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My journey in science (proposed report to the Moscow Mathematical Society)[1]

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Members of the society are well familiar with the works of Moscow mathematicians whom they heard as students and met many times afterwards. Mathematicians from out of town are far less known. Therefore my purpose is to introduce myself to the Mathematical Society. Incidentally, my first such introduction was in January 1930, when I gave my first report to the society, although hardly anybody from those present then is in the audience now.

Before going on to the actual account, I should like to say a little about myself, that is to say, to indulge in some self-criticism. I am not erudite. This could partly be connected with the character of my education—at that time there were no academic grants or wages and so both during my post-graduate time and in the following years I had to combine academic work with two or three teaching posts, all the more from 1930 when I was the main breadwinner of a family. It has to be confessed that both my memory and ability for perception of the new is not a lot above average.

Some people divide mathematicians into those who have mostly a penetrating strength and the mathematical conceptualists. I belong to the second category. In general, I was little attracted to problems posed by others and I particularly did not study famous problems. However, those concepts that were advanced allowed me to solve a number of well-known problems in passing, rather than with directed concentration of effort.

Finally, my work is characterized by a constant overlap of theory and practice, and in relation to practice often stretching to the limits of mathematics.

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(1) Kantorovich prepared this report during the last months of his life, when he was in hospital. Unfortunately, work on the report remained unfinished. This text of Kantorovich's proposed speech was written down from his words and prepared for publication by V.L. Kantorovich. [ed.]

Descriptive function theory

Owing to a certain almost anecdotal incident, my entrance to university happened in such a way that I began my studies only in the middle of November[(1)]. Therefore my first year was spent totally in private study—it was necessary in the rest of the time to prepare and pass a significant complex of subjects.

My scientific work began in transferring from the first year to the second—in preparing for the student scientific circle classes that Professor Fikhtengol'ts was about to begin with the second year; I was given the theme of the conditions of Riemann integrability of functions. For this I studied the text of sections related to functions of a real variable, as at that time there was hardly a single analysis text-book except a translation of the de la Valleé-Poussin course and Lebesgue's classical book on the integral. But not limited by the subject of the report, I posed a question on the conditions under which the upper Riemann integral and the lower Riemann integral coincide with the Lebesgue integral. It happened to give a definition of a semi-continuous function, some theorems on it, and the condition that a function has to be almost everywhere semi-continuous, respectively from above and from below.

These results naturally turned out to be well-known, just like another result obtained in passing—the characteristic of functions that can be oscillations of functions of one variable—Osc $f(x)$. This is any function that is upper semi-continuous. It turned out that this theorem had been proved a couple of years earlier by Livenson, coauthor of a number of my later papers. Its proof was quite short and our joint report on this theme was to have come out in the bulletin of the student scientific circle, but it was never published.

After one or two meetings the student circle broke up, but at that time Fikhtengol'ts began a seminar on descriptive function theory for the third and fourth years, which I began to attend. Participants of the seminar were Faddeev, Natanson, Sobolev, Mikhlin, and others.

The first subject of the seminar was the classification of Baire and Young functions. My first papers were concerned with these and I should like to indicate their origin.

[(1)]Kantorovich entered university when he was 14. As this needed special permission, he believed that he would not be accepted and got to know about his enrolment as a student from the following letter, received on 6 November 1926: "For the second time. To the student of Leningrad State University, Comrade L. Kantorovich. The directorate of the University recommends that you attend a commission on payment on 9 November 1926. Non-attendance at the appointed time will result in exclusion from the body of students at the university without appeal. At the same time we would like to notify you that payment must be sent not later than 9/11/26. Non-payment will carry the same consequences as default to the Commission's Deputy Student Department". (Note by V.L. Kantorovich).

It is known that at every point of non-differentiability four derivatives can be considered—generalized derivatives in Leningrad terminology, or derived numbers—nombres derivés. I posed myself the following general problem: to give a descriptive characterization of four functions that can be generalized derivatives of a continuous function. It was known that this is a Young function of the second class, and the natural condition, that the non-touching corners can occur in the form of exclusion, that is, at no more than countably many points (Young's theorem), must be fulfilled. However, analysis of this problem demanded definite preparation. In particular, since the upper and lower derivatives are limits of the corresponding relations, the problem arises of which Young functions of the second class can be upper and lower limits of a sequence of continuous functions.

This problem was posed for functions of any classes, including transfinite, and a solution was obtained. When the paper was ready for printing, the most recent volume XI of Fundamenta Math. came out, which featured papers by Stepanov and his student Gol'dovskii [1], [2] in which these questions were solved for Young functions of the second class.

Because of this, in the final preparation for publication the paper had to be redone to give the necessary references and to be limited to publishing only the theorems on functions of any classes with some supplementary corrections and with specializations for functions of the second class that generalized and covered the aforesaid results of the Moscow authors. This paper was published in vol. XIII of Fundamenta Math. [3] and presented at Professor Fikhtengol'ts' seminar.

With regard to the general problem, I obtained part of the results in 1928-1929. I even proposed to report on them at the All-Union Mathematical Congress at Kharkov, but two of my reports were already in the programme and, being a student, I considered it unethical to include a third. The paper was reduced to its final form in which it was later published—it came out in 1932 in Matematicheskii Sbornik [4]. The problem posed was not entirely solved: only partial results were obtained, but even so the methods of proof were quite interesting and subtle. In particular, I proved that for perfect sets of zero measure the four functions that satisfy Young's natural condition are the four derived Dini numbers, that is, there exists a continuous function on this set whose Dini numbers coincide with the said functions. Somewhat isolated stands §4 of this paper, where it is proved by simple methods that on any perfect set of zero measure two Young functions of corresponding type are the two-sided derivatives from above and below of a continuous function. Unfortunately, as it was published in its entirety only in Russian, it apparently remained unknown abroad, and as there were at that time a comparatively large number of papers relating to the descriptive characterization of sets of non-differentiability of functions, this paper was not continued, and right up to the present time there are only a few references to it.

