, was formulated in a different context and had different horizon-intentions. Krylov and Bogoliubov discussed the same simple tube generator as Andronov discussed. However, they deliberately treated this system by applying methods which are effective for more complicated systems: They solved the corresponding approximate linear equation and regarded non-linearity, which the rigorous equation contained, as a small perturbation. Like Andronov, they defined self-oscillations as undamped oscillations in a dissipative non-linear system, which were maintained by an external non-periodic energy source. But they interpreted this phenomenon by referring to the old quantum theory (the Bohr-Sommerfeld theory): According to them, non-linearity in the rigorous equation worked like a "quantum condition". It "selected" the self-oscillatory solutions among all the solutions of the corresponding linear differential equation.

Andronov's concept of self-oscillation was oriented to a topological analysis of the problem. Andronov was among those who launched a qualitative theory of differential equations in physics and in general in natural science. His self-oscillations, represented by the Poincaré limit cycles, apparently provided the first example of an attractor in physics. Andronov also started what he called the "historical" and "embryological" investigation of dynamical systems. Such an investigation consisted in tracing the evolution of the phase space structure (e.g., the loss of stability by a limit cycle, the rise of a new limit cycle) when changing parameters of the system. Krylov and Bogoliubov invited the concept of self-oscillations to visualize their sophisticated theory by a simple example. They considered the general problem of the stationary solutions of non-linear equations. As we said above, Krylov and Bogoliubov interpreted the phenomenon of self-oscillations by referring to the old quantum theory: According to them, non-linearity in the rigorous equation worked like a "quantum condition" with respect to the solutions of the corresponding approximative linear equation. This was in their 1933 article. In their subsequent publications, they did not use the analogy with the old quantum theory [184]. However, they held to the same method in them. Their theory of first approximation yielded the auxiliary algebraic equation which gave the criterion of the stationary solutions, and "selected" such solutions among the solutions of the corresponding linear equation. Krylov and Bogoliubov emphasized that this theory gave a qualitatively accurate account: It correctly showed stationary regimes and their stability but the parameters of the regimes (e.g., the amplitude of self-oscillations) were only approximated.

To explain let us consider how Krylov and Bogoliubov solved the Van der Pol equation which described self-oscillations:

$$\frac{\mathrm{d}^2x}{\mathrm{d}t^2} - \varepsilon(1 - x^2)\frac{\mathrm{d}x}{\mathrm{d}t} + x = 0$$

where ε is a small parameter.

As a matter of fact, they reproduced by applying their technique that solution which Van der Pol reached at the beginning of the 1920s.

As a first approximation, they had

$$x = \sin a\psi,$$

where

$$\frac{\mathrm{d}a}{\mathrm{d}t} = \frac{\varepsilon a}{2}\left(1 - \frac{a^2}{4}\right) \tag{10.1}$$

and

$$\frac{\mathrm{d}\psi}{\mathrm{d}t} = 1$$

Thus, as a first approximation they had a harmonic oscillation $x = \sin(t + \theta)$ with the constant frequency $\omega = 1$ and the amplitude which varied according to the differential equation (10.1).

By solving this equation, Krylov and Bogoliubov had

$$x = \frac{a_0 \mathrm{e}^{1/2\varepsilon t}}{\sqrt{1 + \frac{1}{4}a_0^2(\mathrm{e}^{\varepsilon t} - 1)}} \sin(t + \theta)$$

They pointed out that the algebraic equation $\frac{\varepsilon a}{2}\left(1 - \frac{a^2}{4}\right) = 0$, which resulted from (10.1), can be regarded as a condition of selection.

Accordingly, there were two stationary solutions: at $a = 0$ (trivial and non-steady) and $a = 2$ which is steady, since $a(t) \to 2$, if $t \to \infty$.

Thus, if for Mandelstam and Andronov the concept of self-oscillations was a point of departure of their theory, then for Krylov and Bogoliubov it provided just an example to demonstrate how their theory worked. Krylov and Bogoliubov reproduced the results of the Mandelstam-Andronov school to show the validity of their own theory which was oriented toward more complicated problems.

As was noted (Chap. 9), in 1931 the All-Union (National) Conference on oscillations, where Mandelstam's community of physicists played a main part, was held. The founders of non-linear mechanics (N.M. Krylov and N.N. Bogoliubov) did not participate in this conference.

Krylov and Bogoliubov published a generalized paper in 1933 [183] and a book in 1937 [184]. A free English translation of this book was undertaken by Solomon Lefschetz in 1949. Bogoliubov's and Yurii Metropolskii's 1958 book *Asymptotic Methods in the Theory of Nonlinear Oscillations* (Metropolskii is a former Bogoliubov's student who joined this work in the immediate postwar years) became a well-known textbook in non-linear dynamics.

10.3 In Search for Self-oscillations

As was noted, in Andronov's work self-oscillations carried a paradigmatic function. However, the concept of self-oscillations determined more than the paradigm of the theory of non-linear oscillations. As was mentioned, it had an articulated ideological aspect, which cannot be expressed in the term "paradigm". While our concern is "paradigm" we mean "shared examples", the typical problems which have been solved within the framework of the theory and encompass its approach to explanations of empirical facts. While the theory of non-linear oscillations is still going to be applied, empirical facts are described with the help of the concept of self-oscillations as similar or non-similar to the oscillations in a tube generator and of the Froude pendulum. When we are concerned with theoretical explanation of those facts, we look for the limit cycles or attempt to construct their developments.[1]

As its ideological implications, the concept of self-oscillation (the "self-oscillatory ideology") bores an intuition of the attractor. This concept appeared in the context of the following predicates: "to be determined by the properties of the system", "to be determined by the system itself", "to be independent from initial conditions", "to be autonomous", "to be self-maintained", etc. These predicates gained popularity and began to live on their own, so to speak. They came to be used beyond the mathematical technique of the theory, to be incorporated into other theoretical contexts in order to define new scientific concepts, and even to be copied as analogies.

First of all the concept of self-oscillation itself was used not only as a rigorous mathematical concept but also as a physical idea. For Andronov, it was of most importance to contrast self-oscillations with forced oscillations. In Andronov, Vitt, and Chaikin's book, self-oscillation was defined as an oscillation maintained by a non-periodic source of energy. The parameters of these oscillations were determined by the system itself and did not depend on initial conditions; this was considered to be an essential property of them. However, in the subsequent discussion the predicate "to be maintained by non-periodic source of energy" came to be neglected and the

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[1] According to Th. Kuhn, the paradigm (the disciplinary matrix) is characterized by (1) "symbolic generalizations"—differential equations which are basic in the area under discussion, (2) "ontological scheme" (the model of physical reality), (3) values, (4) "shared examples". It was symptomatic that the tendency appeared to treat the concept of self-oscillations ideologically that is to say purely qualitatively as a set of rough pictorial schemes. For example, in A.A. Charkevich's book [77] only the first 30 pages are dedicated to a rather mathematical discussion of self-oscillations. The rest is dedicated to a qualitative description of self-oscillations in many engines and phenomena.

predicate "to be determined by the system itself and to be independent from initial conditions" took the main part. Incidentally, even this predicate was changed. Instead of independence, a weak dependence or dependence within the terminal variation of initial conditions came to be included into the definition.

Other deviations from Andronov's concept of self-oscillations followed. As a result, this concept became vague and sometimes looked like a metaphor rather than a scientific concept.

This can be observed by reading the following quotations from the interviews given to the present author by Neymark and Rabinovich.[2]

Neymark said the following about his scientific concern in the immediate postwar period:

> I was like a bloodhound on the scent of self-oscillations. Where did I not see self-oscillations? My graduate student Yuriy Isaakovich Gorodezkiy was concerned with self-oscillations in metalworking… I dealt with self-oscillations in burning, now I am concerned with, you have seen the journal *Priroda*, self-oscillations in society.

Y.I. Neymark referred to his article [247].

He added about the conception of self-oscillations:

> Sure, self-oscillations was a great discovery. But a long time has passed and one began to feel that this conception is inexact and vague. Moreover, it is floating and even losing its sense. At the end of the 1920s people knew a little. They knew that there were forced oscillations, and there was an autonomous system in which oscillations took place in their own way. The former was called forced oscillations, the latter was called self-oscillations. However, it turned out that there were many transitional phenomena. I can regard forced oscillations as self-oscillations. Let us consider the self-oscillatory system placed inside of another system. This self-oscillatory system acts upon another. I can regard it as an outer force. And everything is being confused. And here the realization is coming that this great term is fallible. And what of it?

In reply to the present author's question if the conception of self-oscillation had ideological implications, M.I. Rabinovich said the following:

> Yes, it had. In fact, the expression "self-oscillations", like the one "self-waves", which was introduced by R.V. Hohlov, is appropriate, significant. Through my life I dealt actually with the non-linear dissipative non-equilibrium systems. These could be media, as a rule, I am concerned with wave problems or turbulence, but always there is a dissipation in these systems. As for me, Hamiltonian systems are a limit case. I am always interested in systems with attractors, the systems, in which something is becoming at $t \to \infty$—chaos, periodic oscillations, stochastic structures… For me, structures and dynamical chaos are merely some types of an attractor at which a system or medium arrives at time tends to infinity, in the course of evolution of behaviour of a system or medium. I am always interested in systems, in which something is becoming, in which there exists something objective, independent of the initial values' variations.

Rabinovich's article *The stochastic self-oscillations and turbulence* [291] had a high impact. In this article, he regarded as the stochastic self-oscillations the phenomenon which was named "dynamic chaos". Rabinovich comments on it as follows:

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[2] Neymark's contribution is represented in the books [245, 246, 248, 253]. Neymark wrote about A.A. Andronov and his school in [248, 250, 252].

> I emphasized in this article the property of self-oscillations, their independence from initial values' variations (this is valid for both a stationary turbulent flow and stationary chaotic motion of a pendulum with a trembling suspension). This property is attractive. A stable structure, stable chaos! True, this forces one to study the transition, to study how the stability is becoming…
>
> Now I don't use the term "stochastic self-oscillations".[3] What is use of forcing an open door, since the concepts of a dissipative structure and dynamic chaos arose? And if I take the term "stochastic self-oscillations", I confront many problems, presumably the problems of its definition…

To sum up let us stress that both Neymark and Rabinovich emphasized the great importance of self-oscillations for the development of their field. However, Neymark tended to maintain the concept of self-oscillations in operation as a very general concept, while Rabinovich tended to restrict its area of application.

Let us turn again to our discussion of the situations in which the "self-oscillatory ideology" is evident. The third situation is the formation of the concepts analogous to the concept of self-oscillations (analogue concepts). In this case, the popular expressions, in which the concept of self-oscillation was formulated, were not only taken into new contexts where they were combined with "alien" expressions, but also copied to form analogies. In other words, on retention of their linguistic structure carrying in a vague form a physical idea (to be determined by the properties of the system and to be weakly dependent on attendant factors—the idea of attractor), some of their terms were replaced by new ones.

This situation will be considered in the following section, where the concepts of self-resonance, self-waves, and self-structures will be described.

10.4 From Concentrated Systems to Distributed Ones

Due to its ideological implications, the concept of self-oscillation, which was developed for lumped systems (a mechanical pendulum, an CL circuit, etc.), was very quickly extended to cover the oscillatory behavior of distributed ones (continuous media).[4] A.A. Vitt seemed to be the first who extended the concept of self-oscillations to continuous media [356–358]. In his 1934 article, this problem was explicitly formulated. In his 1936–1937 articles, he described the oscillations of a violin string when the bow is moved across it [357].

To stress the importance of self-oscillatory phenomena in continuous media, Vitt in his 1934 article referred to the telegraph wires sounding under the action of wind, the aircraft's wings vibrating because of their friction on air, the valve generator schemes containing an antenna. However, he discussed only a very simple example:

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[3] As one can read in his and Trubetskov's 1984 book, "stochastic self-oscillations are random contingent motions which are powered in a dissipative system by a non-stochastic energy source" [345, p. 368].

[4] The CL circuit is called a lumped system since capacity and inductance are localized in some points of it as mass in mechanical pendulum.

Lecher wires containing the lumped capacity and non-linear resistance with falling characteristic (negative resistance).[5] He admitted that the positive resistance was evenly distributed along the wires. He also admitted that the system was near linear and the stability theorems which had been found for lumped systems were valid for his distributed system.

In his articles about the oscillations of a violin string, Vitt developed a more rigorous approach and discussed several regimes. He did not use the term "self-oscillations" but said that in this article he explored the topic of his previous article further. He also used the idea of self-resonance, which could be considered to be an analogy to the concept of self-oscillations. Here he tacitly followed Mandelstam and Papalexy's 1934 article on parametric resonance. In that article, Mandelstam and Papalexy distinguished between heteroparametric resonance and self-parametric resonance. By parametric resonance, they meant the excitation of electric oscillations in a system through the periodical variation of its parameters. Heteroparametric resonance arose if the parameters were varied by an outside force. The self-parametric resonance was invented as a kind of self-regulation: The variation of the parameters (in response to the outside force) was determined by the properties of the system itself.

However, up to the 1960s, the general interest was focused on the lumped systems which were important for radio-engineering and control engineering. As for distributed systems, some simple limiting cases were basically taken under investigation. For example, the violin string which Vitt studied was a bounded continuous system with a lumped non-linearity.

The turn to continuous systems, which occurred after World War II, was mainly connected with the development of quantum electronics, physics of plasma, and chemical kinetics. This turn brought the theory of non-linear oscillations to face fundamentally new issues and matters. Nevertheless, the self-oscillatory ideology drove one to consider oscillatory behavior of such systems with the help of the language framework in which the concept of self-oscillations had been formulated.

Let us consider how the idea of maser (the prototype of laser) was formulated in the USSR. The idea of maser arose at the laboratory of oscillations at the Lebedev Physics Institute of the Academy of Sciences (the laboratory which was headed by Papalexy till his death) at the beginning of the 1950s. This idea came in the course of work on an improvement of the radio-spectrometer. This was an idea to explore the induced radiation for generation and amplification of electromagnetic waves in the microwave range.

In their 1955 article, N.G. Basov and A.M. Prokhorov, to both of whom was awarded the Nobel Prize in 1964, described the principle of the maser as follows [34, pp. 491–492]:

> If a beam of active molecules is passed through a resonator which is tuned to the frequency of a spectral line, then the molecules will lighten (give off as a light, *vysvechivat'*), radiate energy. This energy will be partially stored in a cavity and partially be lost on the walls.

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[5] The Lecher system is two parallel wires along which electromagnetic waves propagate.

> If these energy losses are less than the energy which was lightened by the molecules, the stored energy will be built up, that is, the cavity will become self-excited. As a result a self-oscillatory system with a feedback arises. This system consists of molecules that are lightening and radiating energy in response to the energy which they have lightened before.

In his Nobel Lecture A.M. Prokhorov explained the operation of a maser by referring to a simple tube generator as an analogy. One can read Mandelstam's typical way of speaking in his lecture: "As is well known from radio-engineering".

In connection with maser and laser, the meaning of the concept of "self-oscillation" has changed. Self-oscillations came to be treated as undamped oscillations which were maintained by not only the non-periodic sources of energy but a powerful oscillatory process which had been excited from outside. The reason to treat these oscillations as self-oscillations (rather than forced oscillations) was that they were determined by the parameters of the system (e.g., they can be made to vanish by varying the damping and detuning).

It is remarkable that in the recent developments of the theory of lasers, the role of the concept of self-oscillations became modest. This concept has been transformed into the historically important one.

Under the banner of self-oscillations, Mandelstam's school launched its research in chemical kinetics. The first paper on the periodic chemical reactions was published by A.A. Vitt (together with F.M. Shemiakin[6]) in 1935. The authors proceeded from A.J. Lotka's works in which Andronov as early as 1928 saw the description of chemical self-oscillations. However, Lotka did not construct the theory of chemical self-oscillations. His 1920 mathematical model only approximated self-oscillations because it was a conservative model (that model was reproduced by V. Volterra in 1931 and now it is usually called the Lotka-Volterra model). Vitt and Shemiakin developed Lotka's kinetic findings to propose the models of chemical self-oscillations. But they did not consider purely homogeneous chemical systems, that is, they receded from what was mostly important and revolutionary in Lotka's work. It was homogeneous periodical reactions that eventually challenged the classical thermodynamic paradigm which legitimated only monotonous variations in parameters of chemical reactions.

Extensive research in kinetic of periodic homogeneous reactions started at the beginning of the 1960s. This was A.M. Zhabotinskii's research which continued B.P. Belousov's pioneering research started at the beginning of the 1950s, that is, this was a fundamental investigation into the famous Belousov-Zhabotinskii reaction. As Zhabotinskii himself wrote, this research was initiated by his university teacher professor of biochemistry at Moscow State University Simon E. Shnol who called his attention to Belousov's 1958 paper [393, p. 20]. However, in his main book *The concentration self-oscillations* (Moscow, Nauka, 1974), he regarded himself as belonging to Mandelstam's school (he wrote that his main Teacher was his father Mark Efimovich Zhabotinskii who started as a Researcher under S.P. Strelkov who

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[6] Fedor Mikhailovich Shemiakin was a specialist in analytical chemistry (see: Pechenkin A. The history of research on chemical periodic processes. Springer. 2018).

in turn was Mandelstam's disciple), and that was the case since he and his coauthors used the Mandelstam-Andronov "self-oscillatory ideology" in their work.[7]

Zhabotinskii's and his coauthors' work consisted of two parts. They first investigated chemical kinetics of Belousov's reaction and proposed the mechanism of the reaction (afterward this mechanism was improved by the American chemists R.M. Noyes, Richard J. Field and their coauthors). They also launched their research in mathematical modeling of the process. Zhabotinskii regarded as their early achievement that they transform the Lotka-Volterra conservative model into a self-oscillatory model for the Belousov-Zhabotinskii reaction. He also pointed out an achievement to Korzuchin's theorem: This theorem stated that it was possible to obtain the regime of self-oscillations in a homogeneous chemical system (M.D. Korzuchin was Zhabotinskii's coworker).

The turn to continuous systems manifested by the above research in quantum electronics and chemical kinetics led to the revision and generalization of the concept of self-oscillations. This work was undertaken by several authors. Here we cite the one proposed by M.I. Rabinovich and D.I. Trubetskov in their 1984 book [292, p. 341]:

> By a self-oscillatory system is called a dissipative system, in which as a result of the development of instability it is possible that non-damped wave oscillatory motions are established, with the parameters being determined by the system itself and do not depend from initial conditions' variations.

The concept of self-organization immediately followed the concept of self-oscillations in this book. Indeed, this concept was formulated as an analogy of the former.

> By self-organization we call the formation in a dissipative non-equilibrium system space structures (generally evolving in time), parameters of which are determined of the properties of the system itself and weakly dependent on the space structure of the source of non-equilibrium, the initial state of the system and often the boundary conditions of the process.

However, the real history of ideas was more complicated. Some new analogue concepts ("self-waves", "self-structures") were proposed. These concepts showed that the theory of non-linear oscillations became a new field of phenomena which the concept of self-oscillations covered only partially, the field where the phenomenon of self-oscillations was only particular.

Among the analogue concepts, the concept of self-waves was most important. This concept was put forward by Rem Victorovich Hokhlov in his report on Zhabotinskii's Dr. Sci. thesis (1972). Self-waves followed self-oscillations in continuous media, they also were of much importance in their own right. By self-waves are usually meant self-maintained waves whose characteristics are kept constant by an energy source distributed over the medium. These characteristics (the period, the length of wave, the velocity of propagation, the shape, etc.) depend only on the local properties of

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[7] Sergei Pavlovich Strelkov (1905–1974) worked for Central Aerohydrodynamic Institute (TsAGI) and was Professor at MSU. He is the author of a popular textbook [329].

the medium and do not depend on the initial conditions and even (far enough from the boundaries) on the boundary conditions.

As early as the Belousov-Zhabotinskii reaction, one found several self-wave regimes. Self-waves have been studied in plasma, polymeric film, ionosphere, and heart muscle. Over the first decade of the twenty first century, self-waves became very important phenomena for biophysics and were accepted by synergetics studies which grew within the theory of non-linear oscillations.[8] The above concept of self-organization can be treated as a generalization of not only the concept of self-oscillation, but also the concept of self-waves. Let us recall, however, that "self-waves" are an analogue concept of "self-oscillations". It keeps self-oscillations within its horizon-intentions.

The concept of self-structures was put forward by Andrei Viktorovich Gaponov-Grekhov at the end of the 1970s to embrace such phenomena as Bénard cells at thermoconvection, Taylor vortices, etc., the phenomena which became the paradigmatic examples of synergetics processes (as M.I. Rabinovich said to the present author). This concept also was an analogy of self-oscillations and directly led to the concept of self-organization: by self-structures he meant space structures (generally evolving in time), parameters of which are determined by the properties of the system itself and weakly dependent on the parameters of the source of non-equilibrium, the initial state of the system, and often the boundary conditions of the process.

10.5 The "Self-oscillatory Ideology" Versus the "Self-oscillatory Paradigm"

In Sect. 10.3 we said that, along with the "self-oscillatory" paradigm, the "self-oscillatory ideology", the linguistic resources of the concept of self-oscillations, played an essential part in the development of the theory of non-linear oscillations. In Sect. 10.4, we considered how this "ideology" worked. Let us turn again to the relationship between the "self-oscillatory" paradigm and the "self-oscillatory ideology". The thing is that these two dominants of the theory of non-linear oscillations came in conflict, in the long run. The "self-oscillatory paradigm" (the simple examples of self-oscillations) occurred to be obstacles to the development encompassed by the "self-oscillatory ideology". With the benefit of the linguistic resources of the "self-oscillations", the theory of non-linear oscillations was extrapolated to cover a very wide scope of phenomena. However, the examples of self-oscillations, which were in the core of the theory, could not help much in understanding of the new phenomena. As was mentioned above, the "self-oscillatory ideology" bores in a vague form a physical idea, the idea of the attractor. This idea turned out very productive in

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[8] In Russian scientific and especially in scientific popular and philosophical literature synergetics, which was introduced by Herman Haken as a theory of the parameters of order, was often taken as a very important research field which anticipates the future development of science. Synergetics is treated as the general theory of self-organization.

the study of oscillatory processes in continuous media. On the other hand, in pursuing this subject the “self-oscillatory paradigm” sometimes was out of work or, moreover, led to overlooking the main point.

As early as 1931 Andronov gave a warning that the methods which he used for simple systems (Poincaré-Lyapunov’s methods) became non-effective for higher-order systems (see Chap. 9). He put forth the program to develop an adequate mathematical technique for such systems, the program which was summed up by his disciples as a program “to come out of the plane into the space”. However, as far as physics was concerned, he was keeping the “self-oscillatory paradigm”, taking as illuminative examples those problems which he had solved at the end of the 1920s (sometimes he also took as such examples some of his and Vitt’s problems which were solved at the beginning of the 1930s).

In the war years, Andronov with his colleagues did much work on control engineering problems which were basically associated with the higher-order systems. By accommodating the Poincaré point mapping they succeeded to develop an effective method of stability analysis for such systems. In essence, they solved the problem which Mandelstam originally put before Andronov (see Sect. 10.3), the problem to analyze mathematically the stability of motions (trajectories of differential equations describing non-linear oscillatory systems) the problem yielded by the method of “matching” (“pripasovyvania”). As part of this work, the method of “matching” was elaborated as a regular method of the theory of non-linear oscillations, as the method of piecewise linearization.

This did not mean the abandonment of the “self-oscillatory paradigm”. To the contrary, Mandelstam’s-Andronov’s community was keeping the search for self-oscillations and study of their behavior as central issues. Only the mathematical treatment changed: The search for a stable limit cycle was replaced by the search for a stable fixed point of the corresponding point transformation. In turn, Andronov’s early writings on self-oscillations contained an illuminative example of the “stitching together” (shyvaniia) of phase portraits, which was at the core of the method of piecewise linearization.