Several other short papers come from the same period. I mention a paper on universal functions that contained the solution of two problems posed by Professor Fikhtengol'ts, namely whether for functions of Young's type and for functions of Baire's type, finite and infinite, there exists a universal function of the same type. A function of two variables is called universal for a given class if by specializing one of the variables of all the functions of this class are obtained. A universal function was successfully constructed for Young's classes, but it was proved that it is impossible ([5] – [7]) for Baire's classification.

I want to mention this paper, firstly because I gave my first report to the Leningrad Physico-Mathematical Society on it. The president of the society was the corresponding member of the Academy of Sciences Nikolai Maksimovich Gyunter. The fact is that it was intended to be published in the journal of the society, and following existing tradition I should have made a preliminary appearance with the report. This was in the spring of 1929. Curiously, at that meeting Sobolev's report "A remark on N.I. Staltykov's papers relating to first order partial differential equations" was second.

Secondly, this paper was later indirectly reflected in my applied papers on mathematical machines. Namely, in the 'function generator', whose invention is attributed roughly to 1948, a special switchboard was made, which was also called a "universal function". On this board ten functions were placed simultaneously (sine, logarithm, tangent, exponential, and others) and specializing the parameter allowed the function needed at a given moment to be obtained, so that the term "universal function" was itself given a totally appropriate use here. Of course, these papers are not actually related, but conceptually close.

Finally, in the third place, in this paper an abstract concept was used of an analytic operation on sets that touches on the next series of papers.

I recall in passing that after Aleksandrov's canvassing trip to Leningrad in 1929 with a series of lectures on topology, we started some work in this direction. Andrei Andreevich Markov started a topology circle where, by preference, set-theoretical topology was studied. Topology did not fascinate me, but by participating in the work of the circle I obtained a definite picture of it. For me what was important was that from then on I began my acquaintance and contact with the outstanding mathematician A.A. Markov (jr.), whose distinctive trait was an ability to evaluate the significance of new trends in mathematics and who tried to attract attention to them regardless of his own scientific interests.

Functions over sets, analytic (Aleksandrov) sets, and projective sets

It is well known that in the period primarily from 1915 to 1925 research on analytic sets (A-sets or Aleksandrov sets), discovered and studied by

Aleksandrov, Suslin, and Luzin, occupied a central place in the papers of many Moscow mathematicians (Kolmogorov, Novikov, and others). In Leningrad their work was almost unknown right up to 1927, when the All-Russian Mathematical Congress was convened. The work made a great impression on Professor Fikhtengol'ts, who was participating in it, and in 1928–1929 he started a seminar for the study of A-sets and related questions. Taking part in this work were Faddeev, Sobolev, Mikhlin, Faddeeva, Natanson, and a number of other mathematicians. Also attending the seminar were myself and Livenson, who went to university three years earlier than me, but because of circumstances had to interrupt his studies and was formally taken into my year, and graduated from university at the same time as I did.

In the seminar we studied journal articles, including some of Luzin's memoirs, and the appropriate parts of Hausdorff's book (second German edition, 1927), where an original monographical exposition of this theory was first given (Luzin's book [9] on these problems came out later, in 1930).

In Moscow this field had already begun to go out of fashion. Analytic sets had been studied quite thoroughly by then. However, there were almost no results on projective sets, although they also arose naturally. In particular, there was an interesting problem, afterwards featured in Luzin's book, on the interdependence of the two ways of broadening the concept of analytic sets. In one way, let us say the geometric, a set from the second class of projective sets is obtained as a projection of a set that is complementary to an analytic set. The other way—repeated application of the Aleksandrov A-operation—is to apply it to Borel sets, then to complements of analytic sets (CA-sets), then to their complement, and so on.

During the work of the seminar there were really two advances. Firstly, for Hausdorff δs-operations a question was posed on the study of the set of indices, that is, systems of sequences over which the summation of sets is carried out. It turned out to be useful to study them by means of a mapping onto the real line; it was then possible to consider them not as sets of sequences but as subsets of the set of rational numbers. Some theorems were obtained on how sets of indices are changed by the union of corresponding sets, by the composition of a complex operation, and so on. This work was still carried out during the first year of seminar classes by Livenson and myself. Incidentally, the idea of interpreting index sets as subsets of the set of irrational numbers was proposed by Faddeev, although he was not specially studying these questions.

Secondly, the question of the representation of projective sets was posed. For A-sets the beautiful representation through an A-operation with tuples was well-known, but nothing similar existed for projective sets. I obtained a representation for projective sets of the second class that allowed an extension. This paper was submitted through Academician Luzin to Comptes Rendus and published in 1929 [8].

Unfortunately, the seminar was not continued during the next academic year, but Livenson and I undertook intensive work during the autumn. In particular, the simple idea of a scheme was of real significance: the class of sets obtained as a result of a δs-operation over given sets (if the base of the δs-operation is represented by a set of real numbers) has a simple two-dimensional geometric picture from which various consequences can easily be derived.

We did not ourselves verify that from this group of theorems the solution of one of Luzin's problems directly follows: all sets of a Selivanovskii system, that is, those obtained by analytic means, are projective sets of no higher than the second class, or rather belong to the intersection of sets of the second class and the complementary class. We even decided, once this result had been obtained, that something or other was not correct, but thorough checking fully confirmed it. An easier problem on a Borel suspension over a system of A-sets and CA-sets was solved in passing, and it turned out that they are located at the intersection of sets of the second class and the complementary class.

Both of these problems were featured in Luzin's book that came out in 1930 in Paris in the Borel series [9] and in it there is a remark that, as Luzin knew from a letter of Fikhtengol'ts, they had been solved in our papers. The papers were briefly announced in the French Academy reports [10], [11], and then we undertook to write a long memoir. The first two parts were published in Fundamenta Math. [12], [13], but we did not manage to write the third part: either I was studying other problems or Livenson had some sort of complication. Incidentally, the third part was to have been devoted to projective sets themselves, and the results in it actually directly followed from the results of the first two parts.

In January 1930 at the invitation of Alekandrov and Kolmogorov we reported on this theme at the Moscow Mathematical Society. We were very warmly received and, if I am not mistaken, Privalov chaired and as always Stepanov asked many questions. After the report we were invited for tea with Kolmogorov and Aleksandrov, I'm not sure whose flat, probably Kolmogorov's on Trubnikovskii Street (there were few people and no tea). Here Kolmogorov informed us that he had an old manuscript from about 1921–1922 that he wanted to hand over to us to use and give a partial account of in our memoir.

This manuscript contained a definition of an analytic operation on sets, of Hausdorff δs-operation type, but which had apparently been discovered earlier than Hausdorff's (a small part of this paper was published in 1928 in Matematicheskii Sbornik [14]). We devoted one of the chapters of the second part of the memoir "On Kolmogorov R-operations" to this work. This was not a word-for-word reproduction—some of its theorems were given with other proofs, a number of additional theorems were proved, and a part of the manuscript was not reflected in our paper. Kolmogorov categorically forbade us to indicate the date when this manuscript was writte. Our mistake was that in our youth we did not include this paper in full and it first appeared in its entirety in Kolmogorov's collected works.

Since the theorems on projective sets were to have been presented in the third part of the memoir, which was not published, and they had only been mentioned in the introduction, in about 1936 Academician Casimir Kuratowski published the proofs of these theorems. This forced us to publish a short note in 1937 in Comptes Rendus [15] in which the full proofs of these theorems were given, each of which required only one paragraph and was supported by the published results in the memoir.

We also reported on these papers at the First All-Union Mathematical Congress, which was held in Kharkov in 1930. It was an important meeting for us, but the report did not go very well. The fact was that in parallel in another section Delone was giving a report on four-dimensional cinema and everyone went to this cinema, even specialists in set theory, although they excused themselves to us. It is difficult to compete with the cinema.

The congress was very interesting. I got to know (or at least met) such mathematicians as Hadamard, Montel, Blaschke, Fréchet, Denjoy, and also Russian mathematicians. Bernstein, whom I already knew, was President, and among the younger ones—Mark Grigor'evich Krein, Lev Semenovich Pontryagin, Aleksandr Osipovich Gel'fond, Mikhail Alekseevich Lavrent'ev, Mstislav Vsevolodovich Keldysh, who was still a third-year student, and myself were all pre-diploma.

In the translation of Hausdorff's book into Russian [16] in the editor's preface by Aleksandrov and Kolmogorov there was a reference to our papers and a short account of them. Consequently, as is well known, a number of more substantial studies in this area were carried out by Novikov, Motokiti Kondô, and also Lyapunov and other authors.

The constructive theory of functions

I did not study these questions a great deal, and likewise my results were not very remarkable, but all the same my creative work in this field had great significance for me in the future.

The work began by chance. While I was waiting for a student who was late, I was looking over vol. XIII of Fundamenta Math. and saw in it a note from the Moscow mathematician Khlodovskii relating to Bernstein polynomials. In it I first caught sight of the Bernstein polynomials, which he proposed in 1912 for an elementary proof of the well known Weierstrass theorem using probability arguments. I at once wondered whether it is not possible in these polynomials to change the values of the function at certain points into the more stable average values of the function in the corresponding interval.

It turned out that this was possible, and the polynomials could be written in such a form not only for a continuous function but also for any Lebesgue-summable function. I succeeded in showing that such polynomials, polynomials in Bernstein form as I called them, converge almost everywhere to the values of the generating function.

Without difficulty polynomials of this kind were constructed for functions of the first Baire class and their convergence everywhere was established, except for a set of points of the first category. Two corresponding notes were published in Doklady Akad. Nauk in 1930 ([17], [18]) and comprised the content of my second report to the All-Union Mathematical Congress. Also at this congress an almost obvious corollary was noted relating to the first theorem, that the sequence of Bernstein polynomials themselves for any absolutely continuous function admits termwise differentiation almost everywhere.

In another short paper [19], also using polynomials in Bernstein form, I proved the possibility of an analytic representation of a measurable function at all its points of approximate discontinuity. The paper [20], in which I solved the problem of how much an approximation of a continuous function by polynomials is worsened if it is required that the coefficients of these polynomials are integers, is also related to this collection of papers. This research was continued by Gel'fond in 1955 [21].

The most interesting of this collection is a paper on the convergence of Bernstein polynomials beyond the limits of a fundamental interval [22]. Namely, in it I proved that for an analytic function, and also for a piecewise-analytic function, convergence of the ordinary Bernstein polynomials holds in the underlying domain of holomorphy of the function. In the simplest case, for an analytic function, this is the largest ellipse with foci 0 and 1 in which the function remains regular. It is true that there is a gap in the proof of one of the theorems—at the proof-reading stage it seemed that I had hidden it and I did not put corrections in the printed text. Afterwards it became clear that it was not quite right, but it was already impossible for me to backtrack on it.

The convergence of Bernstein polynomials in a complex domain turned out to be unexpected even for Bernstein himself. He published several papers in which this research was continued and more subtle and accurate results were obtained ([23]–[25]).

For me it was important that because of these papers I became more deeply familiar with the constructive theory of functions, read Bernstein's fundamental memoir—his notable Doctor's thesis—the well-written book by de la Valleé-Poussin, and also Goncharov's course, which was quite good. My creative familiarity with the constructive theory of functions was afterwards used over and over again in different papers on pure and applied mathematics.

Finally, I should note that in Lorentz' small special monograph, published in the Federal Republic of Germany, some of my results were given, some were omitted, but they also had some inaccuracies, in particular, the theorem on termwise differentiability of a sequence of Bernstein polynomials was attributed to another person.