In the postwar years, the Mandelstam-Andronov community highly appreciated the theory of self-oscillations and thus firmly held the “self-oscillatory paradigm”. In the 1940s, 1950s, and 1960s, a large amount of reviews and generalized articles, in which this theory was regarded as a considerable achievement, has been published. Thus, in Rytov’s 1947 review, the theory of self-oscillations, in which the tendency to develop a rigorous non-linear theory culminated, was called an “achievement of Soviet scientific thought”, the achievement which can be traced back to a “pioneering tradition in Russian science” [300]. The following phrase is taken from the 1969 review: “The Soviet scientists have created a new area of the science on oscillations, the area of self-oscillations, which new study and results enrich now” [302, p. 10].

A.V. Gaponov-Grekhov and M.I. Rabinovich’s 1979 article seemed to be the first in which a skeptical tenor sounded with respect to the Mandelstam-Andronov approach. Having reviewed Andronov’s concept of self-oscillations and its developments for complex systems and wave phenomena, the authors pointed out the following: “However, it is necessary to emphasize that we could not gain anything like the rigorous qualitative theory which was founded by Poincaré and extended by Mandelstam and his disciples for the oscillatory systems with a small amount of degrees of freedom” [128, p. 165].[9] Gaponov-Grekhov and M.I. Rabinovich also wrote that we can hope to construct a qualitative theory of the wave phenomena but this theory would be “qualitative” in the sense of visualization and simplicity of schemes in use rather than in the sense of Andronov’s topological theory.

In the style of Pascal who said “I cannot forgive Descartes”, Rabinovich said to the present author that “Andronov thought that the oddest things happen in three dimensional space but the limit cycle is only typical. With this he pulled the wool over many eyes”.

Nevertheless, as late as 1984 Gaponov-Grekhov and Rabinovich continued to propagandize the concept of self-oscillations. One could read in their 1984 article that “self-organization results from the development of spatially heterogeneous instabilities and their subsequent stabilization through balancing dissipative losses and an energy supply from the source of non-equilibrium. This is very similar to the excitation of self-oscillations in a tube generator! The only difference is that “self-organization” in a tube generator took place in time alone, here (in continuous media) it takes place in time and space. It is not surprising that the examples of self-oscillations in continuous media (say, the emergence of an ordered pulse in laser) served as the examples of self-organization” [129, p. 253].

Already in 1992, the same authors have written that “regular self-oscillations are one of the simplest phenomena observed in non-equilibrium non-linear media. The onset of structures having complex spatial organization and appearance of chaos and turbulence are typical for such media” [130, p. 39]. Self-oscillations have lost their value of a “shared example”.

Some members of Mandelstam’s-Andronov’s community conceived the idea that there was something wrong in their standpoint well before the above reappraisal of the conceptual values was announced. In the previous section, Zhabotinskii’s contribution to the theory of periodic chemical reactions was outlined. By referring to this contribution, we explained how the “self-oscillatory ideology” had worked. As was mentioned, Zhabotinskii regarded himself as belonging to the fourth generation of Mandelstam’s school. At the same time, he wrote in his 1989 article that A.M. Turing’s and I. Prigogine’s work made it possible that his research was launched regarding the mechanism and mathematical modeling of periodic reactions. Let us recall this story. Belousov discovered the concentration oscillations in a homogeneous chemical reaction in 1951. However, the scientific community was cool toward his discovery: His submission was twice rejected by chemical journals. In the long run, he published his celebrated article in the little-known “Sbornik referatov po radiatsionnoi meditsine” “Collected papers on radiation medicine” in 1959.

In the meantime, the “scientific public opinion” was beginning to change. In 1952, Turing proposed his dynamical model of morphogenesis. He showed that we could

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[9] As was noted above, Academician Andrei Victorovich Gaponov-Grekhov is a former Andronov’s student, Professor Mikhail Izraiilievich Rabinovich is a former student of Gaponov-Grekhov.

reach stable spatial patterns by combining chemical oscillations and diffusion. In 1955, Prigogine showed that in an open chemical system in a state far from thermal equilibrium stable regimes (dissipative structures) could appear. “From these works the modern stage of research in chemical oscillations originates”, Zhabotinskii wrote [394, p. 8].

To illustrate further how the dissatisfaction with its paradigm has appeared in Mandelstam-Andronov’s community let us consider a piece of the history of studies in turbulence. As is well known in 1944, L.D. Landau proposed the theory of turbulence which became very popular (sometimes this theory is named “the Landau-Hopf theory” because of Hopf’s 1948 contribution). According to this theory, the development of turbulence occurred in the case of weak excitation in the following way: with increasing the Reynolds number, the oscillatory flow became unstable and more complicated flow with two (incommensurable) characteristic frequencies arising. Then after a next critical value of the Reynolds number, a third mode with a new (incommensurable) frequency arises. In this way finally the flow was characterized by n incommensurable frequencies.

In Mandelstam-Andronov’s school, the Landau-Hopf theory was interpreted in terms of self-oscillations: Turbulence resulted from a superposition of many incommensurable self-oscillatory modes. About this Rytov wrote in 1957 [303]. However, an essential step was made by M.I. Rabinovich who put forward a “self-generator gas model” in his 1974 article “Self-oscillations in distributed systems” [290]: Turbulent pulsations were represented by N uncoupled oscillators (driven by the flow) which are randomly distributed in the fluid. In the opinion of the rival (and friendly) community, “this model is of course oversimplified but represents at least one typical property of the transition to turbulence” [93, p. 174].

Let us compare, however, this Rabinovich article and his subsequent 1978 article “Stochastic self-oscillations and turbulence” which was mentioned above. The latter was written under the influence of the idea of the dynamical chaos introduced by David Ruelle and Floris Takens in 1971. The former Rabinovich’s article had been restricted by the traditional view of turbulence as a statistical effect of a very large set of oscillatory modes (Landau and Hopf shared this view). In his 1978 article, Rabinovich had already taken into account Ruelle and Takens’ non-classical view of the origin of stochasticity. As is well known, Ruelle and Takens showed that after a number of Landau-Hopf bifurcations the system generated a “strange attractor”, which could be outlined as follows: a particular region in the phase space is attracted by the trajectories, the paths of which depend very sensitively on the initial conditions. It is remarkable that the system could be simple enough, that is, it could be placed in the three-dimensional phase space.

Challenged by the present author, Rabinovich gave the following explanation of what happened between 1974 and 1978:

> Turbulence appears when very many degrees of freedom are excited. This is a motion of a tremendous system with the tremendous number of degrees of freedom. When I was speaking about self-oscillatory modes and a gas of self-generators, I unconsciously left the question about the origin of stochasticity open. Early that was a “random phrase approximation”.

> There was no profound explanation. One needs to have the strange attractor to explain the origin of stochasticity.
>
> I was shocked by the strange attractor. I learned about it in 1975. I regrettably have a lack in understanding of mathematical works. I participated in the conference (school) nearby Moscow. The very good mathematician V.I. Yudovich participated, too. He said to me that he had recently read a lovely article.[10] This article contained a reference to Ruelle and Takens. Apart from this, the authors archeologically discovered Lorenz' paper about which nobody knew. They made a great publicity for Lorenz who, by investigating a simple dynamical system, the system of thermoconvection in the atmosphere, found non-periodic phase flows in it. His article is namely entitled after these flows.

Mandelstam-Andronov's school came very close to the discovery of strange attractor and dynamical chaos. The interviewed persons emphasized that in essence dynamical chaos had been described in the works of the members of their community. One of the earliest works to which they referred was A.S. Alexeev's 1952 work on an on-off controller with proportioning band. Having used the "four sheets" phase plane and applied the point mapping, Alexeev described a very complicated mode of behavior that this controller showed under some conditions [10]. Following Andronov's classification of oscillatory motions, he regarded this regime as Poisson stable. This meant that this regime showed rough repeatability, repeatability without regularity in time (according to Andronov, the Poisson stable motion is the most general type of the central motion, the recurrent motion which was repeatable and regular in time succeeded the Poisson stable motion, then in decreasing order of generality near-periodical, quasi-periodical, and periodical motions were successively placed).

Although the members of Mandelstam's-Andronov's community observed phenomena which were subsequently called "strange attractors", they did not "translate" these phenomena into scientific problems. The main discoveries were made outside the community. Moreover, as indicated in the above interview, this community needed the outside impulse to adopt such things as the "strange attractor" and "dynamical chaos".

Confining their paradigm to simple systems, the members of Mandelstam-Andronov's community took the ideology of determinism, which implied that a given state of a system at one time issued in a unique state at a later time. This ideology was explicitly expressed in Andronov's unpublished Lectures on Quantum Mechanics, which he delivered in 1934. These were regular Lectures on Quantum Mechanics, with special emphasis on the theory of the Schrödinger equation. However, these lectures contained a few philosophical remarks where Andronov advocated determinism. For example, he admitted that it was possible to construct a deterministic theory which would replace the "Schrödinger-Heisenberg-Dirac theory". One could not come across such a remark in Mandelstam's thoughts on quantum mechanics.

Neymark also argued in favor of determinism in some of his writings. For example, he offered to include determinism in the very definition of a physical system in his 1978 book [246, pp. 7, 8].

True, by invoking the idea of stochastic self-oscillations Neymark and Rabinovich attempted to overcome the "self-oscillatory paradigm" with the help of the

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[10] Rabinovich referred to [227].

"self-oscillatory ideology" [246, 258].[11] However, the ideology cannot break away from the paradigm. Neymark's attempt to invite non-classical stochasticity was confronted with his explicitly expressed determinism. Rabinovich, as he was cited above (see, Sect. 10.3), eventually gave up the term "stochastic self-oscillations": "Now I don't use the term "stochastic self-oscillations". What is the use of forcing an open door, since the concepts of a dissipative structure and dynamic chaos arose?"

Zhabotinskii, at the beginning of his work, followed the "self-oscillatory paradigm". He described the concentration oscillations as similar to the self-oscillations of lumped systems. However, he drew on "self-oscillatory ideology", when he described and explained what Hokhlov called "self-waves". Moreover, the concentration self-oscillations could not be deeply understood within the framework of the "self-oscillatory paradigm".

As the "self-oscillatory paradigm" collapsed, the theory of non-linear oscillations came close to regular Western non-linear dynamics. The "self-oscillatory ideology" turns into ornamentation which could be easily abounded. True, some members of Mandelstam-Andronov's community continue to keep the "self-oscillatory paradigm" and hence the theory of non-linear oscillations as a specific scientific area.[12]

By proclaiming that the theory of non-linear oscillations could not be built as an auxiliary device of linear theory and that physicists should be educated in "non-linear thought", L.I. Mandelstam inaugurated the new non-linear tendencies in science.

In this chapter a "Russian piece" of the history of the theory of dynamical systems has been presented. What are the personal trajectories of the main charactors of the described story? Y.I. Neymark died in 2011. A.V. Gaponov-Grekhov is Academician, the Advisor of the Presidium of Russian Academy of Sciences. M.I. Rabinovich left Russia for the USA.

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[11] In connection with Neymark-Landa's book *Stokhasticheskiie i khaoticheskie kolebania* "Stochastic and Chaotic Oscillations" [253] Y.I. Neymark said to the present author that this title should be read *Stochastic and Chaotic Self-oscillations*.

[12] The position of Andronov's school in the development of non-linear dynamics toward the theory of chaos is also outlined in [31, 84, 85].

Chapter 11
Moscow State University and the Academy of Sciences

11.1 Institute of Physics (Academy of Sciences of the USSR)

An important event in the history of Soviet science took place in 1934: the Presidium of the Academy of Sciences of the USSR left Leningrad for Moscow. Several institutes moved following the Presidium. The Institute of Physics of the Academy of Sciences moved also in 1934.

When the Academy of Sciences left for Moscow, such previously started processes as adaptation of the Academy of Sciences to a new political reality enhanced. As early as in the late 1920s, the Presidium of the Academy of Sciences became integrated into the governing bodies of the Soviet State. A party appointee has become indispensable academic secretary, and scenario written by authorities was felt in the election process (e.g., A.M. Deborin mentioned above was elected as Academician according to this scenario). Management of the Academy was required to organize research planning and bring science to life (see, e.g., [7, 260]).

At the same time, the government step-by-step enhanced material and financial support of the Academy. New buildings were constructed, and new equipment was purchased and supplementary benefits paid for academic titles increased. Establishment of the Soviet Academy has already reached the status of party and governmental elite in the postwar period.

With moving of governing bodies of the USSR Academy of Sciences, fundamental science has become restructured. In the USSR, fundamental science has been mainly developed in the universities and academic institutes. Large research institutes were also included in the Supreme Council of National Economy (e.g., Leningrad Institute of Physics and Technology led by A.F. Ioffe).[1] The structure has become changed since 1934–35. Although a number of research institutions have still officially belonged to the universities, their role has become less important. At the

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[1] VSNKh—Supreme Council of National Economy. Later the Institute of Physics and Technology was transferred to the People's Commissariat of Heavy Industry (NKTP).

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same time, institutes of the Academy have gained momentum. Institutes previously included in the structure of the Supreme Council of National Economy (VSNKh) were turned over to the Academy of Sciences. For example, the Leningrad Institute of Physics and Technology became subordinated to the Academy of Sciences in 1939. The department of chemical physics led by N.N. Semenov and separated from Institute of Physics and Technology entered the USSR Academy of Sciences in 1940 after having got the status of independent institute.

As early as in 1933, directors of the Research Institute of Physics (NIIF) planned to promote and develop the institute.[2] During the second five-year plan (1933–1937), this institute was designated as a leading institute in Moscow (Institute of Physics and Technology headed by A.F. Ioffe was considered a leading one in Leningrad). The program for the development of Research Institute of Physics during the second five-year plan is based on the "objectives of the socialism building" [398, 46-1-69]. However, by Autumn of 1934 many leading physicists had been moved from the Research Institute at the University to the Institute of Physics of the Academy of Sciences (FIAN) led by S.I. Vavilov, former Researcher of the Research Institute of Physics (NIIF).[3] Here is an extract from orders for the Institute of Physics (FIAN): "Take on the stuff the following persons since October 15 of 1934: Tamm with a salary of 500 rubles, Landsberg with a salary of 500 rubles, Leontovich with a salary of 450 rubles, Rytov with as a salary of 350 rubles". In 1935, A.M. Divilkovsky and S.L. Mandelstam (son of L.I. Mandelstam) were taken to the Institute of Physics (FIAN). Officially, Tamm, Landsberg, Leontovich, Rytov, and S.L. Mandelstam were employed in Moscow State University, and no orders on their retirement were issued. However, their research activity was almost completely focused on the Institute of Physics (FIAN).

B.M. Hessen was taken on the staff of the Institute of Physics (FIAN) on March 1 of 1935 as a senior specialist and soon he was appointed Deputy Director of the Institute of Physics (FIAN) for research work [401]. Therefore, Hessen at the same time being director of the Research Institute of Physics (NIIF) became Deputy Director of the Institute of Physics (FIAN). This fact seems symbolic. Though S.I. Vavilov later said he had appointed Hessen as assistant director on the "firm recommendation" of Landsberg, Mandelstam, and Tamm,[4] this appointment was likely to be agreed with the Communist Party authorities. Was Hessen entrusted to reduce university physics

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[2] Report data presented in 1927 by director of this institute Romanov indicate that Research Institute of Physics and Crystallography at Moscow State University was leading research institute in this field: 10 full members worked in the institute, six of them did not belong to the staff of Moscow State University (MSU), "16 persons are employed in the institute at all, and half of them does not belong to the MSU staff. Although the Institute of Physics and Crystallography is officially assigned to MSU it actually includes researchers from the majority of Moscow higher learning institutions and higher technical educational institutions and therefore it is an institution of all-Moscow scale" [401, fund 40, inventory 1, depositary items 2].

[3] As Vavilov wrote in his diary, "FIAN should become the central and theoretically basic institute in Moscow" (see [351]).

[4] See papers of G.E. Gorelik [139, 140].

in favor of the Academy of Sciences? Of course, the hypothesis consisting of this rhetorical question needs to be verified.

Dean of the Faculty of Physics S.E. Chaikin was appointed to the position of senior specialist in the Institute of Physics (FIAN) on October 23, 1935.

Finally, in December of 1935 according to the solution of the Presidium of the USSR Academy of Sciences, Academician L.I. Mandelstam was taken with a salary of 750 rubles, and Academician A.F. Ioffe was taken with a salary of 600 rubles to the Institute of Physics (FIAN). The order on their taking on the staff was signed by assistant director B.M. Hessen.

Appointment of the director of the Institute of Physics and Technology A.F. Ioffe to the Institute of Physics (FIAN) needs to be commented. There is no evidence in archive documents that Ioffe took any participation in the affairs of the Institute of Physics (FIAN): He neither led there in any research area nor solved any problems. His appointment to the Institute of Physics (FIAN) complied with the policy of the leadership of the Academy of Science to concentrate the best Soviet physicists in this institute. Physicists V.A. Fok (Institute of Physics and Technology), Yu.A. Krutkov (State Optical Institute), and M.P. Bronstein (Institute of Physics and Technology)[5] from Leningrad were appointed earlier, in May of 1934. Vladimir Aleksandrovich Fok (1898–1974) actually worked in the Institute of Physics (FIAN), and his name is mentioned in the academic trip orders [401]. N.D. Papalexy was employed by the Institute of Physics (FIAN) in February of 1935. In February of 1936, due to leaving for Moscow he started at a salary of 1000 rubles.

The Institute of Physics (FIAN) was originally located in the building of the Institute of Physics and Biophysics on Miusskaya square. However, in November of 1934 the committee was established to support construction of a new building. Besides director S.I. Vavilov, the committee included L.I. Mandelstam, A.F. Ioffe, G.S. Landsberg, B.M. Hessen, and some other physicists. The construction of the Acoustic building of the Institute of Physics (FIAN) was completed before the war. This building was located in the place of the present Institute between Leninskiy prospect and Vavilov street.

L.I. Mandelstam was a member of Academic Council of this institute from the beginning of its work.

The Institute of Physics has originally the same structure as Research Institute of MSU. The laboratory of oscillations has been led by N.D. Papalexy since 1935. The laboratory of optics included L.I. Mandelstam was led by G.S. Landsberg, and the theoretical department was led by I.E. Tamm.

When B.M. Hessen was arrested, G.S. Landsberg was entrusted with the duty of assistant director for research work; however, he was relieved of his duties of assistant director and head of optical laboratory on May 16 of 1937. M.A. Leontovich was appointed to the position of the head of this laboratory.

The Institute of Physics (FIAN) has grown. According to booklet [131] data, the staff of the institute consisted of 77 employees in 1936, and 38 persons were

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[5] Structure of physics in Leningrad are described in the book of G.E. Gorelik and V.Ya. Frenkel [142, pp. 48–51].

researchers (14 doctors of sciences and 16 candidates of sciences). The institute's budget accounted for 929 thousand rubles.

In 1937, the institute underwent structural and personnel changes. Particularly, the laboratory of solid surface structure was transferred to another institute. The laboratory of atomic nucleus led by corresponding member of the USSR Academy of Sciences D.V. Skobeltsin was included in the FIAN.

According to an Order for the Institute of Physics (FIAN) dated September 16 of 1938, the theoretical laboratory was abolished "in order to make theoretical subjects closer to experimental laboratories and objectives of national economy and socialist construction" and its personnel was distributed among other laboratories. However, I.E. Tamm continued to earn his previous salary, and he remained responsible for "organizing regular theoretical colloquia and supervision over the work of employees adhered to him".

In 1939, the personnel of the Institute of Physics (FIAN) reached 96 employees, 52 persons were researchers, four of them had the title of academicians, seven were corresponding members of the USSR Academy of Sciences, nine were doctors of sciences, and 15 were candidates of sciences. The budget accounted for 1.33 million rubles [399].

The author of this book does not have any data on financing of the Research Institute of Physics at MSU (NIIF) in the last half of 1930s. However, A.S. Predvoditelev (in his paper issued in the first postwar years) mentioned that the Research Institute of Physics at MSU (NIIF) was funded residually. The NIIF earned basic money by carrying out works ordered by other organizations [288].

In any case in 1934, the NIIF faced shortage of personnel. Under the de jure status, Tamm, Landsberg, Leontovich, and S.L. Mandelstam de facto left the NIIF as researchers. A.A. Andronov has worked in Gorky since 1934. Later G.S. Gorelik also moved to Gorky. A.A. Vitt and S.P. Strelkov are the only direct Mandelstam pupils who continued working in the NIIF. Besides, Vitt has cooperated with the School of Chemistry.

11.2 School of Mandelstam, Research Institute of Physics (NIIF) and Institute of Physics (FIAN)

Thus, almost all nearest employees of L.I. Mandelstam and his former post-graduate students have been transferred to the FIAN since 1934.

M.A. Leontovich said in his autobiographic interview: "1934—Institute of Physics (FIAN). All research is transferred there from MSU… At that time the institute was situated on Miusskaya square" [204, p. 433].

In turn, S.M. Rytov mentioned the following:

"In 1934… I was taken to the Institute of Physics (FIAN) organized in the same year as researcher—firstly, in G.S. Landsberg's optical laboratory and later in laboratory of oscillations led by N.D. Papalexy. However, main places of my communication with M.A. Leontovich were Mandelstam's lectures and workshops in the Research Institute of Physics (NIIF) at MSU, as usual, but not the FIAN" [12, p. 44].

Thus, the Faculty of Physics and the NIIF of MSU remained the center of physics teaching although research was transferred to the FIAN [12].

Sometimes transfer of the employees nearest to L.I. Mandelstam to the FIAN is explained by changes in the moral environment in the Research Institute of Physics (NIIF) of MSU. It seems to be partly true. As mentioned above, several months before his arrest B.M. Hessen took a sabbatical vacation and appointed S.E. Chaikin on the position of director of the Research Institute of Physics (NIIF). Before that time, Chaikin became a Dean of the School (Faculty) of Physics. Chaikin has not held these positions for a long time. As early as in 1937, A.S. Predvoditelev, repeatedly mentioned above, was appointed on the positions of Dean and director. Mandelstam and some his employees had apparently ambivalent attitude to Predvoditelev (see Chap. 6, Sect. 6.2, Chap. 8, Sect. 8.1). Theoretical aspirations of Predvoditelev were sharply criticized by I.E. Tamm. However, as mentioned above, Mandelstam gave positive testimonial of Predvoditelev with regard to nomination of the last one for the academic degree of doctor of physics and mathematics.

The matter is not only in this appointment. Arrest and execution of Hessen was interpreted by some employees of the Research Institute of Physics as a blow to L.I. Mandelstam's circle. Personalities against G.S. Landsberg and I.E. Tamm were found at the meetings devoted to approval of arrest and execution of B.M. Hessen.[6] It cannot go unnoticed that transfers to the FIAN started in the period of bloom of the NIIF led by B.M. Hessen and with his personal participation. The further course of events allows different interpretation. For example, as said in such distinguished document as "Memoirs" of A.D. Sakharov, the physicists of L.I. Mandelstam's circle were forced to leave the Faculty of Physics of MSU (see [306, p. 74]).

However, E.L. Feinberg clarified this opinion in his comments on A.D. Sakharov's "Memoirs" [306, p. 765]. He noted that Landsberg, Tamm, and Mandelstam were discharged from MSU in the beginning of war because MSU was evacuated to Ashkhabad and the FIAN, with which Landsberg and Tamm were evacuated, was moved to Kazan. Vladimir Vasilievich Migulin (1911–2002) who worked under academic supervision of L.I. Mandelstam and N.D. Papalexy in the second half of 1930s and was more familiar with the examined events emphasized the events in another way.[7]

Here is an extract of the interview given by him on November 19, 1989, to G.E. Gorelik (the record is stored in the Department of History of the American Institute of Physics).