Approximate methods of analysis

At the end of the twenties and the beginning of the thirties, the network of higher education institutes was greatly widened in connection with the needs of industrialization. The intake of students and the number of higher education institutes increased tenfold. Many institutes were divided up. Thus, nine industrial institutes were formed from the Leningrad Polytechnic Institute. This demanded a large number of lecturers, including mathematicians. For example, initially Kuz'min headed the departments of mathematics at all the nine institutes that were divided off from the Polytechnic. Therefore on finishing university I received seven different offers, as did my comrades.

The number of graduates then was not very large: our group consisted of ten people, and previously it had been seven. True, among these were Sobolev, Khristianovich, Mikhlin, Devison, Goarik Ambartsumyan (sister of V.A. Ambartsumyan), and Zamyatina (Faddeeva).

While a post-graduate student at the university I worked at the Construction Institute at the same time, in the first year as an assistant, in the second as a research fellow, and in the third, which coincided with my finishing early as a postgraduate, as Professor. In 1932 I was simultaneously elected to the post of Professor and Head of the Department of the Institute of Industrial Transport.

I shall not list all the various anecdotal incidents that happened to me at that time. Once, arriving at a lecture for the new intake, I went to the department to an audience that had still not quietened down. At once a pair of students began to pinch me—"Sit down in your place! Look, the Professor is coming now". Often, however, the anecdotal incidents that happened to young lecturers at that time were retold, as those that happened to me were.

The Construction Institute, when it was separated from the Polytechnic, had a reasonably strong teaching staff. It was headed by a well known specialist in elasticity theory and constructional mechanics, Academician Galërkin, who had a whole series of talented young students. We began to establish scientific contacts. This forced me to get to know some applied topics and, in turn, the post-graduate students and young lecturers in the technical departments began to make wide use of new numerical methods, including some of my own, in their work.

My acquaintance with Krylov, a very vivid figure, goes back to this time. A shipbuilder and mathematician, although of a more classical nature, at Smirnov's suggestion he often submitted my papers to Doklady Akad. Nauk.

Until the 1929 elections he was the only mathematician in the Academy of Sciences—Lyapunov, Markov, and Steklov had long since died and Uspenskii had emigrated.

I rewrote his book "The theory of beams lying on an elastic foundation" [26] in terms of ordinary improper Stieltjes integrals and Stieltjes integrals of higher order [27]. Several papers on the Stieltjes integral are connected with this and are interesting in the fact that they were already related to the theory of generalized functions or the theory of distributions, even though of finite order [28], [29]. Gyunter had stimulated interest in these questions. He gave a long course that was then published as a five hundred page memoir on the Stieltjes integral [30], and Sobolev and myself were his main audience. Later the theory of generalized functions attracted my attention.

Simultaneously with the expansion of the higher education network many research and applied institutes were opened, which also attracted mathematicians. Most of them were interested not in theoretical knowledge nor in the mathematical study of classical problems but in forming and developing methods that allowed calculations to be carried out effectively that were needed in the planning of large-scale objectives—power turbines, aeroplanes, complex building tools. Therefore problems of approximate methods of analysis attracted more and more attention.

It should be said that whereas approximate methods for solving ordinary differential equations had been well developed from the time of Gauss and Euler, there were only isolated papers (Runge, Nystrom, Ritz) on approximate methods of higher analysis—partial differential equations and integrals and functional equations.

Because of this a special seminar on approximate methods of higher analysis was opened by Smirnov in 1931. Besides Krylov and myself, Goluzin and Melent'ev also took part.

At the same time Krylov and myself were working on a course of calculus of variations established under Smirnov's supervision from the lectures he gave to physics students, from Chebotarëv's lectures, and from the literature. This course [31] was published by KUBUCh (the Commission for the Improvement of Students' Living Conditions) in 1933, and for some time was used as the basic text-book, and mathematicians of the older generation will remember that they studied the calculus of variations from it.

Smirnov's seminar began by familiarizing us with a few papers on approximate methods—papers on Ritz's method for difference equations, and Nystrom's papers on integral equations. Soon, however, the seminar's participants began independent work.

In 1932 I proposed a new variational method [32] that was an essential generalization and modification of Ritz's method. In El'sgol'ts' book "Calculus of variations" [33] a separate paragraph entitled "Kantorovich's method" was devoted to this method. In Ritz's method the solution is found in the form of a linear combination of some a priori given functions with undetermined coefficients that are found from the condition of minimalization of some integral in such a way that the solution of the partial differential equation or the problem on the extremum of a double integral is reduced to a matrix problem. In the method that I had proposed the form of the solution that is found included arbitrary functions of one variable, thanks to which part of the structure of the approximate solution is taken in the given form, and part is determined from the problem itself—the minimization of the integral becoming a one-dimensional problem. This method of numerical solution and of the qualitative study of the problem turned out to be particularly effective for restricted domains and in many cases allowed not only a better numerical solution to be found but also an approximate analytic solution of the initial two-dimensional problem.

This idea was also applied to the creation of a method, analogous to the Galërkin-Bubnov method, that was based not on variational principles but on the solution of moment equations [34]. This very idea was later used by Dorodnitsin [35], and also in the papers of his students Belotserkovskii [36], [37], Chushkin [38], and others.

The proposed methods at once began to be used at the Construction Institute where I taught, and were also used abroad [39]. The dissemination of our variational method was substantially promoted by my report, given on Lur'e's initiative, to a meeting of the Mechanics Society, in 1935 I think. Lur'e himself proposed interesting applications and modifications of the method [40]. It was also used in a number of his students' papers, in particular in the papers of Arutyunyan [41], [42], who subsequently became President of the Praesidium of the Armenian Supreme Soviet.