"Question. Could not you say that Mandelstam's disciples had not been forced to leave the university before the war?

Answer. I find it is difficult to say. This was probably felt just before the war but it was rather perceived as a transfer to the FIAN's laboratory of oscillations and optics

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[6] See the book of A.V. Andreev [12].

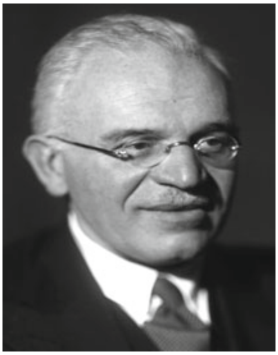
[7] V.V. Migulin is a coauthor of a book on the theory of oscillations [232] (it was cited in Chap. 8, Sect. 8.5).

that was better place of employment with more opportunities and better resource base where the studies were better and more interesting, and it was regarded as a transfer to the institution for more active and creative work".

We venture to assert that the romantic period in the history of L.I. Mandelstam's school finished after arrest and execution of B.M. Hessen. Freedom has started to be understood as the "appreciation of necessity".[8] Mandelstam was probably saddened to know that one of his disciples, A.M. Divilkovsky, angrily reviled another disciple, B.M. Hessen, at the meeting of active members of the FIAN after arrest of the latter. As mentioned above (Chap. 9), A.A. Vitt who got enthusiastic testimonial from Mandelstam for a planned academic trip to Germany was arrested in 1937. The son of A.A. Vitt testified in his speech at the workshop dedicated to the memory of his father in the Institute of History of Natural Science and Technology of RAS (October of 2002 [168]) that as far as he knows nobody interceded for A.A. Vitt. It is only known that G.A. Bendrikov, a post-graduate student of A.A. Vitt, who was the secretary of local Komsomol (Komsomol is the Young Communist League) organization in that time visited the relevant authorities to find out what was the matter. He was told that there was a German plot for which reason many people were arrested, and he was advised not to be concerned with this problem anymore.

Vitt died in the concentration camp in 1938 and had never got to see his little son born after his arrest.

11.3 Radiointerferometry



N.D. Papalexy

The last large research project realized by L.I. Mandelstam and N.D. Papalexy was the radiointerferometric method of the study of the propagation of radio waves.

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[8] A quotation from Engels' "Anti-During". Soviet philosophers-ideologists traced back this statement to Hegel's dialectics and Spinoza's philosophy.

As stated in the biography of L.I. Mandelstam, “radiointerferometric studies of L.I. Mandelstam and N.D. Papalexy have been reported in papers only since 1937 when the development of this method achieved certain perfection” and the first positive results have already been obtained. However, the first application certificate is related to 1930, and the idea was originated earlier (The first application: L.I. Mandelstam and N.D. Papalexy. “The method of measuring distance between two points using electromagnetic waves”. Submitted on December 16, 1930. Author’s Certificate was dated August 31, 1932. This application was followed by two other ones submitted in 1933. Coauthor of the second application was also E.M. Rubchinsky, Researcher of Central Radio Laboratory).

The solution of the practical problem of exact distance measurement using radio waves was the main reason playing a key role in the origin of this research trend.

L.I. Mandelstam and N.D. Papalexy met with the problems of emitting transmitter location as far back as in Strasbourg where they took participation in experiments on direction finding using the loop antenna of F. Braun (just before the beginning of World War I). Comparison of the principal disadvantage of location finding (decrease of linear accuracy with distance) with the opportunities of detection using triangle sides instead of angles, insufficient accuracy of the pulse methods available in that time and, finally, very precise interference methods used in optics to measure length and based on continuous coherent radiation—this was the way that leads L.I. Mandelstam and N.D. Papalexy to the idea of radiointerferometry” [1, Vol. 1, pp. 47–48].

Pulse methods underlying radiolocation were mentioned here. In the pulse method, direction finding is based on measuring the angles of the triangle formed by locating stations and the object’s location.

Let us quote an explanation given by G.S. Gorelik in his manual (see also [230–232]). “Along with pulse method, he wrote, radiointerference method created by L.I. Mandelstam and N.D. Papalexy is another method to measure distance using radio waves that is of great importance. They were awarded the Stalin Prize of the first degree in 1942 for the invention of radiointerference method. Here, we explain operating principle of the unit type based on radiointerference method.

Let the transmitter emitting into space a sinusoid (non-modulated) radio wave be located in point P_1 (immovable and placed on the ground surface)

$$a_1 \cos(\omega t - kr_1),$$

where r_1 is the distance from point P_1 to the point of space considered, a_1 is the decreasing function of r_1.

Let a ship or plane (movable point P_2) bears the receiver tuned into the wave and the transmitter which oscillations are synchronized at a frequency ω with oscillations generated at the output of high-frequency part of the receiver. The receiver is subjected to oscillations proportional to

$$\cos(\omega t - kR),$$

where R is the distance between points P_1 and P_2. The transmitter's antenna of point P_2 emits a wave of the following type:

$$a_2 \cos(\omega t - kR - \alpha_2 - kr_2),$$

where a_2 is the phase displacement detected by point equipment P_2, r_2 the distance from point P_2 to the point of space considered, a_2 the decreasing function of r_2.

Finally, let the receiver tuned into frequency be in point P_1. The receiver is subjected to superposition of oscillations

$$A_1 \cos(\omega t - a_1),$$

coming directly from the transmitter (a_1 is the phase displacement on the way from transmitter to receiver) and oscillations

$$A_2 \cos(\omega t - 2kR - \alpha_2),$$

coming from the transmitter P_2. The intensity (squared amplitude) of the oscillations in the receiver's circuit located in the point P_1 is proportional to

$$A^2 = A_1^2 + A_2^2 + 2A_1A_2 \cos(\cos 2kR + \alpha_2 - \alpha_1)$$

We obtained an equation of the same type as used for the intensity in the interference experiments considered above.

When the distance R is changed, A^2 is changed sinusoidally. Let us know the distance R_0 between the stationary station P_1 and ship (plane) at some initial position of the latter. When this distance is increased, the intensity A^2 will alternately reach maximum and minimum values. Transition from one maximum to another one ("passing one interference band") corresponds to the change of the distance R by $\frac{\lambda}{2} = \frac{u}{2\nu}$, where ν is the frequency, u is the phase velocity under experimental conditions. By calculating (by readings of measuring device that shows the value of A^2) the number n of interference bands passed (in general case, the number is *not* integer) and knowing the wave phase velocity, we will find R from the equation

$$n = \frac{2\gamma}{u}(R - R_0).$$

We described the simplest possible pattern of the interference of *radio log*.[9] However, this circuit is practically unfeasible for the following reason: The amplitude A_2 is always very small in comparison with A_1, and the waviness of the curve of relation A^2 versus R is impossible to observe; the amplification of the oscillations

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[9] In marine affairs, the device measuring the distance covered by a ship is called a log. The simplest log consists of a float thrown overboard of thin rope and on a reel for unwinding this rope.

coming from P_2 to P_1 cannot be useful there because the frequencies of both oscillations entering the receiver are equal and they will be amplified to the same extent. Thus, actual radiointerference devices are designed according to a more complicated pattern: Oscillations of the P_2 point transmitter are synchronized not at frequency ω, but at another frequency ω^{j} related to ω as small integer numbers (as follows from the theory of the resonance of order n (see Sect. 9.5), and such synchronization is also possible in self-oscillating systems—A.P.). Usually, the transmitter P_2 is synchronized at a frequency of $2\omega/3$. Let the wave emitting by point P_2 be given by[10]

$$a_2 \cos\left[\frac{2}{3}(wr - kE - kr_2 - \alpha_2)\right],$$

and oscillations of the type

$$A_1 \cos(\omega t - \alpha_1), \quad A_2 \cos\left[\frac{2}{3}(\omega t - 2kR - \alpha_2)\right]$$

be converging (joined or combined—A.P.) in the receivers of point P_1.

Using resonance amplification at frequency $2\omega/3$, the amplitudes of output oscillations can easily be given values of the same magnitude. Now the total intensity is simply the sum of the intensities A^2, A^2, and to determine R, we should use the method (unknown in optics but easily accessible in radiophysics) of direct measuring of the normalized phase difference

$$2kR + \alpha_2 - \alpha_1$$

between converging oscillations. This is carried out using an electron oscilloscope: Oscillations are delivered to *both pairs* of deflecting plates and the Lissajous pattern is observed.

This is the operation principle of one of the typical radiointerference devices" [136, pp. 296–298].

The history of work on radiointerferometry is expounded in the book devoted to Central Radio Laboratory where all these studies started in 1930–1931. In that time, "L.I. Mandelstam and N.D. Papalexy offered and used in cooperation with E.Ia. Schegolev and E.M. Rubchinsky the method (in different versions) of measuring distances using radio waves propagating above ground and water surfaces….

The radiotechnical method of measuring distance was developed in two versions: one of them used a movable interferometer and another one the radio rangefinder. Later on the third method was developed based on the radiosonde, in which the phase field was generated by two fixed stations situated at some distance from each other

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[10] By synchronization, $\cos \omega t$ is transformed into $\cos \frac{2}{3}\omega t$ and therefore the oscillation $\cos \omega(t - t_0) = \cos(\omega t - \varphi)$ is transformed into the oscillation $\cos\left[\frac{2}{3}\,\omega\,(t - t_0)\right] = \cos\left[\frac{2}{3}(\omega t - \varphi)\right]$, whence follows the formulae given in the text.

and the location of the movable object was determined by the path length difference from each of these stations….

Important experimental data on the radio wave propagation speed was first obtained under real conditions during field studies conducted in cooperation by two institutions, Central Radio Laboratory (CRL) and the Research Institute for Geodesy, Aerial Survey and Cartography, in 1934 in the Caucasus in the area of Pyatigorsk. Measurements were taken at five different stations. The radio rangefinder was used in this method….

Further experiments were conducted above the sea surface in spring of 1935 in the area of Odessa using both methods of radio rangefinder and radio log…" [260, pp. 100–104].

In 1935, the laboratory of high-frequency physics formed a part of CRL and led by Mandelstam and Papalexy was transferred to Leningrad Institute of Electrophysics where Papalexy was a head of the Department of Radiotechnical Science from 1926 to 1934. Due to reorganization of Leningrad Institute of Electrophysics, the department led by Papalexy was transferred to Leningrad Industrial Institute where Papalexy was Professor of the Department of Radio Physics. As a result of these perturbations, the subject of radiointerferometry, as S.M. Rytov stated, was concentrated in the laboratory of oscillations in the Institute of Physics of the USSR Academy of Sciences (FIAN) in Moscow led by Papalexy [304, p. 26].

As mentioned above in the beginning of this section, Mandelstam and Papalexy were awarded the Stalin Prize of the first degree in 1942 for their radiointerferometry studies (to be more precise, they were awarded for their total contribution to science, but the method of radiointerferometry was specially recorded in the decision of Committee). Their money award was 200,000 rubles. "Perhaps, by that time the money will be paid their market price may hardly be sufficient to buy two flour pounds", N.D. Papalexy wrote to L.I. Mandelstam in 1942 after having shared impressions of colleagues congratulations on the occasion of awarding the Stalin Prize [401, 1622-1-82-33].

In conclusion of this section, we will give a fragment of an interview with A.M. Prokhorov (Nobel Prize winner in 1964) taken by A.B. Kojevnikov on October 11, 2000.

A.M. Prokhorov (1916–2002) after graduation from the Faculty of Physics in Leningrad became a post-graduate student of the FIAN in 1940. He worked in the laboratory of oscillations led by N.D. Papalexy. His direct supervisor was V.M. Migulin. A.M. Prokhorov said [163]:

In that time, we dealt with distance measurement for cartography using radio waves.

- **Was it the radiointerference method?**
- Yes, it was, but we failed to use it because radio wave propagation considerably depends on soil composition, and therefore, the optical path between two points, radio path, is constantly changed. For this reason, the distance is hard to be determined exactly.

- **However, Mandelstam and Papalexy were awarded the Stalin Prize for their work**.
- Yes, but the matter is that you would not necessarily obtain directly designed results while working in science. Sometimes collateral results are also very important, and this is the foundation of science. You do one thing, but get something other, which is far better than you have planned.

11.4 Ya.L. Alpert, V.V. Migulin, and P.A. Ryazin

As shown in the papers, three young scientists of the FIAN, whose names are mentioned in the title of this section, were among those who took participation in the experiments with radiointerferometry. In a sense, they are people of the next generation following after the generation of Mandelstam's post-graduate students. As their predecessors, they are devoted to science and were loyal citizens. But there were no such friendly relations between them so as to join them into a consolidated team (even temporally) as worked under the guidance or with participation of L.I. Mandelstam or even without his participation.

Yakov Lvovich Alpert (born in 1911) immigrated to the USA in the second half of 1980s (the crisis of communism in Russia) and wrote a book of memoirs in English. Therefore, his creative path can be traced easier although milestones of his way were determined by himself.

Ya.L. Alpert arrived from Kiev, graduated from the Faculty of Physics of MSU and was taken into the FIAN. While working with radiointerferometry, he made flights in a balloon and climbed mountains. "At the end of 1939 and the beginning of 1940 I made several flights in a balloon to study how the phase structure and velocity of radio waves change as the waves propagate up and away from a ground-based source. The point of the flights was to ascertain at what altitude the radio waves ceased to be influenced by the earth. For these experiments and some earlier ones I designed a new variant of the radio interferometer for measuring the phase difference between two coherent radio waves radiated from a single point. The new instrument was built in FIAN.

The regular radio-interferometer, consisting of a transmitter and a receiver, could only send and receive waves on one frequency at a time. The dispersion interferometer had two transmitters, whose could be adjusted separately, and two receivers with recording devices. The receivers measured the difference in phase of the two frequencies" [11, p. 86].

The dispersion interferometer is also mentioned in V.V. Migulin's biography, but here, it is considered V.V. Migulin's invention [231, 261]. His paper issued in "Uspekhi Fizicheskikh Nauk" ("UFN") (1947) explained the operation principle of this interferometer. Though Migulin referred to the papers written in coauthorship with Alpert, he did not specify who was inventor of the interferometer.

In his autobiography, Alpert wrote that Migulin not only read his paper on dispersion interferometer, he gave him to review as his direct supervisor but also signed it, i.e., became one of the authors. Alpert tried to complain to Papalexy who turned sulky and said: “So be it”.

V.V. Migulin worked at the laboratory led by Papalexy, in Leningrad Institute of Electrophysics (LEFI). He was involved in the development of radiointerference equipment since 1933. As S.M. Rytov wrote, “V.V. Migulin develops paired receiver (radiosonde) and its deviometer, quartz free oscillations are studied. The radiointerference method was tested at various distances near Leningrad in 1934. During these tests the first experiments were carried out to study phase structure of radio waves field using paired receiver and measuring radio waves dispersion”. [305, p. 24].

Petr Aleksandrovich Ryazin (1908–1984) came to the FIAN from the NIIF of MSU. He prepared a Ph.D. dissertation on “forced synchronization and suppression in self-oscillating systems” under the guidance of A.A. Vitt.

P.A. Ryazin took part in radiointerference experiments. His theoretical achievements are also distinguished. The paper of L.I. Mandelstam and N.D. Papalexy contains detailed description of his contribution to A. Sommerfeld’s theory of surface waves mentioned in Sect. 4.7 of Chap. 4.

The group of three (Alpert, Migulin, and Ryazin) was a model group of the FIAN young scientists. The article devoted to them was published in the central press (newspaper “Pravda”, November 15, 1940); they were nominated for the Stalin Prize in 1940, and this nomination was supported by the Presidium of the USSR Academy of Sciences. However, they were not awarded the Stalin Prize.

Ya.L. Alpert described the history of nomination for the Stalin Peace Prize in his memoirs mentioned above. A.Ya. Alpert also recalled the Stalin Peace Prize of the first degree awarded to Mandelstam and Papalexy in 1942. Alpert wrote that soon after this event, he received the letter of Mandelstam and Papalexy, in which his contribution to radiointerferometry was recognized and a “substantial part of their money award” was attached.

These three last “graduates of L.I. Mandelstam’s school” had different fates. A.Ya. Alpert worked in the FIAN and in the Institute of Geomagnetism and Radio Waves Propagation of the USSR Academy of Sciences and dealt with radiophysics, plasma physics, and cosmic rays. As mentioned above, he immigrated to the USA in the middle of the 1980s.

V.V. Migulin became a director of secret institute of physics in Sukhumi in the first postwar years where interned German specialists worked. In 1947, he published in the journal “Uspekhi Fizicheskikh Nauk” (“UFN” in Russian) the paper on radiointerferometry represented his doctorate thesis. V.V. Migulin achieved the highest social status available to the Soviet scientist when he took the Academician’s position in the USSR Academy of Sciences. After the death of K.F. Teodorchik, he became a head of the Department of Oscillation Physics in the Faculty (School) of Physics of MSU, i.e., the department established in the beginning of the 1930s by L.I. Mandelstam and S.E. Chaikin.

P.A. Ryazin worked in theoretical radiophysics for FIAN. In the late 1940s, he was directed to enhance theoretical works in physics and technology of accelerators. He felt very sensitively his isolation from radiophysics. He did not reach great achievements in the new area of work. In the late 1950s, Ryazin became academician in the Kirghiz SSR Academy of Sciences and then even Vice-President of this Academy.

Chapter 12
Borovoie and the Last Year in Moscow

12.1 Borovoie

The letter, which has been mentioned by the end of Sect. 11.3 of Chap. 11, was sent by Papalexy, who was located in Kazan, where FIAN had been evacuated, to Mandelstam, to Kazakh resort Borovoie where aged and poor health academicians had been evacuated. Mandelstam together with his wife, grand-daughter, and daughter-in-law left Moscow for Borovoie 16 June 1941. In Borovoie, he came to feel better, walked much, and worked.

In 1942, he informed the Presidium of AN USSR that he worked on the following two themes: "(1) a theoretical development of some problems related to the general theory of oscillations and to the electromagnetic wave propagation (these problems are connected with those which were worked out under my and Papalexy's leadership in FIAN), (2) an analysis of some propositions of wave mechanics (the energy-time uncertainty relation)" [399, file 1622, list 1, number 57].

In 1943, he wrote approximately the same. True, he added that he works on parametric generators together with N.D. Papalexy. They were planning to write a monograph which would sum up their long research in this field. As is said in the biography of Mandelstam (Mandelstam's "Complete Works"), "this capital work is not fated to be completed. Only separated fragments (the derivation of the Lagrange equations from the Maxwell ones, the classification of electric generators) have been accomplished. They have been published in "Complete Works" Vol. 2".

L.I. Mandelstam took hard his isolation from the colleagues. He shared his cares with Director of FIAN S.I. Vavilov (e.g., his 25.2.1942 letter). In reply Vavilov wrote on 11.03.1942: "Your wish to came to Kazan causes anxiety. The Kazan conditions for life and work deplorable. FIAN is located in the University, it is almost not heated, we must work in coats, in distresses, experiencing interruptions in electricity and water. Papalexy has not received even a plywood partition yet. Work is sometimes very tense, the problems have military character. Young people work, they have strength. About housing and food problems, I believe, you are informed.

A. Pechenkin, *L.I. Mandelstam and His School in Physics*,
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Besides, extraordinary concentration of physicists and generally academic people in Kazan is unbearable. Gossips, quarrels, wounded ambitions have grown up to tremendous sizes. Only for this reason, it is possible to run away from Kazan.

On this and other reasons, I ask you to give up the idea to come to Kazan at least till summer" [399, stock 1622, list 1, N 67].

In Borovoie, Mandelstam received a letter about the death of one of his graduate students, A.M. Divilkovskii. On 10.1.1942, S.M. Rytov writes about his friend who, like he, solved the problems put by L.I. Mandelstam and participated in the M.A. Leontovich seminar: "No information about Divilkovskii during more than two months. It is only known that he studied at the Military courses and was sent to Volokolamsk several days before the Germans came there. Since then no news and this causes alarm.

Yesterday he became Doctor of physico-mathematical sciences. Our scientific counsel had discussed awarding the scientific degree to him without the procedure of defense and, of course, had given this degree to him".

To communicate with L.I. Mandelstam FIAN sent his son Sergei Leonidovich, N.D. Papalexy, I.E. Tamm, and S.M. Rytov to Borovoie.

In Borovoie, L.I. Mandelstam met two prominent Russian and then Soviet scientists, the Russian intellectuals Alexei Nikolaevich Krylov (1863–1945) and Vladimir Ivanovich Vernadskii (1863–1945). About the Mandelstam–Krylov contact there is information in the biography of Mandelstam and in the recollections about him. On arrival at Moscow, Mandelstam delivered a paper "On A.N. Krylov's scientific works" at the general meeting of the Academy of Sciences (September 26, 1943).

In Borovoie at the meeting of academicians, Mandelstam delivered a paper "The optics work of Newton" (16 January 1943). In this paper, he emphasized A.N. Krylov's paper "On the Newton theory of refraction".

The Mandelstam–Vernadskii contacts were almost ignored in the literature on Mandelstam.

12.2 Vladimir Ivanovich Vernadskii[1]

Mandelstam and Vernadskii met in the Moscow–Leningrad train and came into conversation with each other. In 1938, Vernadskii attended a Mandelstam lecture about radio-interferometry. In Vernadskii's diary, the following record had appeared:

> 29.4.1938, morning.
>
> In the evening, there was Mandelstam's interesting and brilliant report at the Academy of Sciences. I listened, it as I seldom listened.
>
> Mandelstam seems to be a Jew-nationalist, who came back to the Jewry after the revolution. He brightly supported the Jewish physicists in the University: Landsberg, Hessen, et al. Now he is suspected. However, his main work has not suffered. One day we together went to

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[1] The author is grateful to Vladimir Pavlovich Volkov, who is the publisher of V.I. Vernadskii's diary [354, 355]. The author could read the original and unpublished materials.

> Moscow in one train car. We had a very interesting conversation. He impressed me by his clear and accurate thought. I saw that he is clear to me by his logic, sometimes formal logic. He is of A.F. Samoilov's type—a bright sanguine person, deep experimentalist and analyst.[2] A noble type of old Jewish culture, philosophically educated.

Here, comments are needed. First of all, it is not understandable why L.I. Mandelstam is "a Jew-nationalist, who came back to Jewry after the revolution". Mandelstam, while he was a student and then a researcher at Strasbourg University, was considered to be Jew in the university documents (see Chap. 2). There is no evidence that he has disavowed his nationality. It is another matter that probably religious issues did not play any role in his life.

What did Vernadskii mean by pointing out that Mandelstam had supported the Jewish physicists? Mandelstam supported Hessen, but Hessen was a person from the other circle. He was a party organization man. He was an organizer of Soviet Physics. By the way, people of the older generation (say, S.M. Rytov) with which the present author has communicated, usually told that Hessen helped Mandelstam, instead of that Mandelstam helped Hessen.

L.I. Mandelstam cooperated with G.S. Landsberg, who was also Jew, but the closest collaborators of Mandelstam were people of different nationalities.

It is possible that Vernadskkii had in mind the discussion which arose in connection with the experiment of the American physicist Dayton Miller, the experiment which had supposedly falsified the relativity theory. Here, two main opponents arose: K.A. Timiriazev, who wrote in favor of Dayton Miller's conclusion, and B.M. Hessen, who criticized Dayton Miller and Timiriazev (1924–1926). Their papers appeared in the prominent journals "Uspekhi Fizicheskikh Nauk" and "Pod zanamenem Marksizma". According to the evidence [223], Mandelstam, who tried to avoid any non-scientific conflicts, protested against an inclusion of Timiriazev's lecture into the program of the plenary meeting of the Fifth Russian Conference of Physicists and declared that he is leaving the organizational committee for this reason.

V.I. Vernadskii himself has not written against the relativity theory. However, he elaborated his own theory of space and time, a kind of Naturphilosophie. He has not applied and analyzed the technique of the theory of relativity.

It should be noted that Vernadskii's closest friend the famous Russian mechanician Sergei Alexandrovich Chaplygin (1869–1942) spoke against the theory of relativity (see http://www.originweb.com/net/mathematics/chaplygin/).

Vernadskii did not rank Hessen's philosophical writings high. About Hessen's paper which became world-famed, Vernadskii wrote: "Hessen's article on Newton is weak, with a superficial and flimsy erudition, he partially forces an open door" [354].

In Borovoie, Mandelstam and Vernadskii apparently met and spoke with each other rather often. In Vernadskii's diary, there is the following passage:

> 25.7.1941.
>
> I feel myself on the verge of illness… But with Mandelstam, I had an interesting conversation about Goethe. He rightly pointed to the importance of Goethe's writings on optics. I think that

---

[2] Alexander Filippovich Samoilov (1867–1930) was a physiologist, professor of MSU.

> Mandelstam is methodologically right: a complicated experiment can distort the phenomena and not always one can reconstruct scientific reality by proceeding from this experiment.
>
> Mandelstam saw Mysovskii's book about atomic nucleus on my desk… His opinion about Mysovskii, like all the physicists' opinion, is not obviously correct. Many things which he ascribes to Kurchatov, really belong to Mysovskii.
>
> I think that a neutron, which passes through matter, is an enigma. Mandelstam thinks that atom–vacuum is able to explain everything. However, could an atom, which does not carry the charge, move?

V.I. Vernadskii writes about Lev Vladimirovich Mysovskii (1868–1939), who was Head of Department of Physics at the Radium Institute which had Vernadskii as its director. In particular, Mysovskii directed the designing of cyclotron (Mysovskii's participation in early nuclear research; see [362, p. 9]). In turn, Kurchatov was Head of "Special group on atomic nucleus" at the Institute of Physics and Technology (A.F. Ioffe was Director of this institute) [Ibid.]

"Mandelstam thinks that atom–vacuum is able to explain everything". In such a way Vernadskii, who was inclined to Naturphilosophie, against which F. Braun already warned, retells the Mandelstam theoretical position which was based on the theory of relativity and quantum mechanics. Vernadskii used such notions as "caducity" of atoms, the space states which appear "non-materially, but through energy".

Let us turn to Vernadskii's diary.

> January 1, 1942. Morning
>
> A very interesting conversation with Mandelstam. January 24, 1942.
>
> Mandelstam was present at my report. His heart is ill, but he made his mind to come here. My report was interesting for him.
>
> March 1, 1943
>
> Today Mandelstam came. A conversation about the variety of spaces and symmetries.
>
> April 5, 1942. Morning.
>
> Yesterday—a talk with Mandelstam—very interesting and logical mind. He correctly said that now a physicist cannot work without philosophy. The rise of contemporary physics has been conditioned by it. The tragedy of physics caused by a lack of creative philosophical mind. The physicists, like Einstein, are most deep philosophers now.
>
> April 7, 1942. Morning.
>
> Hidden intellectual work on the relation of science and philosophy... The past called to mind. I started to read Eddington "Philosophy of physical science".

The Mandelstam–Vernadskii conversations stimulated both their philosophical reflections. In Borovoie, Mandelstam also kept a diary where he registered the philosophical statements which he liked. For example, he wrote Ludwig Wittgenstein's aphorism down: "When the answer cannot be put into words, neither can the question be put into words" ("Zu einer Antwort, die man nicht aussprechen kann, kann man auch die Frage nicht aussprechen") (see [141, p. 76]).

In his "Lectures on the theory of oscillations" delivered on his arrival from Borovoie Mandelstam cited A. Whitehead's statement that the origin of theoretical physics is connected with the application of the concept of periodicity to different matters. V.I. Vernadskii also addressed Whitehead and characterized him as an "original mathematician and metaphysicist" [353, p. 130].

So, L.I. Mandelstam, who discussed the philosophical foundations of science with Richard von Mises in his young years, had received one more philosophical interlocutor in his declining years.

12.3 Election Campaign in the Academy of Sciences

The theme of academic election sounds in Mandelstam's correspondence when he was in Borovoie.

Papalexy wrote to Mandelstam on 8.10.1942:

Apparently, large elections to the Academy of Sciences will occur at the end of the year partially in connection with the Newtonian jubilee. I spoke about Alexander Alexandrovich Andronov with him ("he" means S.I. Vavilov). He agreed to support him as a candidate to the technology department. Chaikin has also been nominated. I have a piece about Andronov that was written by you, Vavilov, and me. If this is expedient in your opinion, I shall direct it to the technology department in case of need. G.S. Landsberg as a candidate for the academicians has obviously passed through the department bureau.

The Papalexy 9.18.1942 letter to Mandelstam: "It is not known yet when the elections occur".

The following Papalexy's letters: "Again no information about the elections" (12.3. 1942).

"No news about the elections. I have a material from S.E. It lies motionless" (2.23.1943). S.E. refers to S.E. Chaikin.

N.D. Papalexy's 4.28.1943 letter says: "As you know, according to the Governmental decree, the elections will take place at the beginning of June. The elections will be restricted by the indicated specialisms. Therefore the elections of theoreticians in the full members are excluded. FIAN seems to nominate G.S. Landsberg. S.E.Ch. and A.A.A. are appropriate as candidates. I shall watch and prepare material. Preliminary applications have been made by me".

L.I. Mandelstam wrote S.I. Vavilov on 4.28.1943: "I especially sympathize Igor Evgenievich and Grigorii Samuilovich as candidates for Department of Physics and N.N. Andreev for the Department of Technology. As candidates in the Corresponding members I consider A.A. Andronov and S.E. Chaikin. I think it is better to nominate Semen Emanuilovich for Department of Technology".

Mandelstam pointed to Nikolai Nikolaevich Andreev who was head of acoustic laboratory at FIAN (he was mentioned in Sect. 9.6). In his "Lectures on oscillations", Mandelstam spoke about the "Andreev hammer".

However, to complete the picture, it is worth to mention an episode of the Mandelstam–Andronov relations. In 1933, A.A. Andronov was nominated as a candidate in a corresponding member of the USSR Academy of Sciences. The Radiophysicist

D.A. Rozhansky (he was mentioned in Chap. 5) was also nominated as a candidate as a corresponding member. However, there was only one position: Only one person could be elected. S.M. Raiskii, who was close to Mandelstam's circle, cited the following piece from Mandelstam's address to the meeting of the academicians and corresponding members: "Alexander Alexndrovich Andronov is my favorite student. He is deserving to be a member of the Academy. However, I believe that Dmitrii Apolinarievich should be elected earlier, I shall vote for Professor Rozhansky" [8, p. 217].

L.S. Pontriagin, one of the great mathematicians of the twentieth century, was rather critical with respect to the role of L.I. Mandelstam in this story. L.S. Pontriagin was Andronov's close friend, they wrote together two important articles. "I had already been Corresponding member of the Academy of Sciences, Pontriagin wrote, when Andronov was nominated as a candidate in corresponding members at our department. I remember how a group of physicists of the older generation (including Andronov's teacher Mandelstam) flunked Andronov in the elections. Instead of Andronov, they decided to elect an old physicist who was their teacher. They had been successful in this operation. This adversely affected Andronov's life. During the war his life was very hard. If he had been Corresponding member, his life would be better" [284, p. 62].

In 1943, Mandelstam endeavored in order to Andronov to become corresponding member of the Academy of Sciences. His correspondence with Vavilov and Papalexy is evidence in favor of this. Although, according to Feinberg's recollections, the Mandelstam–Andronov relations were not completely cloudless, the episode with the academic elections had not spoiled them.[3] In his letters and recollections, Andronov did not make any comment which would cast a shadow on Mandelstam.

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[3] Feinberg writes: "I was told about only one episode in which he exploded. In his office he had a discussion with his favorite young student A.A. Andronov. Suddenly Andronov, with a crimson face, rushed out of the office and through the common office, where there were other people, and ran away. The breach with L.I. lasted for several months but then, of course, everything was settled. One assumes that a young Andronov, then of pro-communistic views, tried to turn L.I. to his beliefs, provoking his fury" [104, pp. 19, 20].

12.4 The Last Year in Moscow

L.I. Mandelstam (1943)

L.I. Mandelstam came back to Moscow on September 1943. He came to participate in the session of the Academy of Sciences that included the election on its agenda. He made his mind not to return to Borovoie. Before his departure, he wrote S.I. Vavilov (4.28.1943):

“I have learned about the project of the Institute of high frequency. True, I possess scanty information. I am not sure that now there is a special need at such institute. On the other hand, I think that the oscillatory problems and laboratory should be present at FIAN. I think that the connection of the oscillatory laboratory with other laboratories of FIAN is important, especially from the perspective of future work. On returning to Moscow I personally have no neither desires, nor intentions to work at any other institute”.

Mandelstam started to work at Moscow State University. K.F. Teodorchik’s letter to him (1943, undated) evidences it: “I and our Dean have arranged that you will come back to the laboratory. If you have no objections, please write the application that you come back to the staff and start occupations from such-and-such date" [399, 1622-1-90].

Mandelstam started to deliver the course of the theory of oscillations at MSU, but he had delivered only four lectures (3.14, 3.28, 4.7, and 5.5. 1944). As it was noted in the Introduction, at the end of these lectures J.A. Fleming had been taken under criticism. This course has also been mentioned in Chaps. 7 and 9. Mandelstam's 1944 lectures did not contain anything about what Mandelstam had not said before. However, these lectures were more philosophical.

Since Mandelstam was Chairman of the All-Union Council on Radio-engineering and Radio-physics, he had to participate in the preparation for the jubilee—the fiftieth anniversary of the Popov invention of radio. Exactly, "fiftieth anniversary of the Popov invention of radio", rather than simply the "invention of radio". This was a Soviet ideologeme (of the type of the "1st May demonstration").

As S.M. Rytov recalls, two books were in preparation: "The inventor of radio A.S. Popov" and "The prehistory of radio". The first was compiled and edited by Aksel' Ivanovich Berg who was Director of All-Union Institute of radiolocation. The second was prepared by L.I. Mandelstam with the help of S.M. Rytov [38, 164].

Mandelstam needed to write the Introduction to this book. Although at that time in the USSR, one should say that radio had been invented by Popov and Mandelstam "had received documents which showed this without controversy", he left the question, who invented radio, open [164]. True, his Introduction was not finished.

On May, 18, 1944, L.I. Mandelstam was decorated by the Soviet highest order, the Lenin Order, for his "long and productive activity in the field of physics and contribution to the preparation of scientific and engineering specialists".

November, 27, 1944, Mandelstam died of a stenocardia attack (angina pectoris).

He went to sleep and did not wake up.

A.N. Krylov and V.I. Vernadskii died within a year after Mandelstam's death. In his 12.30.1944, letter addressed to Lidia Solomonovna, Vernadskii recalled how he met L.I. Mandelstam in the Leningrad-Moscow train.

In 1947 in the sanatorium of the Academy of Sciences Uzkoe (at that time it is located near Moscow, now it is in Moscow), N.D. Papalexy died of a stroke. He was there together with I.E. Tamm. He died by playing chess. Lidia Solomonovna was in a hurry to see him, but did not find him alive.

Judging on material which is kept in the Harvard University Archives, the last time Richard von Mises read a letter concerning Mandelstam in 1944, that is, in the year of his death. The prominent specialist in non-linear dynamics N. Minorsky (he was mentioned in Chap. 9) shared his impressions of a review of this field with Richard von Mises (this review was published in "Quarterly of Applied Mathematics"). Minorsky ironically wrote that "your friend Mandelstam, who is a principal author in this field, is represented as a Bolshevistic scientific boss" in this one-sided review [395, HUG 4574. 5].

In his answer, Richard von Mises agreed with Minorsky that the author of the review is of a very restricted scientific horizon. However, Richard von Mises' letter does not contain any comments concerning L.I. Mandelstam.

12.5 S.I. Vavilov on Mandelstam

In his “Diary”, S.I. Vavilov wrote that he was shocked by L.I. Mandelstam death (351, p. 226). “The terrible news. L.I. Mandelstam died yesterday. Mandelstam was the most remarkable person among scientists with whom I was acquainted. Superhuman accuracy of thinking in physics. Exceptional moral honesty under very difficult circumstances, kindness, goodness…”

In the funeral day, S.I. Vavilov wrote that “the only human being passing with whom I could speak about everything with complete understanding”.

At Mandelstam’s funeral which occurred on the honorary Novodevichii cemetery S.I. Vavilov spoke his ritual short speech. He writes in his “Diary” that he was surprisingly calm.

Chapter 13
M.A. Leontovich: From Freedom in Creativity to Nuclear Weapons

13.1 Biography

M.A. Leontovich's biography has been outlined in Chap. 8. Here, by describing Leontovich's biography, we follow "Bibliography of works of the USSR scientists: materials. Fundamental Library of the AN SSSR. Moscow. 1939"

Leontovich was born in 1903. In 1923, he graduated from Moscow State University (School of Physics and Mathematics, Department of Physics).

Starting in 1923 he worked as an assistant for Institute of Biological Physics (Ministry of Public Health).

1921–1925. Leontovich was an assistant to the Commission on Research On the Kursk Magnetic Anomaly.

From 1925 till 1926, Leontovich was a junior-teacher at Karl Liebknecht Pedagogical Institute.

Starting in October 1925, Leontovich was a graduate student at Scientific Research Institute of Physics (Moscow State University). His scientific supervisor was Academician Mandelstam.

In 1929, Leontovich became a researcher at Institute of Physics (Moscow State University).

In parallel, he was acting as a teacher at the School of Physics (Moscow State University). At the beginning, he was assistant professor, then he received the title of professor. He delivered the course of theoretical physics.

Since 1934 he works for Institute of Physics (Academy of Science) as a senior researcher.

In 1934, he received Doctor of Science degree without the presentation of a thesis, and he received the title of professor.

In 1938, he was elected as a corresponding member of the USSR Academy of Sciences.

The following data has been borrowed from the book [213].

In 1941, Institute of Physics belonging to the Academy of Science was evacuated to Kazan. Leontovich left Moscow for Kazan. In 1943, he came back to Moscow.

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In 1944 Leontovich begin working for NII 108 (Scientific Research Institute 108), which dealt with radiolocation and radiodetection. In contrast to FIAN, NII 108 belonged to Ministry of Radio Technology rather than the Academy of Sciences.

In 1945, Leontovich returned to FIAN. In 1947 after Papalexy's passing, he became the chief of the laboratory of oscillations.

In 1946 to 1954, Leontovich was Chair of the Department of Theoretical Physics at Moscow Physico-Engineering Institute

In 1951 in Leontovich life, a new period started. Leontovich became the chief of theoretical laboratory at the Institute of Nuclear Energy (now Russian Research Center–Kurchatov Institute). He took charge of theoretical research on the controlled thermonuclear fusion.

Leontovich died in 1981.

Book "Scientific community of physicists in the USSR (Issue 1, 2005, pp. 227–229)" contains the following summary of Leontovich's contribution to physics:

"Leontovich's areas of interest were physical optics, electrodynamics, the theory of oscillations, acoustics, radio physics, physics of plasma, and thermonuclear controlled synthesis. In 1929 together with Mandelstam Leontovich explained the tunnel effect, they constructed the theory of direct tunneling in quantum mechanics.

In 1929, he participated in formulating the theory of the combinational scattering of light in crystals.

In his 1931–35 writings, he elaborated the statistical-mechanical approach approach to the calculation of the response of the many body system to the outside actions. In 1937 together with Mandelstam put forward the general method of the analysis of dissipations in the systems with the finite relaxation time.

M. A. Leontovich's approximate boundary conditions for the electromagnetic field on the surface of well-conducting bodies are well known in electrodynamics.

Together with S.M. Rytov Leontovich formulated the interrelation of the correlation of current fluctuations in the medium and its conductivity.

In 1946, Leontovich together with V.A. Fock conducted research concerning the propagation of radio waves along the Earth's surface. Leontovich contributed to the theory of thin wire antennas.

In 1951, Leontovich took charge of the project concerning theoretical physics of a plasma and the controlled thermonuclear fusion.

In 1958, Leontovich together with L.A. Artsimovich, who run experimental part of the project, received Lenin prize. The Lenin Prize committee pointed out the results concerning the production of high-temperature plasmas for the controlled thermonuclear fussion synthesis with the help of impulsing discharge in gases. Together with Artsimovich and Leontovich, eleven researchers received this prize.

13.2 The Formal and Physical Concepts of Probability

The following three sections are dedicated to Leontovich's contribution to statistical physics. We shall basically refer to his book on statistical physics, a book which can be taken as a textbook [209, 211]. In 1936–37, Leontovich delivered the lectures on statistical physics at Moscow State University (there is a record of these lectures in Russian State Library [208]).

Statistical physics makes good use of the theory of probability, its ideas, methods, and mathematical techniques are applied in statistical physics.

Leontovich's discussion runs as follows.

"The modern theory of probability, like every mathematical theory, proceeds from a number of definitions and axioms. Based on them, it is possible to calculate the probability of some event by basing on the probability of other events. An event is a set of values of a number of variables—"contingent magnitudes".

Probability of an event is called numbers having properties which we shall list here by restricting ourselves to the case when the random variable has only a finite number of values.

Let the variable x take n values $x_1, x_2, \ldots, x_n$. Let $W(x)$ is a function of x that $\sum W(x_i) = 1$. $W(x)$ is designated as the probability of the value of x. Let χ be a set of values $x_{q1}, x_{q2}, \ldots x_{qs}$. The following magnitude is called probability of this set.

$$W(\chi) = \sum_{k=1}^{s} W(x_{qk}) \ldots$$

The complex of these statements (and their generalizations for the contingent magnitudes taking an infinity of numbers of discrete or continuous values) and all the theorems which can be deduced from this base we shall call the "formal theory of probability". In order to apply this theory to the problems of physics, we have to make an important step, we need to provide an interpretation of the concept of probability. The thing is that in all the applications probability of an event is identified with the relative frequency that this event happens under certain circumstances" [209, p. 23–24].

In the formal theory of probability, the specific meaning of the concept of probability remains arbitrary. Probability is not connected with any frequencies of any events. Moreover, probability can be treated beyond any field of physically realizable systems.

To solve the problem, one can follow one of two paths. It is possible for any problem to define the meaning of the number of concepts: probability, conditional probability, statistical independence.

However, a more productive way has been analyzed by Richard von Mises. According to Richard von Mises within the framework of the mathematical theory, the concept of probability is identified with the frequency of the event in the sequence of such events. Although this approach meets with mathematical difficulties, these difficulties may be resolved. For Richard von Mises, the concept "collective" is fundamental. Collective is an infinite sequence of values of the variable, a sequence which has the following two features.

1. Among n initial elements of the sequence $n(x)$ which correspond to values of the of variable x, let the following limit exist

$$W(x) = \lim_{n\to\infty} \frac{n(x)}{x},$$

2. For any subsequence, n^1 elements belonging to the sequence n, the limit exists

$$W^i(x) = \lim_{n\to\infty} \frac{n^i(x)}{n^i}$$

and

$$\frac{W^i(x_1)}{W^i(x_2)} = \frac{W(x_1)}{W(x_2)}$$

This second feature can be called by the "arbitrariness of selection". Thus, probability characterizes some "collective" and every operation performed on a probability corresponds the construction of a new collective. For example, when passing from one collective to another whose elements are aggregates of the elements of the initial one, we obtain a collective for which probabilities are equal to the sum of the initial probabilities. If we have a collective of values of a couple of variables (x, y), and if we fix a sequence of those values, for which x has given values, then it is not difficult to demonstrate that this new sequence is a collective, too, and for this collective probability is equal to the conditional probability $W_x(y)$.

Leontovich turned to the "formal concept of probability" when he discussed the general problems of statistical mechanics, for example, when he discusses the concept of probability that the system is located in some area of the phase space. He characterized this probability as a relative time of being of the system in this area [209, p. 40].

$$W(\Xi) = \lim_{T\to\infty} \frac{T_\Xi}{T}$$

However, by describing some issues, Leontovich emphasizes that probability is taken in Richard von Mises' sense. "If a sequence of the states of the system is taken as the Markovian chain, then the precise specification of the initial state determines the probability of the state of the system for subsequent instances of time (this specification does not determine the state itself like this is assumed in mechanics).

Here we take "probability" in its "physical sense", we believe that a set of appearance of states of the system at the moment t forms the "Kollektive" and the probability $w\mathrm{d}x$ is a relative number of appearances of the states x, $x + \mathrm{d}x$ in the collective" [209, p. 226].

In the statistical theory of the processes, for example, in the theory of Brownian motion, the concept of the probability of a transition appears and the concept of statistical independence is used. These concepts can be physically interpreted if the concept of probability is connected with some sequence of events, with some collective.

It interesting that Leontovich's twofold approach to probability is kept in a modern textbook. If Gnedenko [134, 135] strictly followed Kolmogorov's definition of probability, Ventzel and Ovcharov [353] treated the frequency of an event as an approximate value of probability which is conceptualized along Kolmogorov's axiomatic system. True, it should be noted that Leontovich took the frequency definition of probability as real definition in physics, and used the two definitions interchangeably.

13.3 The Theory of Fluctuations

M.A. Leontovich published a number of papers on the theory of fluctuations [205–207, 210, 214]. In his 1944 textbook, he summarized his ideas and systematically presented them. Leontovich dedicated a chapter to the problem of fluctuations.

He wrote at the beginning of this chapter:

"Till now we discussed the average values characterizing the system in the state of thermodynamic equilibrium. However always in an every system the deviations of magnitudes from this state take place, deviations called fluctuations. They lead to a number of phenomena studied by experimental methods. Local deviations of density in gases, liquids and rigid bodies result in the scattering of light in transparent bodies, the so called molecular scattering of light. At temperatures close to criticality we observe an especially profound effect. This is the so called critical opalescence, the phenomenon which had remained unrecognized for a long time, since it seemed contrary to classical thermodynamics as it was formally presented. An explanation of the phenomena of the fluctuations can be reached only within the framework of statistical physics. From the point of view of statistical physics these phenomena should take place in any system" [209, p. 91].

Leontovich discussed the limit of sensibility of a number of typical apparatuses, the limit resulting from the fluctuations.

It is natural to put a the question as to what is governing the value of the fluctuations of any parameter. For which parameters the fluctuations are considerable, for which they are small and how they depend from the parameters of the system? Is it possible to consider the value of the fluctuations by ignoring the details of the system construction and by proceeding from its phenomenological characteristics?

"Smoluchowski and Einstein provided an answer, Leontovich writes in his book. They drew on the Boltzmann principle which relates the relations between probabilities of any couple of isothermal systems to the difference of their free energies. If we take a closed system (with respect to energy), Boltzmann principle relates the ratio of probabilities of a couple of states to their entropy difference" [209, p. 111].