Another of my papers was devoted to questions of an approximate conformal mapping of a disc onto a domain close to it, or more generally of a domain onto a domain close to it ([43], [44]). The idea of the method consisted in the fact that a mapping function is found in the form of a series in powers of a small parameter, where the terms of the series are found either from an infinite system of linear equations or by successive approximations. The need for such a method was determined by the applications of the methods of the theory of functions of a complex variable in hydrodynamics calculations that were spreading at this time (Lavrent'ev and Golubev). In particular, they were widely applied at the Central Aerodynamics and Hydrodynamics Institute, so that the proposed method of finding mapping functions had a great applied significance. For example, Lavrent'ev highly valued this paper.

A strict proof was given of the convergence of this method at definite boundaries of parameter change. This result, and also its proof, was used in Golubin's well known collection of papers on the theory of one-sheeted functions (incidentally only the first paper had the appropriate reference, and afterwards he referred only to his own paper [45], see also [113], 323).

A paper on approximate conformal mapping, still awaiting publication in Matematicheskii Sbornik, was included by Smirnov as a separate section—"The method of conjugate trigonometric series"—in the third volume of his classical textbook "A course in higher mathematics", which came out in 1933. I made an attempt to extend this method to multiply-connected domains, but the relevant paper was rather carelessly written and needed radical reworking. It was published [46], but in my paper in 1937 I showed that it could not be verified [47]. I continued to study the method of approximate conformal mapping later and a number of my post-graduate students' articles have been related to this.

At the time of Jacques Hadamard's visit to Leningrad, in 1933 I think, at a meeting with him in the office of Smirnov, the Rector of the university, a short report was made on some of the achievements of Leningrad mathematicians, including a discussion of the paper on approximate conformal mapping. The paper interested Hadamard, and he jokingly expressed the fear that the same fate would befall Kantorovich as Galois. In reply someone said that I did not have such an aggressive character.

I move on to my book with Krylov on "Approximate methods for solving partial differential equations" [48]. I should like to mention a curious case when a systematic plan played a positive role. In the prospectus put out by the "Publishing House of General Technical Literature and Nomography", as well as the list of actual books proposed for publication, a number of themes were indicated in which the publisher was prepared to publish books but did not have any in their portfolio. This included approximate solution of partial differential equations. This made the formalities relating to the conclusion of a contract and approval of a manuscript noticeably easier, but it also had a definite psychological significance for us. I must say that until then there was no suitable book of a general character in the world's scientific literature. In this work, where even the structure was not clear to us at first, a bibliography of journal articles on these questions was given as well as the actual advanced development.

While we were working on the book, new research (on errors and convergence) was being undertaken, and a number of new methods appeared, so that many results were first published only in this book. Thus, in the first chapter, devoted to Fourier's method, a method of solution was developed based on the reduction of a boundary problem to an infinite system of linear equations, and a largely original theory of infinite systems was presented. Here also methods directly similar to this were given for improving the convergence of series that are obtained in the approximate solution of boundary problems—a question to which I returned many times later on [49].

The second chapter, on approximate solution of integral Fredholm equations, contained error estimates, given for the first time in practice, that arise as a result of replacing the integral equation by a system of linear equations. Methods were put forward for successive approximation and analytic continuation, application of an integral equation to the solution of Dirichlet's problem, and also the English mathematician Bateman's method was presented. This was perhaps the first systematic account of approximate methods for solving integral equations.

The chapter on the lattice-point method was based on available literature and was perhaps insufficiently developed. In the chapter on variational methods a necessary theory of equations of elliptic type and the variational problems related to these was presented, the classical Ritz method. A special section was devoted to the new method of reduction to ordinary differential equations that I have already talked about.

A large part of the book was concerned with methods of approximate conformal mapping. Here, alongside Bieberbach's method and the method of orthogonal polynomials that is analogous to it, considerable attention was given in the account to the method of expansion in series in powers of a small parameter that I have already talked about, and also to an analogous method developed by Krylov for the case of mapping a domain onto a disc. We also presented the interesting method of Melent'ev, a mathematician and inventor, that combines graphical and analytic methods. A separate section was devoted to the problem of mapping a half-plane onto a domain bounded by a polygon. The Christoffel-Schwartz classical formula theoretically gives an accurate solution of this problem, but actually finding the parameter presents considerable difficulties. Here the idea of applying Newton's method, worked out in Stenin's dissertation [50], is useful. In the last chapter methods of approximate conformal transformation are applied to the solution of fundamental problems (Cauchy, Neumann, Dirichlet, and others) for canonical domains.

In the book a number of other methods are given that are of some interest. The method of collocation (coincidence) is presented for partial differential equations when the initial equation is replaced by a system of equations that relate to separate curves, that is, it is required that the approximate solution satisfies the equation only on some curves [51]. Strictly, Nystrom's method for integral equations has such a character. It was proposed for ordinary differential equations in Duncan's paper that came out in 1937 [52]. Our work was written up in 1934, so the question of priority remains unclear. It seems to me that this method is of some interest.

Another method was based on the fact that an approximate solution of a partial differential equation with a boundary problem of elliptic type was found as a function that has various analytic specifications in different parts of the domain with the requirement that continuity and smoothness are preserved. The spline function method that appeared much later is reminiscent of this method. The interconnection of these methods was studied not long ago by a mathematician from Kharkov, Ukrainian Academician Rvachev.

To conclude the survey of papers on numerical methods I note that I also wrote some elementary papers that were concerned with classical approximate analysis. Thus, in 1934 I published a short note on the calculation of definite integrals and certain methods of dividing singularities [53], where I proposed to divide the singular, irregular part from the function and apply one of the quadrature formulae to the comparatively regular remaining part. As such, this note looks really elementary, so that it even surprised one of my mathematician friends Vallander: "What's this, you, Leonid Vital'evich, sending such elementary material on calculating simple integrals to Matematicheskii Sbornik?".