Usually, the Boltzmann principle is formulated as follows:

$$S = -k \ln q$$

This means that entropy of a system in any of its state is proportional to the logarithm of the probability of the state. This formula is present in many textbooks on thermodynamics and statistical physics. However, its status is rather controversial. To begin with, there are several names of this formula: The Boltzmann principle, Boltzmann theorem, Boltzmann formulae (the last terminology is present, e.g., in the textbook written by Leontovich's collaborator D.V. Sivukhin [317]). In addition, several logical problems arose. "This statement, which is absolutely meaningless in the case of an isolated system, obtains, as we see, some meaning for a system in a larger system. This can be accomplished, however, only by using … the generalization of the notion of entropy which is introduced "ad hoc". In fact, one must not forget that this notion is used in connection with the second law of thermodynamics which loses meaning when the generalized definition of entropy is used. All existing attempts to give a general proof of this postulate must be considered as an aggregate of logical and mathematical errors superimposed on a general confusion in the definition of the basic quantities" [79, p. 105].

However, Leontovich formulated the Boltzmann principle by demonstrating the solution of the specific problems. Leontovich wrote the following:

"By treating the problem on the fluctuation of the volume of a gas thermometer we came to the formula which expresses the Boltzmann principle for this special case. It shows that the probability of the fluctuation of the volume V can be formulated as the following

$$W(V)\mathrm{d}V = \text{const}\,\exp\left[-\frac{\varphi(V) - \phi}{\theta}\right],$$

where ϕ is the free energy at the specified external pressure, $\varphi(V)$ is the value of this free energy at the same pressure P but at the volume which differs from its equilibrium value, $\theta = kT (T$ is absolute temperature). The probability relation for these two states is

$$\frac{w(V_1)}{w(V_2)} = \exp\left\{-\frac{\varphi(V_1) - \varphi(V_2)}{\theta}\right\}$$

In the general case of "the system in a thermostat", the Boltzmann principle can be analogously formulated. Let we have two states, 1 and 2 (generally speaking, they can be non-equilibrium states), that are characterized by the definite values of the internal parameters $\xi_1, \xi_2, \ldots, \xi n$. For the former state, they have values ξ_i^1, for the latter—ξ_i^2.

Let

$$\omega(\xi_1, \xi_2, \ldots, \xi_N)\mathrm{d}\xi_1\mathrm{d}\xi_2..\mathrm{d}\xi_n$$

be the probability of the state $(\xi_i, \xi_i + \mathrm{d}\xi_i)$,

$$\frac{\omega_1}{\omega_2} = e^{-\frac{\varphi_1 - \varphi_2}{\vartheta}},$$

where Θ is kT, T is temperature of the thermostat, φ_1, φ_2 are the free energies of these two states" (op.cit, p.112).

Leontovich dedicated the final section of his chapter about the fluctuations to the deduction of the latter formulae from the general principles of statistical thermodynamics. The key point was the concept of free energy for the non-equilibrium state that he introduced. This was a rather complicated concept. Starting with the formulae

$$F_1 = \Psi_1 \text{—} U,$$

where F_1 is the free energy in the state 1, U is a potential energy of the force field, for which the state 1 is the equilibrium state, Ψ_1 is the free energy of this equilibrium state corresponding the modified external conditions—the occurrence of the force field. Leontovich conducted a number of calculations and discussions which allow us to apply this definition in his theory of fluctuations.

The Soviet (after 1991—Russian) physicist Yu.L. Klimontovich (who was mentioned in Chap. 4 in connection with Mandelstam-Planck polemics) dedicated several papers to Leontovich's creative work [177, 178, 377]. Basically, he described Leontovich's writings on fluctuations.

Moreover, in his 1982 monograph "Statistical physics", he reproduced Leontovich's theory on fluctuations. True, Klimontovich's terminology was different: He formulated the concepts "conditional entropy" and "conditional free energy" [178]. Moreover, on the whole his approach is another. Klimontovich proceeded from non-equilibrium thermodynamics which was developed in the second half of XX.

Klimontovich emphasized that Leontovich had come to realize that the classical approach formulated by Boltzmann could not form the basis for the calculation of the fluctuations. He referred to Leontovich's 1935 paper.

Leontovich wrote the following: "Kinematics treats of processes in gases. This is a statistical theory since the statistical principle (Stoßzahlansatz) forms the basis of the Boltzmann equation. However, the structure of this theory is very far from optimum. According to this theory the magnitude $f\mathrm{d}w\mathrm{d}\rho(\mathrm{d}w = \mathrm{d}vx\mathrm{d}vy\mathrm{d}vz, \mathrm{d}\rho = \mathrm{d}x\mathrm{d}y\mathrm{d}z)$ has the meaning of statistical average (mathematical expectation) with respect to the number of particles in the volume $\mathrm{d}w\mathrm{d}\rho$ of the phase space. However within the framework of the theory the meaning of this mathematical expectation is not clear as the theory does not consider the probabilities by means of which the mathematical expectations have been generated. The theory is not able to give any information about fluctuations in a gas and their temporal evolution" [207, p. 211].

13.4 Brownian Motion

To develop the theory of stochastic processes in physics, Leontovich takes Brownian motion as paradigm in his 1944 book. Leontovich started to elaborate the theory of Brownian motion in his paper written in coauthorship with A.N. Kolmogorov [214]. They solved the problem of how to calculate the area covered by a Brownian particle over a period of time. Kolmogorov put this problem within the context of the theory of probability (the formal theory in the terminology of Leontovich's 1934 book). In turn, Leontovich solved this problem as a problem of mathematical physics.

By treating Brownian motion in his 1944 book, Leontovich wrote that he proceeded from ideas produced by Einstein and Smoluchowski. However, he advanced their ideas and he concentrates on the temporal development of processes. He returned to the problem of fluctuations (the fluctuations of current in a wire, the chatter of the mirror in a galvanometer, etc.).

Leontovich started by considering a model. "Let us consider Brownian motion in one dimension. We shall find the average shift of a particle in a given time (and the average shift squared). We need to have in mind that here the average is not the average over time, this is the average over a set of particles" [209, p. 219].

By elaborating this model, Leontovich came to the Einstein formulae which connect the diffusion coefficient with the coefficient of friction. Leontovich also came to the formulae for the average square of the distance of a particle shift in a time t.

However, Leontovich wanted to have a general theory. To solve this problem, Leontovich turned to the theory of Markovian processes. Below his definition of these processes has been reproduced. Let any system can take one of the n states $1, 2, 3, \ldots, n \ldots$ Let one observe the state of the system at regular intervals of time, say at every second, at the points $t = 1, 2, 3 \ldots$. In the course of time, the system can go from one state to another. A sequence of states which the system can take in the course of time is a Markovian chain, if the probability of what the system is in the kth state is completely determined by the specification of the state in one of the preceding moments $t = 0$.

Leontovich shows how to extend the concept of the Markovian process over the processes where t is continuous. He writes that the Markovian chain is not a universal scheme of how to solve problems in statistical physics. However, in statistical physics, many processes can be represented as Markovian chains.

In the preceding section, we mentioned the Soviet physicist Yu. Klimontovich who wrote a number of papers on Leontovich's contribution to statistical physics. Klimontovich treats Leontovich's results in statistical physics as very promising but he is sorry that they had not received any considerable development in the second half of XX.

13.5 Physics of Plasmas

As was noted, at the beginning of the 1950s Leontovich became the manager of theoretical part of the nuclear research conducted in the Institute of Atomic Energy. The results of his work are mainly represented in five-volume book “The problems of the theory of plasmas” [375] (1963–1967) edited by Leontovich. All five volumes have been translated into English and published in the USA.

Leontovich wrote in Introduction the following [375, vol. 1, p. 3–5]:

“The present book opens publication of a number of the collected papers dedicated to different aspects of the theory of plasmas. To be sure, the reader would prefer to have a book containing a complete presentation of this theory. The authors made an attempt to write the monography on the theory of plasmas. However, in the process of work we found out that this problem could not be solved now as we have not a complete theory of the behavior of a real plasma.

However, ten years ago it was almost obvious that from the point of view of dynamics a plasma differs from a conventional gas only a little and therefore the theory of transference (electrical conductivity, transference of heat, diffusion) could be constructed by analogy with the theory of gases. Such a theory has been constructed by many physicists and now it is usually called classical.

Regrettably, as experimental research demonstrated, the behavior of plasma is not consistent with what the “classical theory” states. There is a wide diversity of instabilities along with oscillations influences the averaged parameters of plasma. The complete theory of plasmas needs to be a theory of nonlinear (and turbulent) motion. This theory is still in a primitive stage of development. Nevertheless, we can outline what should be essential for the future complete theory.

If we take a gaseous and completely ionized plasma and concentrate ourselves on its dynamics (we don’t take the elementary processes, radiation into account), than the theory of plasmas can be constructed on the classical base—on the Maxwell equations for fields and the Newton equations for charged particles. In doing so, we should apply a statistical description.

By integrating the Liouville equation over all but one particle, over all but two particles, etc. we come to the chain of Bogoliubov equations which can be solved by series expansion (the small parameter equals to the inverse number of particles in the Debye sphere). This leads us to the kinetic equation with self-congruent fields and with the collision term in Landau’s form.

Here we came to the problem of the collective processes in plasmas. The thing is that even in moderately non-equilibrium plasmas the collision term in its Landau form has a logarithmic precision only… By considering non-equilibrium plasmas we should take the heat fluctuations of the electric field into consideration. These fluctuations could considerably influence the processes of transfer.

In highly non-equilibrium plasmas we come to more complicated problems. In such a plasma the amplitude of noise can reach so considerable values that the interaction between harmonics manifests itself and we have turbulent plasmas. In a highly non-equilibrium plasma the paired interactions are of secondary importance and the collective effect of noise development is responsible for the variation of the averaged quantities.

In contrast to an usual gas which has only one characteristic time (the time between the consecutive collisions), a plasma has a number of characteristic times. In an equilibrium plasma this is the periods of a variety of oscillations. In highly non-equilibrium plasma we have the characteristic times of the development of oscillations resulted from instability and the energy exchange between oscillations…".

As was noted, five volumes of "The problems of the theory of a plasmas" had been published in 1963–1967 with M.A. Leontovich acting as editor. Although Leontovich wrote a few papers dedicated to the plasma physics [212, 215], he did not participated in these volumes as an author.

These facts show that M.A. Leontovich's participations in the nuclear project were mainly organizational and ideological.

One of the young physicists who worked under Leontovich (young in the 1960s–1970s) writes in his recollections: "We young physicists were surprised by the degree of accuracy with which Mikhail Alexandrovich led research of the researchers at his laboratory. After the regular phrase "What are you doing? Let us have a look", he sat down nearby the researcher and reproduced step by step all the calculations which the researcher showed him and conducted his own ones. If he came to the positive result, he said "Continue on this way", yet if the result was negative, he pointed to the key problem and said "some frog is sitting here". He thought over that problem at home and on the other day he came with the solution or he pointed to the method of how to obtain a solution" [377, p. 370].

13.6 Political Activity

In the recollections of Leontovich, his political activity is usually mentioned. It should be noted that the authors mean the political activity in the Soviet sense of the word. It does not mean that the political activity implied an opposition to the State, to the leadership of the Communist Party. It was not a political activity of the opposition which arose in the 1970s, Leontovich was not politically oriented like Aleks. Solzhenitsyn and Andrei Sakharov were. Moreover, as the recollections show, Leontovich belonged to Russian intellectuals who sympathized with the idea of socialism.

After Hessen's arrest and his execution, Leontovich's political activity consisted in keeping silence by attending the political meetings at Moscow State University and at Institute of Physics belonging to the Academy of Science (FIAN). It was difficult, since the speakers who unmasked Hessen as the "enemy of the people" touched with the organizational problems. A.V. Andreev in his book on the history of the Research Institute of Physics describes the discussion which took place during the meeting of the Scientific Counsel of Physics School of Moscow State University (1937). M. Leontovich said a few word concerning the financial backing for research conducted by Predvoditelev. His comment has been treated as political, anti-soviet by some of participants [12, pp. 85–86].

It should be noted that the politics of keeping silence was not typical for the members of the Academy of Science. As was noted, Mandelstam's former student Divilkovskii took the floor at the meeting dedicated to B. Hessen as the "enemy of people" and spoke about the loss of watchfulness by some of FIAN's scientists.

"We demand merciless reprisals against the vile traitors of our great homeland". This letter (1937) was signed by Academicians A. Bach, V. Komarov, B. Lavrentiev, N. Vavilov, G. Krshizhanovskii and a number of the other authoritative figures [30, p. 1].

After World War II, Leontovich participated in the collective letters to the authorities, the letters expressing warning, disagreement with some plans, situations which the authorities supported. So, Leontovich was one of the authoritative figures.

In 1956, Leontovich signed the "Letter of three hundred" sent to the Presidium of the Central Committee of the Communist Party of the Soviet Union. In other words, this letter which disavowed Academician Lysenko' s contribution to biology and agricultural science and Marxist line in genetics proclaimed by T.D. Lysenko was sent to the supreme leadership of the Soviet Union. It should be noted that in 1956 this leadership was headed by N.S. Khrushev who supported Lysenko on the whole.

In 1966, M.A. Leontovich put his sign under the "Letter of twenty five" which objected the complete or even partial rehabilitation of Stalin, rehabilitation which seemed to be under preparation. This letter was sent to the General Secretary of the Communist Party of the USSR L.I. Brezhnev. Together with Leontovich, this letter was signed by such great representatives of the Soviet culture: Paustovskii (a writer), Tovstonogov (a producer), Kataev (a writer)…

All in all, five physicists (and generally speaking scientists) put their signatures under this letter: Artsimovich, Kapitsa, Leontovich, Sakharov, and Tamm.

According to the recollections, Leontovich put his signature under the letters in support of some of Stalin's prisoners and dissidents (see: [376, pp. 258, 306, 307, 374]).

When the Soviet famous writer Boris Pasternak was forced by the Soviet authorities to decline his Nobel prize, Leontovich visited him in Peredelkino (the cottage village nearby Moscow) and said words of encouragement to him (1958).

Chapter 14
Mandelstam's Operationalism

14.1 In Which Way the Term "Operationalism" Was Applied to Mandelstam's World View

In this chapter, we discuss Mandelstam's philosophical position, which, according to the present author, may be characterized as operationalism.

In the world literature, the position of the American physicist, Nobel Prize winner for his work in high-pressure physics, Bridgman Percy (1882–1961) is called operationalism. In 1927, Bridgman published a book "The Logic of Modern Physics", which contains the thesis, which became the banner of operationalism: "the concept is synonymous with a corresponding set of operations". For example, "the concept of length is fixed when the operations by which length is measured is fixed, that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined" [67, p. 5].

Percy Bridgman stressed that his operationalism is genetically associated with Einstein's methodology of the theory of relativity. For Einstein, the spatial and temporal coordinates are what is measured by rulers and clocks, and the simultaneity loses its absolute status and requires clock synchronization.

The term "operationalism" was applied to L.I. Mandelstam's philosophy of science by the famous mathematician Anatolii Danilovich Aleksandrov in January 1952 (Aleksandrov became Academician in 1964, in 1952, he was Corresponding Member of the USSR Academy of sciences). He delivered a lecture at the "United Institute Colloquium" of the Physics Institute of the Academy of Sciences (FIAN). This lecture was entitled "On ideological mistakes in some textbooks on physics". Aleksandrov pointed to the source of the mistakes. This was the fifth volume of L.I. Mandelstam's *Complete Works*, containing his lecture on the theory of relativity and quantum mechanics. Aleksandrov said: "This is a prescriptional view of the definition of scientific concepts, view which was developed by idealists Percy Bridgman and Philip Frank. This trend to reduce concepts to operations is a trend of subjective idealism, which tends to eliminate the objective reality and to reduce everything to immediate data" (cited in [324, p. 181]).

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Aleksandrov's lecture was criticized by I.E. Tamm, S.E. Chaikin, and some other colleagues of the late L.I. Mandelstam. They gave the quotations as evidence in favor of Mandelstam's materialism. However, they agreed that Mandelstam's philosophy of science was not consistent and he was not a proponent of dialectical materialism.[1]

Now that the ideological battles of the early 1950s are history and not the closest history, it is hardly worth trying "to restore justice". It is hardly necessary to prove specifically that the terms "operationalism" and "idealism" are not more offensive than the "social democrat" and "anti-globalists". It is clear that Aleksandrov spoke from a position of orthodoxy, which should deal with all kinds of heresy. It is also clear that those who opposed him tried to be closer to orthodoxy. However, we have a claim that L.I. Mandelstam was operationalist and idealist. Aleksandrov, having philosophical parallels, came to the conclusion to which it was not hard to come: Mandelstam, like many physicists of his time, was in the orbit of ideas, dating back to Einstein's first article on the theory of relativity (1905), to Heisenberg's articles on quantum mechanics and eventually to the works of Ernst Mach. Aleksandrov described Mandelstam's position as operationalism, bearing in mind its proximity to the concept of P. Bridgman. But Mandelstam did not refer to Bridgman. Calling the philosophical credo of Mandelstam operationalism, we are planning to analyze the relationship of this credo, formulated authentically, to the paradigmatic operationalism of Bridgman.

14.2 How Did Mandelstam Formulate His Operationalism?

Mandelstam's operationalism is scattered among the notes of his lectures and seminars, which constitute the fourth and fifth volumes of his *Complete Works*. The most comprehensive and clearest of the formulations can be found in his 1933–1934 *Lectures on the Theory of Relativity* and 1939 *Lectures on Quantum Mechanics*. Mandelstam formulated the principles of his operationalism in his discussion of the nature of physical concepts and physical theories.

In *Lectures on the Theory of Relativity*, where he tended to follow directly Albert Einstein [1, Vol. 4, pp. 90–305, 3, pp. 83–285], Mandelstam spoke mainly about the nature of concepts. He clearly was aware of the philosophical character of the problem which he posed, although he had never used the word "philosophy". Having outlined the problem of how to reconcile the principle of relativity which had been formed in classical physics and the principle of the constant velocity of light, the

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[1] Sonin's book [324] is cited here. Sonin also wrote a big book concerned with the accusations of some Soviet scientists of "cosmopolitism" in the Cold War years [325]. In the latter, there is no excursion to the Aleksandrov 1952 lecture. As a matter of fact, A.D. Aleksandrov's attack on Mandelstam's operationalism requires a more detailed analysis. It is clear that A.D. Aleksandrov, who was a famous topologist writing on the philosophy of space, could not accept operationalism. However, it is not clear why he preferred such a form of his attack (Aleksandrov's lecture dedicated to Mandelstam's "Complete Work").

problem which, according to him, was Einstein's point of departure, Mandelstam stated [1, Vol. 4, pp. 177–178]:

> In order to do this, another discussion should be launched, the discussion of the structure of physical concepts. I can not speak about it in details, for (1) I am not a specialist in this field and do not know these matters enough and (2) these matters would take us far away from our problems. However, some important peculiarities, without which a physicist cannot work, we shall see. We shall see that we speak a lot of words which have no content, and confusions result from this. Let us look at some simple facts.
>
> When we are speaking about some scientific laws, for example, Newton's laws, we mean formulas containing x, y, z. To test these formulas we need to substitute certain numbers for x, y, z. However to do so, we must be able to measure length.
>
> What does it mean to measure length for a physicist? At first he must have a unit of length. What is the unit? This is a distance between two marks on the rod which is kept in Paris…
>
> This is not all. Once a physicist has a unit he also needs a technique for measuring. He needs a real process that gives him a number which is, by definition, the length of the object. A physicist must have a prescription for how to measure length.

Mandelstam emphasized Einstein's philosophical contribution to physics. He said [1, Vol. 4, pp. 180, 196]:

> A number of concepts is not experienced but accepted by definition in the course of cognition of the real world. Einstein shows that it is the point that has been overlooked and this is his great contribution to science. Einstein performed his great service when he elucidated that the concept of simultaneity is a concept like the concepts of length and the time of an event.

Discussing the physical concepts in his *Lectures on the Quantum Mechanics* [1, Vol. 4, pp. 347–415], Mandelstam seemed to follow Heisenberg's celebrated 1927 paper where the uncertainty relations had been formulated (he had never referred explicitly to this article). In turn, with respect to his methodological part, Heisenberg apparently followed Einstein's 1905 article.

(The real history was more complicated. As is well known, in the 1960s, Heisenberg reminiscently recalled how he spoke with Einstein in 1926, who told him the famous phrase that "it is the theory that decides what we can observe". However, these recollections do not contradict the observation that both Einstein's 1905 article in its kinematical part and Heisenberg's 1927 uncertainty article were written in the same operationalist tenor. It is not surprising that, as Heisenberg pointed out, he recalled his conversation with Einstein just before writing his uncertainty paper. "Operational definitions of fundamental concepts subject to quantum mechanics and the uncertainty relations quickly followed. The theory did indeed decide what could or could not be observed and remembered" [74, p. 239]).

Mandelstam said [1, Vol. 4, p. 354]:

> Quantum mechanics rightly abandons prejudice that laws of macro-world are valid in micro-world. But only the mathematical part of the theory completely proceeds from this point of view. In the text-books it does not take sufficiently into account that prescriptions for the transaction [from the math- ematical technique to the real objects] must differ from those in classics.
>
> If in classics I state that x is the position of a material point than I mean a clear prescription: if I set properly a rigid rod graduated according to a definite prescription, then x numbers that marking with which the point coincides.

> As far as we speak about the molecular issues this prescription is not performable... Thus having called x the position I only pretend that I establish the relation of it to the nature. With such a definition theory is in the air.

Mandelstam continued [1, Vol. 4, p. 358]:

> The uncertainty relations trouble us, since calling x and p position and momentum respectively we are thinking about the corresponding classical magnitudes... Why do we called p momentum? This is self-delusion again... Until we have no new measuring prescriptions it would be better not to use old terms.

He explained the uncertainty relations as follows:

> The very definition of quantities, with which the theory works, presupposes the theoretical impossibility of simultaneous exact values of x and p. The situation is the same as in classics. The question "What is the oscillation frequency of a pendulum at a particular instant of time?" is absurd. So, the thing is in the very definition of the concept.

Mandelstam gave the usual operational (for Mandelstam, "prescriptional") definitions of the position and the momentum of a particle: the position of the dot on a photographic plate resulting from the incidence of a particle on the plate and the curvature of the track of a particle in a cloud chamber, respectively. However, he pointed out the inadequacy of such an approach: The momentum of an uncharged particle (say, neutron) cannot be measured by measuring the curvature of the track of the particle in a cloud chamber. Mandelstam stressed that the direct measurements are exceptional and outlined the theory of indirect measurement, which was not articulated by Heisenberg and the other founders of quantum mechanics. This was an important move.

I.E. Tamm commented on this move in his essay of Mandelstam's biography as follows [333] ([8, pp. 135–136] is cited; for an English translation, see [340, p. 275]):

> As far as I know, Leonid Isaakovich was the first to include in lectures the very important distinction between direct and indirect measurements in quantum systems. The last stage in any measurement of a quantum system necessarily has a macroscopic character. L.I. calls measurement direct when the first measurement step is macroscopic. Example: An electron incident on a photographic film leaves a blackened spot. The macroscopic coordinate of the spot, by definition, is the coordinate of the electron upon its impact on the film. It is important to note that the direct measurements are possible only for free or nearly free particles in free fields. For example, it is impossible to determine the coordinate of an electron in a hydrogen atom by placing a photographic film inside the atom.
>
> In addition to direct measurements, indirect ones are also possible. In these we force the quantum system on which we want to make measurements to interact with another microsystem on which direct measurement are possible. The date of these direct measurements we use for theoretical calculations of the values of the quantities relevant to the first system. Example: By measuring the angular distribution of electrons scattered by an atom, we can find the distribution of bound electrons in this atom.

Thus, Mandelstam extended the concept of operational definition by including "indirect operational definition" suggesting theoretical calculations. With this extended operationalism, he examined the foundations of quantum mechanics.

It is more difficult to trace Mandelstam's outline of the structure of a physical theory to its philosophical sources. To some extent, this outline was close to that which had been given by some ideologists of modern physics at the turn of the century (H. Poincaré, P. Duhem, K. Pearson, et al.). In short, it could be expressed by the scheme: mathematics + experiment. Mandelstam, however, emphasized the prescriptional character of those rules which relate the mathematical technique of a theory to nature.