However, the application of this idea to an integral equation is by no means obvious, and the fact that a non-smooth equation is replaced by a smooth one has great significance. For example, in calculations in the applied papers for which I was awarded the State Prize in 1949, the use of this method for solving "transfer" equations gave not only a large gain in the dimension of the system being solved. Parallel calculations were still being carried out in one authoritative office for forty quadrature points; we had only applied it in all to two or three nodes, and the result obtained was more accurate and qualitatively different and so had a fundamental significance. In this area of physics this "elementary" method was used for many years after our paper.

A note on a method of calculating integrals for even and odd functions that allows twice as many nodes to be preserved [54] is also related to these elementary papers.

At the beginning of 1935, after having actually written the book on approximate methods, I obtained an interesting, honorary invitation from Kolmogorov the Director of the Institute of Mathematics and Mechanics of Moscow State University. He proposed a month's scientific programme and contacts with Moscow mathematicians. The programme, in particular, proposed giving a short special course. As the theme I chose approximate methods for solving partial differential equations. I should say that whereas in Leningrad applied mathematics was by tradition used to attention and respect, in Moscow at that time things were different (though in Kiev interesting research was then being carried out by Krylov and Bogolyubov). A characteristic conversation took place between myself and Pontryagin, with whom I was on very good terms at that time. On the way to my lecture, before which he had to present me to the audience, Pontryagin asked "Why have you, Leonid, been concerned with calculations, when you have had such interesting theoretical papers?" And he added that in Moscow they only took the weakest students of the year for calculation. I replied "You do it your way, and I'll do it mine". None the less, after presenting me he remained until the end of the lecture on infinite systems of linear equations and listened to it very attentively, which made a very pleasing impression on me.

Perhaps my lectures to some degree helped to arouse the interest of Moscow mathematicians in these problems. They seemed to underestimate the theoretical difficulty of studying approximate methods. And in fact the majority of papers at that time consisted of a description of a proposed algorithm and an experimental test of its application. Among them, for instance, the convergence of Galërkin's method for the case of a one-dimensional differential equation, established by Keldysh in a paper [56] in 1942 after a preliminary paper of Academician Petrov [55], is considered to be one of his brilliant achievements. And even now there remain a number of non-simple unsolved problems, for example the question of the range of values of the parameter in a conformal mapping problem in which the convergence of the approximate solution holds.

During my stay at Bolshevo at the rest home of the Commission for Assistance to Scientists I made many personal contacts with mathematicians, and Gel'fond introduced me to many physicists—Tamm, Mendel'shtam, and Kapitza who had just arrived in the USSR. I also visited the Lavrent'evs. In Bolshevo an unpleasant incident befell me. Coming down from the hills to Klyaz'ma on skis I fell into a hole in the ice and half sank into the water. They helped to pull me out and get me to the rest home, where they put me to bed straight away and, to the envy of the others, they started drinking port wine. I did not want to upset my mother and didn't tell her about this episode. However, it turned out that from the letters of Muscovites to Leningrad she unfortunately learned about this.

I remember with delight another earlier journey to Moscow. If I am not mistaken in 1933 or early in 1934 a conference of young scientists was taking place in Moscow to which Sobolev and I went of the mathematicians, and also the physiologist Asratyan, the economist Mileikovskii, and the geologist Markov. Sobolev talked about a series of his papers on the application of functional equations to seismological problems. I gave a short report devoted to the generalization of Parseval's formula to the continuous case—the corresponding note was published at that time in Compositio Math. [57]. If a family of kernels that depends on a parameter is a complete family, that is, there are none that are orthogonal to it, then a formula is true that is outwardly analogous to Parseval's formula—the sum of the squares of certain integral means coincides with the integral square of the function. This fact is interesting not only as a generalization of the well-known formula but also by its method of proof, in which probability arguments play an essential role.

At the conference I first met Gel'fand, who became a post-graduate student of Kolmogorov's before completing university. Although at that time it appeared that only two small notes of Gel'fand's had been published in the newspaper that was issued for this conference, including his well-known lemma on weak convergence, talks with him were extremely interesting for me. At the time in Moscow a great deal was known about functional analysis as Plesner was working there, and lectures were given systematically—in Leningrad we had only just begun to study it. Our talks with Gel'fand did not touch upon just this subject but also many others. His completely different understanding astonished me, while on the other hand there were some questions that I knew well and that I was even studying, for instance the approximation of functions. The impression was that I and those leaders whom I had studied saw the subject from the outside, but Gel'fand was talking from somewhere inside. My relations with him, sometimes more intensive, sometimes less so, have continued to the present day.

The theory of partially ordered spaces

The twenties and beginning of the thirties were marked by the rapid development of a new branch of mathematical analysis—functional analysis. After Hilbert's classical papers there followed fundamental research by Riesz (1918) and Banach (1922/1923). It was clear that this field was gaining ever increasing popularity. Of course, there were the sceptics who did not appreciate its significance, thinking that this was just a way of retelling the well-known facts of mathematical analysis in another more general language.

The appearance in 1927 of a book by von Neumann "The mathematical foundations of quantum mechanics" had great significance. In it functional analysis (Hilbert spaces and so on) was successfully applied to the mathematical formulation of new, complex physical theories that had appeared not long before this in the ideas of Dirac and others. Dirac had already received a Nobel Prize in 1933 for his work in quantum mechanics.

We in Leningrad knew little of papers on functional analysis. Smirnov, who had exceptional erudition and rapid orientation to new areas, was the first to realize the significance of this trend. As he gave lectures to physicists, unlike many other mathematicians, he found out about the latest work in theoretical physics.

In 1933 at the university a large scientific seminar was set up on functional analysis, in which not only those active in the theory of functions participated, but also other mathematicians who were working in Leningrad then. I recollect that at the first meeting there were Gyunter, Kuz'min, and Mikhlin. Alongside the titled (Markov (junior), Tartakovskii, Yanchevskii) also taking part in the seminar were a number of young mathematicians, post-graduates, and students (Vulikh, Gavurin, Lozinskii).