It is remarkable that Mandelstam developed in essence his discussion of a physical theory along the same line as the positivistically inclined philosophers of his time (Hans Reichenbach, Rudolf Carnap, Henry Margenau) and philosophers who came out later (Karl Hempel, Ernest Nagel) kept in their discussion of this matter. One can read in Mandelstam's Lectures on Quantum Mechanics:

> Every physical theory consists of two parts that supply each other. I shall start by indicating what the second part is. This is a set of equations of a theory (Maxwell's equations, Newton's equations, Schrödinger's equation, etc.). Certain symbols are contained in these equations (x, y, z, vectors E and H, etc.). With this, the second part is completed.
>
> The first part of a physical theory consists of the connections of these symbols (quantities) with the physical objects, connections, which proceed in accordance with the specific prescriptions (we must have the real objects as standards and a real measurement technique). [1, Vol. 4, p. 349]
>
> The building of a physical theory can be divided into two stages.
>
> First of all, one should introduce physical quantities that depend on the field to which this theory refers. Among them, we assume mathematical relations (e.g., in the form of differential equations).
>
> The second stage consists of connecting the mathematical quantities with the physical objects. To achieve this, for every quantity, we must formulate a definite prescription for how to attach a numerical value to this quantity. [1, Vol. 4, p. 408]

Having reviewed the mathematical scheme of quantum mechanics (self-adjoint operators, the wave function, the Schrödinger equation), Mandelstam said [1, Vol. 4, p. 359]:

> We need to coordinate the symbols, belonging to the Schrödinger equation, with the objects of nature. For a physicist to state such a relation means to give an actual prescription according to which numerical values of physical quantities could be extracted from the real objects.

Mandelstam carefully formulated his operationalism in his discussion of the non-classical physical theories; he was explicitly following Einstein and implicitly following Heisenberg. However, in his 1930–1932 Lectures on Oscillations (fourth volume) and in 1932–1933 Lectures on Selected Issues in Optics (fifth volume), Mandelstam treated some fundamental concepts of classic physics, concepts with which he was in touch within the main portion of his research and teaching, proceeding from the operationalist point of view. One can point to the operationalist essays in his big article representing his lecture delivered at the 1931 All-Union (National) Conference on Oscillations [1, Vol. 3, pp. 52–86] (this conference was taken into consideration in Chap. 9).

It is probable that Mandelstam accepted operationalism by studding fundamental articles of Einstein and Heisenberg and after that he applied it in his analysis of classical physical theories to which he basically contributed. It is also possible that

Mandelstam came close to the operationalist point of view by solving the conceptual problems which arose in his research in classical physics and Einstein's and Heisenberg's writings only supported his operationalist inclinations and stimulated their elaboration. In any way, in his lectures and articles on classical physics, he was not an ostensible operationalist: He did not use the term "prescriptions" and did not discuss the definitions of concepts, but he emphasized that the problem of physical reality (in order to escape being a pseudo-problem) must be posed with respect to experiments and measurements which are able to fix things whose reality is under question.

In the next section, we shall consider two examples of Mandelstam's operationalist treatment of the conceptual problems of classical physics.

14.3 Operationalism in Classical Physics: Two Examples

The two examples are concerned with the reality of components resulting from the Fourier analysis of physical phenomena. Mandelstam summarized his view of problems which arose in connection with the Fourier expansion by stating the following: "Every expansion is correct and reasonable in relation to the experimental device which is in usage" [1, Vol. 4, p. 173] and "The question of the reality of the expansion into a sinus series often arises. This question reaches meaning when it is put in connection with the apparatus that receives oscillations" [1, Vol. 4, p. 119].

As G.S. Gorelik recalled, at one of the seminars, Mandelstam explained his approach to physical reality as follows. There is a collection of balls, which are big and small, ferrous, and cupric. If we are sorting the balls with a sieve, the collection consists of big and small balls. If we are sorting them by magnet, the collection consists of ferrous and cupric ball (cited [138, p. 153]).

The examples, which we plan to discuss in this section, differ from each other in the physical contexts in which the problem of the reality of the Fourier components arose. The first is borrowed from radiophysics.

In 1930, in his paper in *Nature*, the outstanding English radio-engineer Ambrose Fleming struggled with "widely defused belief in a certain theory of wireless telephonic transmission that for securing good effects it was necessary to restrict or include operations within certain width of "wave band" [113, p. 92] (about Mandelstam's criticism of Fleming see Chap. 3).

According to Fleming, "wave band" was merely a kind of mathematical fiction and does not correspond to any reality in Nature" [ibidem].

Fleming referred to the "wave band theory" which was implied by the series expansion of the modulated signal, emitted by the transmitter. "When we sign or speak to affect the microphone at a broadcasting studio", Fleming wrote, "the result is to cause the emitted vibrations, which are called the carrier waves, to fluctuate in amplitude but not to alter the number of waves sent out per sec". Suppose the broadcasting station emits a carrier wave of frequency p. If q is the acoustics frequency, then the modulated vibration can be expressed by the function:

$$a = A\cos qt \sin pt. \tag{14.1}$$

However, this function can be expanded as follows:

$$a = \frac{A}{2}[\sin(p+q)t + \cos(p-q)t] \tag{14.2}$$

In Fleming's opinion, the modulated signal (14.1) corresponds to something in Reality, whereas the "wave bands", presented in (14.2), are merely a kind of mathematical fiction. Fleming with his paper about the "wave band" theory launched a polemics in Nature of 1930.

Oliver Lodge, the outstanding English physicist, contributed to the polemics [221]. He greeted that Fleming, "in his admirably clear article", raised the question "Whether a mathematical alternative does or does not invariably correspond with some physical reality". In contrast to Fleming, he, based on physical properties of electromagnetic field, argued that the "wave bands" existed.

In reacting to the "wave band" discussion, Mandelstam did not mention O. Lodge's contribution to it. As regards Fleming's position, Mandelstam's aim was to disavow it.

According to Mandelstam, any question about reality should be put against an apparatus or instrument by means of which an object, whose reality is a question that has no sense. This is not a way to put a question. A transition from formula (14.1) to formula (14.2) is mere trigonometry. No reception apparatus can detect whether there is one modulated wave or three (Mandelstam's formulas slightly differed from Fleming's—A.P.) nonmodulated waves from three transmitters. The question about the reality of wave bands is one of the kinds: Which is actually true, $10 = 2 + 8$ or $10 = 5 + 5$? [1, Vol. 4, p. 177].

If we are interested in applying a higher selective receiver, the representation (14.1) is not helpful. This receiver gives physical reality to that component of the sum (14.2) to which it tunes. However, a regular (not very selective) receiver gives physical reality to a single modulated wave.

Mandelstam also expressed this as follows: If we use as a receiver a tuning fork, it distinguishes between the components of the sum (14.2). But a human ear hears the single modulated signal.

The second example is in touch with the more complicated problem [1, Vol. 4, pp. 66–74]. In 1932, in his "Lectures on Selected Issues in Optics", Mandelstam discussed a paradox which arose in physical applications of the Fourier integral. This paradox was physical rather than mathematical, and it was eventually connected with the same principles of the resonance theory as the above reality of the "wave bands". However, here the problem was more complicated, insofar as a continuous spectrum is involved.

Let $f(t)$ represent a wave packet, where

$$f(t) = \sin nt, \quad |t| \prec \frac{T}{2},$$
$$f(t) = 0, \quad |t| \succ \frac{T}{2}.$$

We also admit that $nT = 2\pi N$, where N is an integer. Thereby we provide continuity of the function $f(t)$ at $t = \pm T/2$.

The expansion of $f(t)$ into the Fourier integral is

$$f(t) = \frac{1}{\sqrt{2\pi}} \int_{-\infty}^{+\infty} g(u) \mathrm{e}^{iut} \mathrm{d}u.$$

Having calculated the Fourier factor, we arrive at

$$f(t) = \frac{n}{\pi} \int_{-\infty}^{+\infty} \frac{\sin(u-n)\frac{T}{2}}{u^2 - n^2} \mathrm{e}^{iuT} \mathrm{d}u \tag{14.3}$$

The last formula is a superposition of infinite sinusoids which extended from $t = -\infty$ to $t = +\infty$. The paradox is formulated as follows: Before $t = -t/2$, the function $f(t)$ is equal to zero. In which way the "infinite sum" of sinusoids each of which is not equal to zero turns out to be equal to zero?

If the sinusoids had been real, we should see light before the light has been switched on. It is natural to say that these sinusoids, in contrast to their sum, are not real. However, this answer suggests a non-operationalist (according to Mandelstam, non-physical) notion of reality.

Mandelstam proposed another solution. Taking into account the equivalence between the LHS and the RHS of (14.3), one can only conclude that the actions of the infinite sinusoids, into which $f(t)$ is expanded, are summed in such way that the result of their summation is zero. "We shall prove", Mandelstam said "that this is the case even for a resonator of which damping is so small as we like" [1, Vol. 4, p. 69].

Mandelstam proved that even a higher selective receiver sets off a continuous bond of infinite sinusoids rather than a sinusoid, with the bond sinusoids taken together adding up to zero.

Nevertheless, an infinite sinusoid reaches physical reality too. Mandelstam considered two limit cases: (1) if $nT \gg \frac{\omega_o}{\delta}$ and (2) if nT $\ll \frac{\omega_o}{\delta}$, where ω_0 is the proper (eigen) frequency of a resonator and δ is the constant of its damping.

In the first case (the damping is large), the resonator shows the spectrum which does not depend on the duration of the train represented by LHS and corresponds to the action of an infinite sinusoid to it. In the second case (the damping is very small), the resonator shows the shape of the train which is represented by $f(t)$.

Reading the notes of Mandelstam's lectures and seminars, we come to his treatments of reality of the Fourier components in connection with some other physical problems ("anomalous dispersion and the principle of relativity", "the light beats"). However, from a methodological point of view, these Mandelstam's comments do not add something essential to the above discussion.

14.4 The Main Points of the Comparison with P. Bridgman's Operationalism

Now, we are able to compare Mandelstam's operationalism with the philosophy of science of the American physicist Percy William Bridgman, a philosophy which spread very wide, became well known, attracted severe criticism, and received the name which we apply to Mandelstam's philosophical conception.

Mandelstam had much in common with Percy Bridgman. Both came to the philosophy of science as working physicists; both, in developing their philosophical views, were guided by Einstein's methodology contained in his 1905 article on special relativity, both emphasized the importance of experiment and measurement for the clarification of physical concepts, both saw in their philosophical accounts a tool for criticism rather than a "doctrine", both attacked "pseudo-problems" and struggled against "language stereotypes" (we deliberately use a term which neither Bridgman nor Mandelstam used and which hence is neutral with respect to them), and both Bridgman and Mandelstam explained their philosophical accounts by the same examples with "length", "time", and "simultaneity".

We, however, concentrate here on the difference between Bridgman's and Mandelstam's approaches. This difference can be summarized in the following four points:

1. In contrast to the "essentially American philosophy", as G. Holton worded it, of P.W. Bridgman [158, p. 132], Mandelstam emphasized the intersubjective character of his operationalism: According to him, the operations which supply scientific terms with meaning must be repeatable and reproducible. In his Lectures on the Theory of Relativity, he especially pointed out that operations should meet the request of "invariability and unambiguity" [1, Vol. 4, p. 182]. Bridgman also spoke about his operations as physical and hence reproducible for fellows and colleagues. "In principle the operations… should be uniquely specified" [65, p. 10]. However, his operationalism shows a solipsist tenor which becomes stronger in his later writings. As early as 1936, his Nature of Physical Theory demonstrated that he regarded operations as "private", as "mine and naught else" [66, p. 14] (see also [67, p. 158], compared with [68, p. 8] where Bridgman writes about "objectivity" of operations).
2. Originally Bridgman restricted his "operational analysis" to the physical operations which could be actually performed. In such a way, he secured knowledge with respect to contradictions which could penetrate into it through the "mental operations". In his later writings, he extended his operationalism by including

the "paper and pen" operations, that is, theoretical calculations [66, p. 123, 68]. Mandelstam had never formulated his operationalism in such a rigid formula as Bridgman's original conjunction "the concept is synonymous with the corresponding set of operations" and "if the concept is physical…, the operations are actual physical operations" [65, p. 5]. In his Lectures on Quantum Mechanics, he formulated his concept of operations by including theoretical calculations as an essential part of the operations: As was mentioned above, he developed the theory of indirect measurement in these Lectures. In contrast to Bridgman's vague "paper and pen operations", Mandelstam's indirect measurements obeyed the definite rules which quantum mechanics suggested.

3. Describing the structure of physics, Bridgman tended to fix a sequence of the dichotomies: "theories and factual knowledge", "mathematical equations and text", "mathematical models and physical models" [65, pp. 1, 59, 62]. This was in the style of the approach common to the philosophy of science of his years and afterward. Due to his theory of indirect measurement, Mandelstam turned out to be close to a holistic approach to a physical theory, the approach which was expressed by W.V.O. Quine and K. Hempel in the 1950s. If (at least) two operational ("prescriptional") definitions can be formulated for a physical concept, one of them can be treated as an empirical sentence testable against experiment and observations and the other as a proper definition which is a convention. In another situation, the former can replace the latter and the latter can replace the former. From the point of view of the theory of indirect measurement, all the quantum postulates are "rules" of measurement, and all of them provide the "prescriptions" for how to conduct indirect measurement. This means that this physical theory consists in operational definitions. However, these definitions can be testable in turn: One of them is considered to be an empirical sentence; the others provide a test as definitions, that is, conventions.
   Mandelstam did not use terms an "empirical sentence" and a "convention". However, what he said can be treated with the aid of these terms and his position can be labeled as holistic: The theory as a whole, rather than its individual sentences, is testable against observations and experiment.
4. Bridgman did not devote much room to "physical reality" and "truth" in his writings. He was in touch with them only critically: his aim was the "operational criticism" of these philosophical concepts and the limitation of their applicability. Mandelstam was not concerned with these concepts either. However, his position can be called realistic. He permanently mentioned that "operations", "prescriptions" related the mathematical symbols to "nature", to the "real objects", to the "reality" rather than "experience", "observations", "sense data". Apart from this when he wrote and spoke about the confirmation of theories, he applied the qualifications "true" and "false" to theories.

True, his realism was of a kind of scientific realism: It was restricted by his holistic approach to a physical theory. Mandelstam rejected the "a priori concepts", which were given "by themselves" (see [1, Vol. 5, pp. 183, 406]). Through his lectures, the picture of nature was open to discussion. Nevertheless, he retained some ontological parameters. Sometimes, Mandelstam, for example, was inclined to accept "ontological determinism".

Let us turn again to Mandelstam's Lectures on Quantum Mechanics. Mandelstam joined von Neumann's discussion of the completeness of quantum theory. This does not mean that Mandelstam shared von Neumann's philosophy of causality. He tended to avoid the indeterminism which von Neumann proclaimed. Thus, von Neumann wrote that "es gibt gegenwaertig keinen Anlass und keine Entschuldigung dafuer, von der Kausalitaet in der Natur zu reden" [373, p. 167]. Mandelstam said, however, the following [1, Vol. 4, pp. 403, 414]:

> They say that von Neumann demonstrated that the construction of the theory on the base of determinism is impossible. I think that such phrases say next to nothing.
>
> If they sometimes say that von Neumann demonstrated that the causal theory of the atom phenomena is impossible then this is not the case.
>
> This was not identical with von Neumann's claim above.

To conclude, let us turn once more to Bridgman's operationalism. Although Mandelstam's philosophy of science can be summed up under heading "operationalism" indicating Bridgman's teaching, it does not lack originality. With this, we arrive at the question by which the next section is entitled.

14.5 What Philosophical Tradition Was Lying Behind Mandelstam's Operationalism?

Was Mandelstam influenced by Bridgman's celebrated *The Logic of Modern Physics*? Materials at our disposal give no hint to answer "yes, he was" to this question. It is likely that Mandelstam arrived at his operationalism independently by studying Einstein and Heisenberg's articles and meditating on the foundations of physics.

There is a reason to answer in such a way to the question about Bridgman's influence on Mandelstam. As mentioned above, Mandelstam met Richard von Mises in his Strasbourg years and went on to communicate with him when they were stranded on opposite sides of the border in later years.

According to Papalexy's recollections, Mandelstam and Richard von Mises discussed the philosophical foundations of physics.

Mandelstam and Richard von Mises exchanged letters in the 1920s and 1930s (Chaps. 5 and 6). Mandelstam visited Richard von Mises when he was in Germany in 1923 and he stayed at the Richard von Mises' when he was visiting Germany in 1930. It is very probable that they continued their discussion on the philosophy of physics when they met together.

Mandelstam and his disciples enthusiastically greeted Richard von Mises' book *Wahrscheinlichkeit, Statistik, und Wahrheit* (J. Springer, 1928) in which (in essence) the operational definition of probability was proposed and a Machist ideology of this definition was developed. (This book was a popular presentation of his ideas which were first published in 1919.) As was noted in Chaps. 2 and 6, Mandelstam contributed to the rapid publication of its Russian translation, and he and his disciples inspired the discussion of it at seminars and in journals in the USSR.

Richard von Mises was among the classics of neopositivism and a great propagandist of Machism. However, it is remarkable that although the German positivists made a note of *The Logic of Modern Physics*, Richard von Mises did not mention Bridgman's operationalism in his 1939 *Kleines Lehrbuch des Positivismus* [370] and in previous philosophical writings. He mentioned it only in his 1951 English version of it entitled *Positivism*. "The physicist P.W. Bridgman", Richard von Mises wrote, "devised in his operationalism a theory of knowledge that is closely related to, and in full agreement with, the main teachings of Mach" [371, p. 361].

As was mentioned above, Richard von Mises in essence developed the operational definition of probability in his book *Wahrscheinlichkeit, Statistik, und Wahrheit*. It should be noted that he did not use the term "operational definition" then and, in contrast to Bridgmans', his definition referred to theoretical principles and included theoretical considerations.

Attempting to answer the question about the generic relation of Mandelstam to Bridgman, we are in the same difficult position in which Max Jammer seemed to be when he discussed Heisenberg's operationalism as it was expressed in his 1927 article where the uncertainty relations had been formulated. On the one hand, Heisenberg's article shows apparent operationalism. On the other hand, "it would be rash to classify Heisenberg as a pure operationalist" [166, p. 58]. To justify this, he referred to the facts that P. Bridgman did not approve of Heisenberg's interpretation of the uncertainty relations and Heisenberg, in turn, did not accept Bridgman's 1929 explanation of these relations [166, p. 472].

Thus, like in Mandelstam's case, there is no indication that Heisenberg's operationalist interpretation of his uncertainty relations and Bridgman's operationalism are genetically connected. Heisenberg developed in his 1927 paper his own version of operationalism which can be traced back to the kinematical part of Einstein's 1905 article and to E. Mach's positivism. Indeed, although the main portion of Mach's book and papers is written in a descriptivist tenor, his teaching admits interpretation in operationalist way. In fact, Mach was close to operationalism when he provided this definition of some physical concepts, for example, the definition of mass (see also Mach's definition of the strength of illumination in [222]).

Mandelstam was prepared to elaborate operationalist philosophy of science by his education at Strasbourg University and his following scientific contacts. He was familiar with German positivist literature (see Chap. 2, Sect. 2.4; Chap. 5, Sect. 5.5; Chap. 6, Sect. 6.7). Apart from what followed his contacts with Richard von Mises, one can refer to his Borovoie diary in which he cited, for example, L. Wittgenstein (Chap. 12).

So, Mandelstam brought to Soviet science the methodology of the German community of physicists. Having interacted with the scientific and pedagogical problems which Mandelstam posed working in the Soviet Union, this methodology yielded his operationalist philosophy.

14.6 Andronov's and Chaikin's Principle of an Expedient Idealization

As an example of the development of Mandelstam's operationalism in his scientific community, we consider the principle of the expedient idealization, which was first formulated in Chaikin's 1935 preface to Russian translation of Baltasar Van der Pol's review on the theory of non-linear oscillations [75], in the Introduction to Andronov, Vitt, and Chaikin's 1937 book *Theory of Oscillation*, in Chaikin's 1948 *Mechanics* [76] and then in his *Physical Foundations of Mechanics*. Andronov also formulated this principle (to avoid any associations with "bourgeoise philosophy", he called it the "principle of a correct idealization") in his 1944 article, written at length, "Mandelstam and the Theory of Non-Linear Oscillations" [18].

In Andronov, Vitt, and Chaikin's book, we read [26, pp. xv–xvii]:

> In every theoretical investigation of a real physical system we are always forced to simplify or idealize to a greater or smaller extent the properties of the system. The nature of idealization permissible in the analysis of a problem is determined by the problem in its entirety.
>
> Thus one and the same idealization can be both "permissible" and "impermissible", or better, expedient or inexpedient depending on questions to which we want to answer. An idealization of the properties of a real system, i.e. use of a mathematical model, enables us to obtain current answers to certain questions about the behavior of the system but does not, generally speaking, give us the possibility of answering other questions correctly about the behavior of the same system.

To trace Mandelstam's root of this principle, let us turn again to Mandelstam's lectures, where he gave the warning "No idealization can be extended to infinity. One needs idealize sensibly, keeping in mind the limits" [1, Vol. 4, p. 148]. By referring to the operations, Mandelstam not only struggled against "pseudo-problems" and took traditional concepts under examination but also explained how to develop and change idealizations.

A good example is provided in Mandelstam's 1930–1932 lectures [1, Vol. 5, p. 72]. The standard idealization of a spring with a small bob on its end, which is hung vertically and whose mass is negligible in comparison with the mass of a bob, is a simple pendulum. One can come across this idealization in many elementary textbooks on mechanics and physics. However, this idealization hangs on our initial operations of a kind aimed to excite oscillations. If we pull down the bob and then allow it to move freely, then the standard idealization is good. However, if we pull the same string down from its halfway point and allow it to move freely, the standard idealization will fail to be helpful. We need to idealize the same set up not as a concentrated system but as a continuous medium system.

To feel better Mandelstam's approach to idealizations, let us recall the example which illustrated his operationalist approach to physical reality (Sect. 14.5). This example fits his approach to idealizations too. If we work with a sieve, we take the idealization of big and small balls; if we work with a magnet, we take the idealization of ferrous and cupric balls.

Idealizations are not new to physicists. Many of them declared that physics presupposed idealizations. To access that development of Mandelstam's philosophy of science which Andronov and Chaikin elaborated upon, one should recognize that for them, an *expedient* idealization became a working device. In I. Lakatos' terminology (see [196]), one can say that for Mandelstam and his followers, it became a kind of "positive heuristic" (see, e.g., Andronov's sketch of the history how he struggled with the idealization of Abraham–Bloch's multivibrator and how Mandelstam and Papalexy helped him in Chap. 9).

In this connection, it is interesting to compare the Mandelstam–Andronov–Chaikin methods with those of the other (to some extent rival) Soviet school in non-linear science, the Nikolai Mitrofanovich Krylov–Nikolai Nikolaevich Bogoliubov school (see Chap. 10, Sect. 10.2).

By calling their subject "nonlinear mechanics", Krylov and Bogoliubov emphasized that their approach differed from that of Mandelstam, Andronov, and Chaikin using the terminology of the theory of nonlinear oscillations as the name of their subject. Certainly, Krylov and Bogoliubov employed idealizations, too. However, this was not the main point of their methodology. This was approximations (first of all the asymptotic methods) in the theory of differential equations. The method of expedient idealization was used in Andronov, Vitt, and Chaikin's book as the following two steps procedure: (1) idealization of the phenomenon under consideration, (2) the strict mathematical solution of the differential equation which resulted from this idealization ("the theoretical treatment of the idealized scheme should be carried out with full rigor" [27, p. xv, 26, pp. 18–19]). In turn, Krylov and Bogoliubov tended to take the following course of actions: (1) the strict description of the phenomenon under consideration (or the description of it with minimal idealizations), (2) the solution of the differential equations, which provides this strict description, by means of methods of approximations.