The work of the seminar at first had a purely academic character—Smirnov gave a number of introductory lectures, and I gave several reports. We systematically studied Banach's book "The theory of linear operations" and separate important memoirs were reported. Several meetings were devoted to Fantappiè's theory. Extraordinary range and breadth were characteristic of functional analysis: this science arose from various problems—the theory of functions, mathematical physics, probability theory, and so on. It was natural that some papers had a random character and were not given further development.

Almost at once participants of the seminar began independent work that touched upon well-known trends. Thus, Fikhtengol'ts and I were given the theorem on the general form of a linear functional in the space of all measurable bounded functions, where Radon's integral is used. The problem of the cardinality of a set of functionals in this space, which is directly related to functional analysis, was solved in passing and turned out to be $2^{2^{\aleph_0}}$. The paper contained a number of other propositions, for instance, on weak convergence in this space. It was sent to Studia Math. and published in Vol. 5 (1935) and in Doklady Akad. Nauk [58], [59]. However, part of this work was already known to specialists in Warsaw. There appeared a response to the paper by Hausdorff who, in place of our quite cumbersome transfinite construction in the proof of the theorem on the cardinality of a set of functionals, proposed a much shorter proof for the lemma upon which it rested that took all of half a side.

My next paper was concerned with the problem of the extension of functionals as a characteristic of a Hilbert space [60]. Namely, a certain class of function spaces is established in it in which, as in a Hilbert space, it is possible to extend all functionals from a subspace onto the whole space preserving additivity. As was noted in the review in Zentralblatt, there are no such spaces other than the Hilbert space. However, this theorem can be turned round and considered in such a way that one more property is thereby established that divides a Hilbert space from the class of all normed spaces, as Kutateladze does [61].

Questions related to the theory of generalized functions, developed here by Sobolev and later the Schwartz and Gel'fand schools, bordered on functional analysis. I even noticed my own interest in this area, as it seemed to me important both for analysis and functional analysis, and I produced several papers on this. I outlined some approaches to this problem and the way for further work different from those mentioned. In 1935 two notes [62], [63] were published on methods of extending general and particular Hilbert spaces. A third note, in which I wanted to relate this to applications to analysis, was not completed and was not published. To the present day I still do not know how much this path was workable and fruitful.

The fact is that just at this time I was interested in another theme, a new set of problems. To some extent the choice of themes was influenced by external circumstances. Several years before this Fikhtengol'ts, Natanson, and I were planning our work for the Leningrad course on the theory of functions of a real variable. I was the one who lagged behind, I had to do the descriptive theory. Beginning this work in Teberda, in one of the popular resorts of the Committee for Assistance to Scientists, I reflected for a long time on how exactly to introduce a description. To introduce it in the usual way for real functions was already somewhat old-fashioned, and it was still not understood how to do it from the more general point of view of functional analysis. It is already clear from the account of my work what role ordering played in the descriptive theory—but it was absent in the known function spaces.

Then an idea arose for enriching the apparatus of functional analysis—the introduction of spaces on which an order relation is defined. It even seemed strange to me that this very important property of the majority of mathematical objects had not up to that time been reflected in functional analysis. Partially ordered spaces had been constructed in which an order relation is defined with known properties for some sufficiently rich collection of their elements.

My note on linear partially ordered spaces [64] had already been published in the Doklady Akad. Nauk of 1935. I even thought about including a supplementary report at the forthcoming conference on topology in Moscow in September, but apparently from bathing in the cold mountain lakes of Teberda I became quite seriously ill(1). Although I attended the conference itself, I was not strong enough to prepare a contribution at an appropriate level. Despite the fact that I was not in full health, I was so struck by this theme that in 1935-1936 a whole series of my notes appeared in Dokl. Akad. Nauk and Comptes Rendus devoted to this new theme. I also made several reports at a seminar on functional analysis.

The first axiomatic treatment of partially ordered spaces looked very cumbersome. It appears that it was November 1935 when I succeeded in constructing a quite simple and beautiful axiomatic treatment [65], and I presented a report to the Moscow Mathematical Society. Kolmogorov recounted his perceptions of the report to me—at first the examples that were being considered did not seem interesting to him, but when the idea came into his head that a space of functions of bounded variation can be regarded as a partially ordered space, regarding a monotonically increasing function as positive, he began to understand the diversity of these objects. He had naturally revealed this example in his own account before I went up to him.

The theory of partially ordered spaces presented me with a whole trend in functional analysis that was new and promising. I studied it myself in later years and drew the attention of my students—Vulikh, Pinsker, and others—to this work. It turned out that this area began to be intensively

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(1)Kantorovich related that when he was ill for a long time he was diagnosed as having intestinal tuberculosis, at that time incurable. Despite this, he worked extremely intensively, hoping to succeed in advancing his research as much as possible. He talked about this during his last illness, persuading us to tell him the real diagnosis, and knowing it conducted himself with great fortitude, worked hard, and was always benevolent. (Note by V.L. Kantorovich) developed in the West, where these questions appeared in papers by Birkhoff, Freudenthal, and von Neumann. In the Soviet Union somewhat later Krein and his school developed a theory of spaces with cones of positive elements that was similar to this. However, the class of linear partially ordered spaces was, apparently, first introduced by me. To some extent Riesz came close to it in his report at the Bologna congress [66] but, in essence, he did not have a general theory of such spaces. Spaces of functionals with these properties had been studied. Later in 1940 in Birkhoff's book "Lattice theory" the theory of partially ordered spaces was linked to my name and a separate chapter was devoted to it [67].