Although Mandelstam and his disciples, on the one hand, and Krylov and Bogoliubov, on the other hand, referred to each other, they did not really discuss works written by the competitive school. To compare their methods, it is worth to turn to Nikolas Minorsky's book [236]. Thus, in his discussion, so-called relaxation oscillations, Minorsky referred to the idealized discontinuous treatment of relaxation oscillations in Mandelstam's and Chaikin's papers. He also mentioned the treatment of these oscillations given within the framework of the asymptotic methods "without involving any a priori idealization" [238, p. 600]. This treatment was an improvement of the method of the Dutch engineer Balthasar Van der Pol who strictly described the relaxation oscillations but solved the differential equation by the isocline method.

So, the method of expedient idealization is worth to be mentioned not only due to its philosophical connotations. This method is not trivial as a tool at a physicists' hand.

14.7 A.D. Aleksandrov Versus L.I. Mandelstam

Let us recall the first section of the present chapter. This brings up the question: Why A.D. Aleksandrov, an outstanding mathematician, a topologist, the teacher (perhaps, one of the teachers) of the great mathematician Grigorii Perelman, took a floor to criticize the lectures which had been delivered by the prominent physicist, Member of the Soviet Academy of Sciences, L.I. Mandelstam, the lectures published posthumously, five years later L.I. Mandelstam's death? We cannot find the only answer to this question.

First of all, we should take the "mathematicians—physicists controversy" into account. Operationalism is the physicist's world view. It was put forward by the physicist Percy Bridgman who took the methodological considerations which the other physicists provided into account. As he himself wrote, he proceeded from Einstein's definition of simultaneous events, the definition which refers to the synchronization of clocks distant one from other.

To illustrate his position Bridgman put the question: What does mean the length? The length means the operations which allow us to measure the length of any object, the operations with a ruler or any other instrument to measure the length.

Operationalism has never been popular among mathematicians. Mathematicians look for the foundations of mathematics by considering the set theory and logic. True, there is constructivism as the philosophy of mathematics. There are constructivists' theories which appeal to the concept of algorithm. However, this concept is mathematical. It has a mathematical definition, and it is included in the context of mathematical theory.

If a mathematician is asked what the length is, he would refer to the concept of metric. Metric is a mathematical formalization of the distance. Length can be treated as a special case of the distance. However, the distance has a mathematical definition.

This example helps us to understand the idiosyncrasy of A.D. Aleksandrov as a mathematician with respect to the methodology of operationalism.

However, as was mentioned, there are other reasons for such idiosyncrasy. Operationalism was inconsistent with the symbols of the Soviet citizen's belief. It was inconsistent with the concept of the "matter", "objective reality", "dialectics", since it refers to the human being's action and in the long run to the consciousness. By criticizing Mandelstam's lectures, A.D. Aleksandrov met the Soviet citizens' common sense. Here, by speaking of the Soviet citizens, we mean engineers, researchers, journalists, officials, students, schoolteachers, etc., in other words, we mean the educated Soviet citizens. We do not take into account the intellectuals educated in the Russian prerevolutionary traditions. Their amount was small, and they were not influential.

A.D. Aleksandrov wrote in the stile of the Soviet ideology in 1958, too (The All-Union meeting on the philosophy of contemporary science). However, this was another period in the development of the Communist ideology. Heavy battles against the bourgeois philosophy which took place at the end of the 1940s became a thing of the past. Stalin was dead, the head of Stalin's security service Beria had been executed... The problem to criticize the bourgeois ideology became *one* of the problems which the Communist Party put before the Soviet philosophy. The authorities became to proclaim the creative development of Marxism–Leninism. In his Introductory Address President of the Academy of Sciences Nesmeianov told about the "objective laws" of the development of science. Philosophers should take these laws into account.

Nevertheless, A.D. Aleksandrov wrote the following:

> The theory of relativity is a physical theory of space and time and the fundamental concepts "motion", "mass"; "energy", etc. are connected with this theory…
>
> Space and time are the forms of being of matter. This means that the space-time relations don't exist in themselves, as they are; they are determined by the material interconnections of things and of phenomena. Correspondingly, the laws of these relations (the properties of space and time) are the laws of the general structure of the material interconnections of things and phenomena…" [9, p. 93]

It is remarkable that Aleksandrov's opponents who came out at the 1952 all-institute colloquium took the concepts of matter, of reality non-critically, too. They tended to explain that L.I. Mandelstam actually was a materialist. They claimed that one really could point to some small deviations from materialistic orthodoxy in his lectures which were published as the fifth volume of his "Complete Works".

There is a comparison of the communist ideology with religion [189]. This comparison is rather productive. Marxist–Leninist excursions to the scientific concepts did not lead to any productive shifts in science. These excursions could not be falsified, operationally improved, productively criticized, in a word they have not led to any positive steps in the development of science.

Above, we have been concerned with two possible reasons of A.D. Aleksandrov's attack of Mandelstam's fifth volume.

The third possible point is career. The Soviet science has been constructed hierarchically. The great amount of research workers makes the "ground floor", the Corresponding fellows of the Academy of Sciences make the "second floor", and the Academicians make the "upper floor". There were academicians of the Academies of the Union Republics. They approximately correspond the Corresponding members of the USSR Academy of Sciences. Among research workers, there was a hierarchy too. There were scientists with the Doctor of Science degree, with Candidate of Science degree, and the researchers without any degree.

Aleksandrov was Corresponding Member of the USSR Academy of Sciences. Naturally, he wanted to become an Academician. In his lecture, A.D. Aleksandrov referred that by analyzing Mandelstam's writings he carried out the request of the "one organization". He referred to the "one organization" in his closing speech too. It is very probable that the "one organization" was the Central Committee of the USSR Communist Party. This organization could be helpful for a person who wanted to make a step in his career.

This section is based on material containing in [324, 325].

Chapter 15
L.I. Mandelstam's Interpretation of Quantum Mechanics in the Context of the Discussions of the 1930–1940s

15.1 Mandelstam and Quantum Mechanics

Operationalism is only one aspect of the philosophy developed by L.I. Mandelstam. This philosophy cannot be characterized without explaining his attitude to one of the most controversial problems of twentieth-century physics, namely to the problem of the interpretation of quantum mechanics. This problem was discussed by many leading physicists of the 1930s (Niels Bohr, W. Pauli, M. Born, W. Heisenberg, A. Einstein, E. Schrödinger, P. Jordan, M. Laue, H. Weyl, et al.). Mandelstam also discussed it.

Indeed, the development of quantum mechanics turned out to be in touch with the sophisticated picture of reality. Classical physical theories, including those which were the core physics of the twentieth century, described reality, nature as it exists without human beings, and regardless of a human being, i.e., described it objectively. Along with the development of quantum mechanics, its interpretation arose, the interpretation which enters into the physical theory of the generalized image of the scientist and experimenter (the image is usually called the "observer"). This interpretation was proposed by Bohr, Heisenberg, Pauli, and many other physicists who have made a decisive contribution to the creation of quantum mechanics in the 1930s. It was named the Copenhagen interpretation in honor of the Institute for Theoretical Physics in Copenhagen, headed by Niels Bohr. Since the Copenhagen interpretation is presented in basic textbooks on quantum mechanics (L.D. Landau and E.M. Lifshitz, D. Bohm, A. Messiah, etc.), it later became known as standard and orthodox.

In 1927, when the Copenhagen interpretation and quantum mechanics itself were at the stage of their early development, Albert Einstein delivered a paper at the fifth Solvay Conference [95]. He called the Copenhagen approach into question and outlined an alternative interpretation of quantum mechanics. This paper marked the beginning of Einstein's opposition to the Copenhagen interpretation and its criticism. Along it radical criticism arose, this was the program of "hidden variables": Introduction of "hidden variables" should make quantum mechanics

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closer to classical physics. In some of his critical statements, Einstein was close to the "hidden variables" program. However, the more rigid criticism was provided by the Viennese philosopher Karl Popper, who became very influential and popular in the postwar years, when he received the status of Reader at the London School of Economics and Political Science. Popper followed Einstein. However, his criticism was more philosophically systematic (see [286]).

L.I. Mandelstam belonged to the soft critics of the Copenhagen Interpretation, who did not formulate an explicit alternative, but tried to penetrate to the foundations of quantum mechanics and made skeptical comments with respect to the orthodox formulations.

In Chap. 8 the Mandelstam-Leontovich article "On the theory of Schrödinger equation" was taken under consideration. Apart from having published this article, Mandelstam did not publish anything on quantum mechanics. His collaborative with I.E. Tamm in the article about the energy–time uncertainty relation was published after L.I. Mandelstam's death in 1945. Nevertheless, Mandelstam was in touch with quantum mechanics in his lectures and seminars. He did not formulate new problems, and he explained the foundations of quantum theory. As was mentioned in Chap. 6, L.I. Mandelstam wrote Richard von Mises about his interest in quantum mechanics. This was in 1928 when Richard von Mises published his philosophical book "Probability, statistics, and truth" (in Russian translation—"Probability and Statistics"). In this book, Richard von Mises presented his frequency conception of probability and applied this conception by discussing the foundations of physical theories, in particular the foundations of quantum mechanics.

L.I. Mandelstam systematically presented his approach to quantum mechanics in his 1939 lectures delivered at Moscow State University (these lectures had the subtitle "The theory of indirect measurement"). As is said in the Mandelstam biography, "Mandelstam came to the final clearness and clarity in his physical interpretation and understanding of the principal foundations to quantum mechanics" in these lectures [1, Vol. 1, p. 52]. However, Mandelstam went on to work on the foundations of quantum mechanics. He was planning the second part of his course, the part about the mathematical foundations of quantum mechanics. These lectures have never been delivered. We have only two small fragments. One of them, written in 1942–43, was published in [1, Vol. 3]. This fragment can be treated as a result of preparatory work under the article about the energy–time uncertainty relation. Partially in this fragment, Mandelstam's ideas, which were present in his 1939 lectures, were specified and developed.

In his 1942–43 note "On energy in wave mechanics" (the exact data remains unknown) Mandelstam objected to L. Landau and R. Peierls' interpretation of the energy–time uncertainty relation [199] (this interpretation was used in [197]).

The second fragment is published as an appendix to Mandelstam's "Lectures".

However, the "final clearness and clarity" (that was emphasized in the biography of Mandelstam) has not come with the Mandelstam–Tamm article. This article was taken under criticism by V.A. Fock in his article collaborative with his former graduate student N.S. Krylov published in 1947 [185, 186]. Louis de Broglie wrote that the energy–time uncertainty relation was deduced by Mandelstam and Tamm on the base of controversial assumptions [87, p. 160].

It is important, however, that this article came to philosophical discourse. It is cited in the writings on the history of quantum mechanics.

Not only Mandelstam's disciples highly appreciated the Mandelstam 1939 lectures which S.M. Rytov wrote down. In A.D. Sakharov's recollections, there is an episode: by the end of his first visit to Tamm's office (Tamm was his supervisor) he had received two books and a manuscript. The books were W. Pauli's books on the theory of relativity and quantum mechanics. The manuscript was Mandelstam's lectures on quantum mechanics. A.D. Sakharov considered that these lectures are wonderful with respect to their clearness and profundity [306].

15.2 Controversies in the Interpretations of Mandelstam's Interpretation

The Mandelstam interpretation of quantum mechanics itself was interpreted in different ways. For example, I.E. Tamm, who was close to Mandelstam, did not see any differences between the interpretation, which Mandelstam put forward, and the Copenhagen interpretation. Tamm always spoke in favor of the Copenhagen interpretation [337, 338].[1] The popular statement belongs to him: "There is not any correct interpretation of quantum mechanics that would differ from the Copenhagen interpretation" [335, p. 193, 339, p. 434].

V.L. Ginzburg, Tamm's former graduate student, the Nobel Prize winner, too, spoke in the same spirit.

In contrast to Tamm and Ginzburg, M. Jammer, the historian of science, ascribed the Mandelstam interpretation to the type of interpretations which arose in opposition to the Copenhagen interpretation and was declared by D.I. Blokhintsev after World War II [166]. In other words, Jammer considered that the Mandelstam interpretation belongs to the statistical or ensemble interpretations. D.I. Blokhintsev himself wrote that his interpretation went back to the Mandelstam interpretation [51].[2]

V.A. Fock, who was asked to participate in the preparation of Mandelstam's lectures on quantum mechanics for publication, only corrected a couple of technical mistakes. However, in private conversations he expressed his disagreement with the conception of Mandelstam's lectures. V.A. Fock, whose interpretation of quantum mechanics was very close to the Copenhagen interpretation (see [119]), considered that the Mandelstam interpretation provided an incorrect answer to the question what the statistical collective in quantum mechanics is (see Fock's 1951 review of the Fifth Volume of Mandelstam's "Complete Works" [117] and his subsequent articles on

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[1] Tamm spoke in favor of the Copenhagen interpretation in his early paper [331]. The Copenhagen approach to quantum theory was well known among Soviet physicists mainly due to [151].

[2] Blokhintsev's book [51] was rather popular in the USSR.

the interpretation of quantum mechanics [118, 119]. Fock highly rated Mandelstam's book, but he objected to Mandelstam's ensemble interpretation.

A.D. Sakharov, whose admiration for Mandelstam's lectures on quantum mechanics was mentioned, distinguished the Mandelstam interpretation from the Copenhagen interpretation as it is presented in the Landau–Lifshitz book.

We come to the question about the logical types of the interpretations of quantum mechanics and to the question what the ensemble interpretation is.

15.3 Definition of the Ensemble Interpretation

To reach historical accuracy, let me formulate an inexact definition of the ensemble interpretations. I call an "ensemble interpretation" any interpretation which places an emphasis on the concept of a statistical collective. It is well known that the Copenhagen (orthodox) interpretation treats quantum mechanics as a theory which in its foundations tracks the behavior of a single physical system (an atom, electron, etc.). If the concept of an ensemble arises under this interpretation, it arises as a logically derivative concept: The concept of ensemble is introduced as the instrumental (empirical) interpretation of the mathematical apparatus being formulated. In addition, the concept of an ensemble appears in quantum statistical mechanics to extend the principles of quantum mechanics to the mixed states which are represented by the density matrix. In turn, the ensemble interpretation connects the essence of quantum mechanics with the concept of a statistical collective.

To make this clearer, let me distinguish between two basic interpretations of any abstract physical theory: an instrumental (empirical) interpretation and an interpretation that contributes "to our understanding of the natural world". The former interpretation consists of a set of rules which connect the mathematical symbols with brute facts, the latter interpretation constructs a "real description of the physical world" [92, p. 2]. The instrumental interpretation of quantum mechanics is statistical: It was proposed by Born, who considered electron collisions and defined the square of the amplitude of the wave function as the probability of finding the particle at a given point in space (here the present author follows traditional terminology: as shown by L. Wessels [379], what we call Born's interpretation was really formulated by W. Pauli, who improved Born's formulation).

Born's interpretation was generalized by P.A.M. Dirac and von Neumann. However, Born's interpretation was itself interpreted in two ways. The first was provided by the ensemble treatment of probability. The probability of finding a certain value of the observable (physical magnitude) refers to the fraction of all systems in the ensemble which are characterized by the prescribed value. One can find a refinement of the ensemble probability in Richard von Mises' definition: probability is the limit of the sequence of relative frequencies with increasing number of trials (1919) (see: Chap. 2, Sect. 2.6). Given this definition, one can test the probability that results from the wave function with the probability that follows from measurement (actually, the latter probability emerges from a finite number of trials).

The second approach to Born's interpretation of the wave function refers to the probabilities of single events. This interpretation does not allow one to test the probability resulting from the wave function against a measurement. However, quantum mechanics itself guarantees the empirical status of this probability, since it is verified by its ample application.

By distinguishing between the ensemble interpretations of quantum mechanics and its Copenhagen interpretation, one proceeded from their reconstruction of the physical world. In explaining quantum mechanics, the Copenhagen-type interpretations refer to thought experiments with a single physical system, whereas the ensemble interpretations basically use the image of statistical collectives. As a matter of fact, both the Copenhagen and the ensemble interpretations referred to Born's statistical interpretation of the wave function. Historically, however, the Copenhagen interpretations tended to consider probabilities of single events. Nevertheless, there are several exceptions. For example, D.I. Blokhintsev in his 1944 Copenhagen-oriented "Introduction to Quantum Mechanics" used the ensemble concept of probability. In turn, the proponents of the ensemble approach unanimously use the ensemble probability. It is not surprising: by referring to ensemble probabilities, they show the fundamental importance of ensembles for quantum theory. In this connection, Blokhintsev' trajectory is remarkable: In his publications after World War II, Blokhintsev already combined the ensemble treatment of probability with the ensemble interpretation of quantum mechanics (he referred to Mandelstam as a predecessor). But the present chapter does not embrace the post World War II discussions.

15.4 The "Real" and Ideal (Gibbsian) Ensembles

To describe the ensemble interpretations of quantum mechanics, a narrower definition should be formulated. For his part, Popper regarded quantum mechanics as a theory of "real ensembles" (to use E.J. Post's term; in developing Popper's interpretation Post introduces the concept of the real ensemble as the "phase and direction randomized ensemble" [287, p. 55]). By contrast, the scientists whose ensemble interpretations are described here dealt (with a reservation concerning Mandelstam) with ideal Gibbs ensembles. Popper meant the aggregate of particles which all are in the same state—to produce such an aggregate one needs to fix the macroscopic parameters of the producing device. For example, if in a vacuum tube a hot filament emits a beam of electrons, the temperature, voltage, configuration, etc., of the filament must be specified. The leading proponents of the ensemble interpretation presupposed experimental ensembles producing identical systems in identical quantum states (or if you like, they meant experiments which repeatedly and many times placed the same system in the same quantum state).

The leading proponents of the ensemble interpretation distinguished between two kinds of experimental operations in quantum mechanics: state preparation and measurement. Popper, however, spoke of the preparation of "real ensembles". His "preparation of state" was the production of an ensemble in a fixed state. The American physicist E. Kemble and the physicist–philosopher H. Margenau spoke of an ensemble of preparations. They meant a set of operations, each of which placed a system in a quantum state [173, 174, 218, 226].

The quotes for "real ensemble" are intentional, since these ensembles are real only in comparison with ideal Gibbsian ensembles. Actually, "real ensembles" are also a consequence of thought experiments. The "real ensemble" must have such a low density that one can treat its elements as independent from each other. Each system in such an ensemble is in its quantum state, but all these states are identical. American physicist collaborating with N. Bohr in 1924 John Slater is one of the first proponents of the ensemble approach. In 1928, he delivered a paper at a symposium on quantum mechanics held under auspices of the American Physical Society [319]. He stressed that quantum mechanics "operates with ensembles" [ibid., p. 453]. "Just as in ordinary statistical mechanics, we must here choose an ensemble,… by considering the sort of statistical distributions actually present in the repetitions of the experiment being performed" (ibidem).

In his recollections he provides the following explanation: "An ensemble… represents a collection of many repetitions of the same experiment, agreeing as concerns the large scale or macroscopic properties which we can control, but taking different values of microscopic properties which are on such a small scale that we cannot experimentally determine or control them. It does not necessarily imply a system with a great many particles in it. The essence of the ensemble is the large number of repetition of the experiment… The probability of finding certain coordinates in certain ranges... means simply the fraction of all systems in the ensemble which lie within the prescribed limits" [321, p. 44].

E.C. Kemble (he was a teacher of Slater) followed Slater but he used more refined terminology. He spoke of an ensemble of pairs "the preparation of state and measurement". He meant the "Gibbsian assemblage of identical systems so prepared that the past histories of all its members are the same in all details that can affect the future behavior as that of original system" [173, p. 54]. Measurement consisted in a "series of observations on suitably prepared assemblage of completely independent systems each in its own separate box and laboratory" (ibid., p. 55).

The American physicist and philosopher H. Margenau first described measurement on the "real ensemble": "Numerous observations, or a single collective observation, on a physical assemblage of many similar systems in the same state" [226, p. 352]. Although he did not use the term "Gibbsian ideal ensemble", he subsequently specified it: "Numerous repeated observations on the same system, state in question being reprepared before each observation" [ibid.]. Margenau emphasized that namely the latter ensembles lay in the foundations of quantum mechanics.

K.V. Nikolsky is the first Soviet physicist who proposed the ensemble approach, describing it in his 1936 article [256]. In the foundations of quantum mechanics, Nikolsky stated the ensemble of "quantum processes", that is, the "ensemble of experiments with single quantum particles that had initially been set in the certain conditions" [256, pp. 26, 27] or the "ensemble of passages of microparticles through a diffraction device" [ibid., p. 148]. Since Nikolsky, in contrast to Margenau, did not sharply distinguish between preparation of state and measurement, he spoke of two kinds of measurement: the former is measurement that formed (in Kemble terminology, prepared) an ensemble. The latter is measurement in the proper sense, measurement that sorted an ensemble according to the values of a physical magnitude, and provided the measurement of the magnitude. As Nikolsky spoke of the ensembles of quantum processes, he meant measurement in the latter sense: his quantum ensembles were ensembles of measurement operations which issued statistics.

How did Nikolsky define probability? Let α be a magnitude which is measured. As a result of measurement, the original ensemble is divided into a number of subensembles corresponding to values of the magnitude $\alpha_1, \alpha_2\ldots$ resulting from measurement. The probability that under measurement $\alpha = \alpha_1$ equals the ratio $N_\alpha 1/N$, at $N \to \infty$, where N is a number of measurement operations (elements of the ensemble) and $N_\alpha 1$ is a number of the measurements issuing the value α_1 (elements of the subensemble).

In his 1939 Lectures on Quantum Mechanics, L.I. Mandelstam presupposed "real ensembles". However, in his lectures on the reduction of the wave packet and on the Einstein–Podolsky–Rosen argument, ideal Gibbsian ensembles manifested themselves. Mandelstam also refers to the ideal Gibbsian ensembles in his 1942 manuscript "On energy in wave mechanics" which is historically and logically connected with his article written with I.E. Tamm "The uncertainty energy-time relation in nonrelativistic quantum mechanics" [1, vol. 2, pp. 306–315, 339, 340] (this article was published in Russian in *Izvestia AN SSSR*, Seria of Physics and in English in *Journal of USSR Physics* in 1945). In this manuscript, the concept of measurement of energy at a given moment is under discussion. "Let the wave function be $\psi(x, t)$. In order for statistics to make sense, reiteration must be performed, that is, the experiment must be repeated many times, where t is the time elapsed from the beginning of the experiment in each of the experiments. The measurement at a "given moment of time" is the measurement in different experiments, but each time at the same time from the beginning of that experiment" [1, Vol. 3, p. 402].

15.5 One More Distinction: Hidden Variables

E. Post believes that the interpretation of quantum mechanics in terms of the ideal Gibbsian ensembles looks like the "Copenhagen-oriented text" [287, p. 11]. However, the relation of the Gibbsian ensemble interpretation to the Copenhagen approach calls for further explanation. Gibbsian ensembles were not in themselves an antidote against the "hidden variable" spirit. One more distinction should be drawn. Following D. Home and M. Whitaker [160], let us distinguish between minimal ensembles and ensembles of which the elements are characterized by preexisting initial values (the PIV ensembles). The minimal ensemble simply is ensembles of similar physical systems prepared in the same quantum states. No physical properties beyond those which the instrumental interpretation attaches to the ensembles are envisaged; that is, elements of the ensembles are characterized by the probabilities and the means (mathematical expectations) of physical magnitudes (observables) which are intended to be measured. In addition, the PIV ensembles are characterized by their premeasured objective probabilistic structures. The minimal ensembles are described with respect to actual or potential measurement, whereas in the PIV ensemble at all times all physical magnitudes have precise values.

Popper was a "believer in PIVs" [160, p. 280]. The American and Soviet proponents of the ensemble approach tended to hold the minimal ensemble approach. In view of von Neumann's theorem, they proceeded from the fundamental completeness of quantum mechanics. It is true that this gave Nikolsky pause. In his polemics with V.A. Fock, Nikolsky spoke in favor of Einstein's 1935 approach, which opened the way to the PIV interpretation [257, p. 558]. However, all our protagonists eventually came to the minimal ensembles. Nikolsky's personal trajectory provides a good example. In his 1941 book, he refused to acknowledge Einstein's approach and spoke in favor of the minimal ensembles [259, p. 147]. In this book, the quantum ensembles were treated by him in the strong connection with the process of measurement (the above citations).

In this connection, it is useful to follow how the proponents of the ensemble approach treated the Heisenberg uncertainty relations. M. Jammer distinguished between two ways of the interpretation of the uncertainty relations: a "non-statistical way", according to which these relations provided the principle of limitations in measurement precision, and a "statistical" way which could be summarized as follows: the product of the standard deviations of two canonically conjugate variables has a lower bound given by $h/4$ [285, p. 81]. All the proponents of the ensemble interpretation accepted the latter approach. However, to demonstrate the significant importance of the uncertainty relations, Kemble and Mandelstam additionally referred to the thought experiments with a single system. In turn, Nikolsky and Margenau restricted themselves to the "statistical" interpretation of the uncertainty relations. "The scattering of measurements has its roots in a fact more fundamental than the destruction of states by interaction with measuring device, namely in the definition of states peculiar to quantum mechanics" [218, p. 422]. According to Nikolsky, "the formulae which express those which are called the uncertainty relations by the Copenhagen school allow us to quantitatively formulate how quantum ensembles differ from classical ones" [259, p. 65].

By contrast the "believer in PIV" considered the uncertainty relations to be just macroscopic formulas which did not rule out the possibility of exact predictions concerning single particles [285, pp. 218, 223, 229, 234–235]. Here we cite an English version.

In the preceding section, Mandelstam and Tamm's article on the energy–time uncertainty relation was mentioned. This article also formulates the uncertainty relation in a statistical way. As was mentioned in Sect. 14.1, V.A. Fock and his former graduate student N.S. Krylov criticized Mandelstam and Tamm's article. Fock and Krylov distinguished between two senses of the energy–time uncertainty relation [185, 186]. They credited N. Bohr with the former sense: this is concerned with a single particle and a single measurement. The latter sense (Mandelstam and Tamm) is concerned with statistics of the measurements. This is the relation between the uncertainty in energy of the ensemble of particles prepared with a given energy, on the one hand, and "the standard time", that is, the time that it takes for some other ensemble magnitude to change its value over the value of its standard, on the other hand.

In essence, Tamm agreed with Fock and Krylov and acknowledged that his deduction of the energy–time uncertainty relation developed with Mandelstam had a lack of generality [337]. In [339, 340], the old version of this article is published. This remark on the generality is absent in it.

15.6 Soviet Ensemble Interpretations: K.V. Nikolsky

It is interesting that the ensemble interpretations of quantum mechanics were rather popular in the USSR in the 1930s. Konstantin Viacheslavovich Nikolsky, who, like Mandelstam, worked for the Physics Institute (FIAN), was the main propagandist. According to recollections and archival material,[3] his quantum endeavors were supported by the institute's director S.I. Vavilov (who became President of the Academy of Sciences after World War II).

The FIAN archives hold the letter of Nikolsky to Mandelstam. However, nothing is known about their contact. Mandelstam's former graduate student S.M. Rytov recalled that K.V. Nikolsky spoke "reasonable things" (his interview given to the present author).

In common with some foreign proponents, Nikolsky was a "molecular structuralist": Nikolsky's major book which summed up his results was "Quantum mechanics of a molecule" (1934) [255]. He also wrote a popular book *Photon* (1936). In 1935, Nikolsky enthusiastically wrote to his former supervisor V.A. Fock that his article on the foundations of quantum mechanics was accepted by *Physics Uspekhy* and was about to appear. This article led to a hard polemics [116, 257] that broke up their relationship and which was restored only in the 1960s when Nikolsky, who had just returned from the mental sanatorium, begun to send mathematical puzzles to his former supervisor and friend. Nikolsky asked Fock to help him with the publication of his puzzles.

Nikolsky's "Quantum mechanics of a molecule" was written in the Copenhagen spirit. But it contained hints as to why he later arrived at the ensemble interpretation. Like many books on quantum mechanics of molecules, it widely used approximations which historically and logically connected with the Bohr–Sommerfeld quantum theory and the prequantum models of molecules, hence sharing in the ontology of particles. However, Nikolsky directly pointed to his preference for particles. This preference proceeded from the role of the potential energy in the quantum theory of molecules. The system of interacting particles is characterized by a complicated structured potential energy. The essence of approximations in this field consists in simplification of the potential energy by neglecting one or another of its components.

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[3] (1) Interview with S.M. Ryrtov; (2) The Archives of Lebedev Physics Institute (FIAN) contain a copy of the director 1937 order to give a bonus to Nikolsky for his contribution to the foundations of quantum mechanics.

Some of the proponents of the ensemble approach held to the particle-wave symmetry in their discussion of the interpretation of quantum theory. Nikolsky spoke in favor of particles. He suggested what amounted to modeling waves by means of particles. "When we take under consideration a particle with a definite energy and momentum, to determine its future behavior, we must invite the totality of its possible motions with the following initial conditions: the definite momentum and arbitrary coordinate. This is just what we call a plane wave" [255, p. 15].

Nikolsky went further. In his 1941 book, Nikolsky treated the particle-wave duality as the duality of a single particle and an ensemble of particles (p. 28). As the wave function must represent the state of an ensemble, the particle-wave duality is resolved in favor of ensembles. "Quantum mechanics has yet not been elaborated as a theory of individual processes… An individual process is treated via the prism of the statistical method" [259, p. 28].

Nikolsky pushed materialism as early as his 1934 book.[4] He insisted that "all the physical phenomena are processes which are progressing in time" (p. 10). Later he contrasted his point of view to the Copenhagen one, identifying the contrast as one between materialism and idealism. "Heisenberg's approach", he wrote, "leads to giving up objective processes progressing in space and time, that is, it leads to an explicitly idealistic conclusion" [258, p. 28]. He also contrasted his point of view to the approach of what he called the "the Soviet branch of the Copenhagen school", that is, to the conceptions pushed forward by V.A. Fock, L.D. Landau, and M.P. Bronstein [258, p. 557].

Besides Fock and Landau, Nikolsky mentioned Matvey Petrovich Bronstein (1906–1938) who worked for Leningrad Institute of Physics and Technology. In 1937, he was arrested and then executed.

By contrasting scientific objectivity to the Copenhagen school, Nikolsky, however, proceeded more than philosophical materialism. He took ensembles as vehicles of scientific objectivity. Let us follow Nikolsky's discussion. In his terminology, the quantum particles did not exist independently from the macroscopic bodies which they composed, and they could not be cognized independently from the macroscopic bodies. The quantum of action specified the type of interconnection between the quantum particles and the macroscopic bodies, whose behavior is classical. In particular, the measuring device is a macroscopic body. When a measurement is performed, a microscopic system reacts to the measuring device, but this reaction is inevitably uncertain. "However, it is possible to avoid this uncertainty resulting from the use of classical means in the quantum realm. To do this the problem must be posed in a statistical way. A statistical treatment does not imply an elimination of uncertainty. But it is a method to describe quantum processes as objective reality in spite of the uncertainty" [256, p. 54].

As was mentioned, in his 1941 book, Nikolsky directly referred to Kemble's "objective states". By formulating quantum mechanics as a theory of an individual atomic system, Nikolsky claimed, we inevitably come to the conclusion that the

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[4] Objectivism in the philosophy of quantum mechanics should not be mixed with materialism and even realism. About the structure of the concept "objectivity" see [86].

"wave function is a notebook of an observer". The quantum ensembles allow us to restore objectivity [259, p. 150].

In essence in his 1936 article and 1941 book, Nikolsky used Gibbsian ensembles. True, he did not refer to Gibbs. It should, however, be taken into consideration that in parallel to his work on the foundations of quantum mechanics Nikolsky worked hard to translate Gibbs' *Principles of Statistical Mechanics* into Russian. Although his translation was published in 1946, judging by the date of the translator's Preface, the translation was completed and prepared for publication in 1940.

As a materialist, Nikolsky was happy to note that statistical mechanics celebrated atomism (the atomic theory of matter). Nevertheless, in the spirit of Americans, he put emphasis on Gibbs' idea that "ensembles possess more reality than individual events" [254, p. 8] and he stressed that Gibbs' method is of much importance for quantum theory.

We have a brief outline of Nikolsky's biography. He was born in 1905. He graduated from the North Caucasus State University in Nalchik in 1927. In 1927–28, he was a graduate student at this University. In 1929–30, he worked at the theoretical department of the State Optics Institute (Leningrad). In 1930–34, he prepared his Doctor Science Dissertation at the Institute of Mathematics and Mechanics of Leningrad University (his scientific supervisor was V.A. Fock). The extended text of this dissertation was published as a book ("Quantum mechanics of a molecule"). Since 1936, he had worked at the Lebedev Physics Institute of the Academy of Sciences in Moscow. In 1946, he was arrested for his anti-Soviet statements. However, the judge concluded that his statements resulted from his mental disease. He was treated in the psychiatric sanatorium and then he was under guardianship of his sister and (after her death) the psychiatrist Dr. Beniash. Nikolsky died in 1979.

15.7 The Prerequisites of the Mandelstam Interpretation

As a matter of fact, Mandelstam's interpretation of quantum mechanics has been characterized in the previous sections. Its prerequisites can be summarized in the following three statements. (1) Richard von Mises' empirical frequency conception of probability, called by some of Mandelstam colleagues an "objective" conception; (2) operationalism: as was shown in Chap. 15, in his lectures on quantum theory and in some other his courses Mandelstam developed his original operationalism, which can be traced back to Mach's philosophy[5]; (3) the statistical ensemble treatment of physical experiment and measurement—Mandelstam developed the "oscillatory ideology" which presupposed a transformation of the theory of oscillations into

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[5] American proponents of the ensemble interpretation were also influenced by operationalism: see Kemble's writings [175]. Slater prepared his Ph.D. thesis under Bridgman and attended Kemble's lectures. Kemble also started under Bridgman [309]. Margenau criticized Bridgman's operationalism, but he was also influenced by this philosophy. There are cross-references in American writings about the ensemble interpretation of quantum mechanics [122, 173–175, 319–321]. Kemble, Slater, Nikolsky implicitly proceeded from the ontology of particles (about this ontology see: [132]).

the universal language of physics (and perhaps science) and emphasized regularly repeatable phenomena, ensembles of phenomena.

In contrast to Nikolsky, Mandelstam was strongly attracted by the wave ontology. His favorite way of explanation was to appeal to the "wave notions" (the wave packet, modulation, etc.). In fact, his article written with Tamm 1945, where the uncertainty relation between energy and time was derived with the benefit of the Schrödinger equation, repleted with the wave ontology. Mandelstam and Tamm started their section concerning "examples" by considering the wave packet for which the center of gravity, the width ΔR, and the time of travel ΔT were fixed. In their opinion, the uncertainty relation showed that the localization precision of the time of travel of the wave packet through a certain space point is directly dependent on the dispersion of the full energy and cannot be large at a small value of the latter.

V.A. Fock in his article written with N.S. Krylov criticizing Mandelstam and Tamm's paper (see Sect. 14.4) showed that this example could be translated into the particle language. According to Krylov and Fock, the wave packet presented the statistics of the measurements on the ensemble of particles prepared with an average value of energy.

For the proponents of the ensemble approach, objectivity was associated with the description of regularly repeatable experiments and measurements. By contrast, the Copenhagen scientists, who proclaimed the reduction of the wave packet resulting from a single act of observation, attached much importance to a single experimental event.

It is natural to state that Slater, Kemble, Nikolsky, and Mandelstam adopted the classical culture of macroscopic experimentation which results in statistics (see [124]). By contrast, the Copenhagen physicists who pushed the conception of the wave packet reduction were theoreticians who presumably conducted thought experiments. Nevertheless, let me suggest a material counterpart of their thought experiments with single particles. By analyzing the experimentation culture of the first half of the twentieth century, P. Galison distinguishes between two traditions which he conventionally calls "logic" and "image" traditions [125]. In the logic tradition, the classical culture of macroscopic experimentation has been continued. This is a tradition to "sacrifice the details of one for the stability of many" [125, p. 20]. The image tradition has a presumption that "a single picture can serve as evidence for a new entity or effect" [ibid.]. This tradition is provided by the invention and usage of the devices that worked like an eye. Let me cite an explanation of the quantum measurement which runs in the style of the image tradition: "In general between the observer and the quantum object there is a so-called classical device which under the action of the measured quantum object irreversibly changes its own state in a manner that the observer can directly comprehend. Examples of such classical measuring devices are the photo emulsion of photographic plate, the supersaturated steam in a Wilson cloud chamber… There is even an example in which the device was a human eye: Pavel Cherenkov discovery of Cherenkov radiation in which he detected individual photons directly with his eyes" [58, p. 39].

15.8 B.M. Hessen and A.A. Andronov

Nikolsky did not influence Hessen as Nikolsky started to publish his articles on the foundations on quantum mechanics in 1936. Hessen was arrested and executed in 1936. Hessen's approach to quantum mechanics was formed under the influence of Mandelstam and his graduate students.

B.M. Hessen touched upon quantum mechanics in the course of his discussion of the concept of probability [152, 154]. He wrote in favor of the ensemble interpretation by arguing in the Mandelstam school manner. In particular, he [147] referred to the principle of expedient idealization typical for Mandelstam's disciples (see Chap. 13, Sect. 13.6). Hessen wrote that we use a causal idealization, when we treat the "macroscopic world". We consider the space-time trajectories of macroscopic bodies then. With respect to microscopic particles, we concentrate on the ensembles. We put aside the causal behavior of particles.

In 1934, Mandelstam's former student A.A. Andronov (see Chaps. 8–10) delivered a course on quantum mechanics at Gorkii University, where he started to work by moving from Moscow in 1931. Andronov's lectures characterized the "air" in which the fundamental problems of quantum mechanics were discussed in Mandelstam's community (see also Chap. 10, Sect. 10.5). Andronov delivered a regular course dedicated to the theory of Schrödinger equation (at the present author's disposal there is a copy of the notes written Andronov's graduate student A.G. Liubina). However, like Hessen, Andronov expressed his dissatisfaction with Copenhagen position. Probably, his dissatisfaction resulted from his philosophical position. Andronov's book written in coauthorship with his wife Andronova-Leontovich [21] shows that he sympathized with materialism as a philosophical position.

M. Jammer in his 1974 celebrated book discussed the Soviet ensemble interpretations of quantum mechanics in a special section entitled "Ideological reasons" [166].[6] In the article [277], the present author compared American and Soviet interpretations which arose before World War II. He showed that, contrary to Jammer, the Soviet physicists shared with their American colleagues not only scientific problems, theories, and ailments, but also the philosophical backgrounds, that is, philosophical problems, theories, and diseases. The relation between American and Soviet ensemble interpretations can be understood as a mutual self-elucidation: By comparing these interpretations, we understand them better.

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[6] As a matter of facts, the Soviet science is separated from Western science in the writings of Sovietologists (see, for example, [145, 146]). A. Vucinich provides a more tacit estimation: He points to the Soviet scientists' opposition to dialectical materialism [378].

Chapter 16
Conclusion

History, Michel Foucault wrote, “is certainly the most erudite, the most aware, the most conscious, and possibly the most cluttered area of our memory; but it is equally the depths from which all beings emerge into their precarious, glittering existence” [120, p. 218].

What is left of Mandelstam in the historical memory? In the Introduction to this book, this has already been mentioned. Let us reiterate what was said in somewhat different terms. Mandelstam’s life and work are reflected in the reports and recollections of his friends and colleagues that have been written in connection with his death. Some of these reports have become the fundamental articles on the development of the scientific problems on which Mandelstam with his colleagues worked. They are adjoined by articles written by Mandelstam’s colleagues and friends on the occasion of the 100th anniversary of his birth, which happened 34 years after his death. Such articles are not very many, because many of Mandelstam’s friends and his coauthors were already dead. In addition, about Mandelstam his contemporaries, former students, and graduate students recollected and did so on different occasions or just for no reason.

About Mandelstam, letters are kept in different archives. Mandelstam was reserved in his letters like he was probably reserved in his life. The letters are laconic enough. Mandelstam often asked to excuse him for a delayed reply. There are no emotions in his letters. A few letters addressed to Richard von Mises make an exception.

Mandelstam left a stream of papers on physics and radio-engineering. This stream started with his Strasbourg dissertation and gained power with the passage of time. In this outpour, Mandelstam’s articles spilled over each other, with the subsequent articles correcting and supplementing the earlier ones. Mandelstam was an integral part of the scientific realm of his time. The common neglect of Mandelstam’s work results in a deficient and often peculiarly lopsided perception of physics in the first half of the twentieth century.

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The radio-engineering equipment with which Mandelstam and Papalexy worked in the 1910s, 1920s, and even the 1930s has gone. But their articles on radio are interesting not only from a historical point of view. They evidenced how a young scientist was growing and how the collaboration between Mandelstam and Papalexy progressed.

The Brillouin-Mandelstam effect and combinational scattering of light were applied in the course of the investigations of the structure of matter and the interaction of matter with radiation. Lasers not only provided better observation of the Mandelstam-Brillouin effect but also led to the discovery of the stimulated effect which has high intensity (the stimulated light scattering has also been discovered).

In the Russian literature, these effects are connected with L.I. Mandelstam and G.S. Landsberg, respectively. However, over the last decades the expression "Raman effect" has become widespread in the Russian literature.

Physicists keep L.I. Mandelstam as a deeply decent man in their collective scattered memory. This characteristic has been fixed by A.N. Krylov, who addressed the 1944 joint meeting of the Department of Physics and Mathematics of the Academy of Sciences and Moscow State University, the meeting in commemoration of Mandelstam. A.N. Krylov called Mandelstam "righteous". S.I. Vavilov told about "uncompromising moral" of Mandelstam at that meeting.

L.I. Mandelstam was a man of science, and his communication with outward things seldom exceeded the bounds of scientific communication. The archives have not fixed any conflicts of Mandelstam with colleagues. His tactful attitude to A.S. Predvoditelev was emphasized above. Mandelstam did not enter any explicit conflicts with Timiriazev, although they were scientifically antipodes. Concerning Kasterin's paper in Odessa (see Chap. 5), the paper resulting in Tamm's indignation, "Mandelstam was at a loss, he could not say anything", as Tamm himself said by describing Mandelstam's reaction.

L.I. Mandelstam lived in the terrible time of Stalin's move to power and his domination. As was noted, Mandelstam's former graduate student A.A. Vitt had been arrested and died in a prison.

Tamm's graduate student who worked for MSU Physics Institute, Semen Petrovich Shubin, was executed in 1938. In the first half of the 1930s, Shubin, who was exiled from Moscow, was in correspondence with Mandelstam [8, pp. 63–64]. In the past, S.P. Shubin was a Trotskyist and, although he broke with political activity, any communication with him was perilous (see [308]).

According to Feinberg's recollections (in an interview which the American Institute of Physics holds, E.L. Feinberg gave information from what others have told) in 1930 in the process of the faculty meeting dedicated to condemnation of the so-called Industrial Party (Prompartii), Mandelstam and Predvoditelev abstained from voting for the suggestion about the capital punishment.

There is no information of how Mandelstam behaved himself in the course of the meetings supporting the condemnation of the "enemies of people". Probably, he tried to avoid participating in such meetings.

Prestige of the Mandelstam School is so high that a number of prominent physicists, who never worked with Mandelstam and with his former graduate students, recommended themselves as members of the Mandelstam community. For example, Corresponding Member of Russian Academy of Sciences, Nikolai Vasilievich Karlov, who was Rector of the Moscow Institute of Physics and Technology and Chairman of the Higher Certifying Commission (1987–1997), introduced himself as a representative of the third generation of Mandelstam's disciples (his recollections are in [172]). However, Academician Nobel Prize winner Prokhorov really was Karlov's supervisor. True, Prokhorov started as Researcher at the laboratory of oscillations that Papalexy headed.

In Chap. 10, we were concerned with A.M. Zhabotinskii's research, which was based on the Mandelstam-Andronov theory of non-linear oscillations and consisted in establishing the mechanism of the Belousov-Zhabotinskii reaction. As was noted, in his main book Zhabotinskii called himself belonging to the Mandelstam School. However, the real supervisor of Zhabotinskii was S.E. Schnol', who was Biophysicist and did not belong to the Mandelstam School.

Chapter 11 shows that some fragments of the Mandelstam School went into a recession. Mandelstam's graduate students did not all become friends with each other. However, they all felt themselves as members of a community. But what Alpert writes about Migulin shows that the moral and spiritual interconnections between those who could be taken as a member of the Mandelstam community tend to become blurred.

Like other structures, scientific schools become blurred; they disperse as time goes by.

Scientific schools are one of the characteristic messages of Soviet science [176, 187]. However, what does this book say about the Soviet science as a historical phenomenon? The facts collected in this book show that science (and scientists) materially and financially were supported by the Soviet government body. We refer to physics, technology, and mathematics. Humanities and biological science need a special discussion. However, science had a moral support in the USSR, too. As we have seen, scientist's scientific results played an important part in his career, in obtaining him the scientific degrees and academic positions. State rewards, premiums, etc., were given to people who really contributed to science. The authority highly appreciated the consultations of scientists. Under many reservations, one can state that the Academy of Sciences enjoyed a kind of autonomy.

The moral support proceeded from many factors: romanticism of the state ideology, its atheism, tacit positivism.[1] Let us recall that when a Soviet official wanted to praise Marxist philosophy he characterized it as scientific and not the reverse. In the Russian historical literature, a remarkable fact was pointed out: Stalin eliminated the term "Marxist biology" in T.D. Lysenko's 1948 paper (this does not mean that he refused to support Lysenko) [299]. Certainly, the totalitarian regime was an obstacle to the development of science. First of all, it was an obstacle to the development of a scientific culture which is connected with international scientific contacts. However, Mandelstam's lectures show that there were loopholes here.

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[1] See [176, 244].

In the contemporary history of science, the topic of the Marxist ideological pressure put upon scientific research became popular (see, e.g., [140, 176, 324–326]). The 1948–1953 “Ideological campaigns” became a favorite topic of the Russian historical literature in the 1990s. Usually, the authors sympathize with the “victims” of the campaign among whom there were prominent scientists (“victims” are those who were called “idealists” and “Machists”). However, they do not pay attention to the fact that in the long run the “victims” often won victories: They often reached success in their social status.

It should also be noted that “the direct contribution of physicists in the military and economic might of the Soviet Union, which provided the high international prestige of the country, was due to their contribution to world science and, what is also important, it was connected with their indirect contribution to the scientific culture and ethics in the country” [176, p. 14]. Since the end of the 1920s, L.I. Mandelstam’s community of physicists had become one of the most important factors that determine not only the standards of scientific productivity and the quality of scientific research, but also the approach to humanitarian components of science and research ethics.

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