I prepared a number of memoirs that were in part published later, and in 1936 I was already able to give a special course on "Functional analysis on the basis of the theory of partially ordered spaces". Some of my first audience were my future co-authors Vulikh, Pinsker, and also Gavurin, Shanin and others. Some of the results were presented in memoirs, some were only announced in notes, but it was quite obvious that this new point of view, defined by the inclusion of ordering in the study of objects of functional analysis, significantly enriched and diversified them. Even some facts of the theory of functions of a real variable were thus rediscovered, for example, Fréchet's theorem and Vitali's theorem. Some new theorems were obtained—I don't know if the theorem had been proved earlier that any set of bounded measurable functions has a least upper bound, that is, there exists a function that is greater than every function in this set at all points (other than perhaps a set of points of zero measure) and moreover is the least of these functions.

One of the trends that attracted my attention was the analytic representation of linear operations that transform one space into another. In this case three classes of such operations are singled out in view of the presence of two types of convergence in every partially ordered space—*o*-convergence and *t*-convergence. The main part of these papers I completed together with Vulikh [68], [69]. As is known, questions on the representation of operations in Banach spaces were the subject of Gel'fand's first papers [70], in which he proposed fresh approaches and obtained a number of subtle results. He and I had had definite contacts on these questions and our development partly overlapped. A profound theorem on the extension of spaces, related to the concept of the disjunction of elements, was obtained by Pinsker [71]. This paper, though with different terminology, is in part related to work by Japanese mathematicians and also to Krein's research on groups.

Research on functional equations in partially ordered spaces is also of interest. The fact is that the principle of comparison and the principle of majorants, widely applied in classical analysis, had not been reflected up to this time in functional analysis. In the language of partially ordered spaces it was quite meaningful and natural to formulate the principle of majorants and some general theorems on majorization. In this case yet another generalization had to be found—along with ordinary normed spaces, spaces that are normed by elements of partially ordered spaces had to be brought into consideration (for example, for Banach spaces the spaces B_K). Since the elements of a partially ordered space are in the main very near to the concept of a number in their properties, they, just like real numbers, can be used as a norm. This gave the general and useful principle of majorization which, in particular, I applied to some problems on numerical methods that I was speaking about earlier, for example, in the theory of infinite systems of linear equations, and so on. Here was a source for obtaining new results on convergence, and on estimates of an approximate solution and its characteristics that are obtained by means of a majorizing equation. This paper was published here [72], and also at the suggestion of Carleman, who arrived in the USSR in 1938, was sent to Acta Math. and published there in 1939 [73], so that it became quite well known.

My course, though it is true it was not very well designed (I had an idea of publishing a monograph on these questions but then I was not able to do it), was presented in the form of a manuscript in the competition that was taking place in Leningrad just at this time for the work of young scientists and it was awarded a general (over all specialities) first prize. The following year a similar All-Union competition took place, and this paper received a first prize in mathematics alongside papers by Sobolev, Aleksandrov, and Pontryagin. In 1938 in the Leningrad Philharmonic Hall a celebratory meeting was held for the results of the competition, where Academician Orbeli handed me my diploma and I gave a short report.

Linear programming

It surprises many people how it suddenly happened that I became an economist. It must be said that I had always had some interest in economics and economic solutions. For example, I attended the lectures on political economics that Voznesenskii gave to us in the third year with great interest. Voznesenskii was later Rector of the university and brother of a well-known economist who was President of the State Planning Committee (Gosplan). I often went to him with questions after the lectures.

I even happened to work as an economist. After the third year—summer 1929—we had to go on work-experience. For the mathematicians work-experience consisted in calculating numbers from one to ten—cloudiness in the geophysics observatory or accounts in a savings bank. I found a uniquely suitable place—work as a statistician at Tashkent in the directorate of the Central Asian Water Board (a large department that was studying the design and construction of systems of irrigation in the whole of Central Asia).

The statistician post did not work out, and I was taken on as a junior economist. Interestingly, it turned out that my supervisor was Maria Spiridonova(1), who worked there.

For half the time I studied economic materials, the description of irrigation conditions, the use of water resources and their distribution. I worked on the Chu-Talasska water system that went through two republics—Kirgiziya and Uzbekistan. The other half of my time was for my own studies—I wrote some sections of my memoir with Livenson on analytic and projective sets, which were often used later on.

However, this was a very quick episode. As I said, for a choice of topics for my studies, along with my intrinsic interests and mathematical aspirations, external factors and the general situation also had a definite influence. In 1936–1937, when I finished my work on partially ordered spaces, I felt some dissatisfaction with mathematics. Not because my work was uninteresting or unsuccessful, but because the world was facing a strange menacing brown plague—German fascism. It was clear that there was going to be a very hard war for some years that would threaten civilization. And I felt a responsibility, understanding that people out of the ordinary must do something.

Of course, my work in pure and applied mathematics found a use, particularly in special themes. At the same time I had a clear perception that the state of economic solutions was a weak spot that was reducing our industrial and economic power. This was not only my evaluation. In a basic report at the 18th Party Congress, devoted to questions of perfecting the management of enterprises, it was said that output of production could grow by 20–25% only from the elimination of the phenomenon of rush work. And this was far from being a unique deficiency indicating imperfection in the management of economics.

Just at this time a new Constitution and elections were introduced, and the Supreme Soviet was formed. There was a somewhat more democratic situation for the discussion of economic problems and I thought about these problems more and more often, it is true more at a dilettante level than scientific, but still using my general intuition. For example, at the Supreme Soviet I wrote a note about the ridiculous situation in the book trade, with the circulation system and prices that lead to an unwarrented book deficit, to speculation, and to a great loss to the state and people.

Before the 18th Party congress an opening discussion took place, similar to the one now taking place before the 27th. It was possible to write in suggestions and observations, and so I sent them an article on the extreme distortion of the pricing system because it does not reflect reserves, and about the loss that arises as a result of this. It was not printed, but I received an answer from the Gosplan Prices Bureau that they had read the

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(1)M.A. Spiridonova (1844–1941), see Great Soviet Encylopaedia, Vol. 24, 425.

article, but my suggestions did not take into account the existing way of determining prices and the politics of prices. In short, I was sent a standard letter.

However, to some extent even a chance factor indirectly helped my transition to economics. Being a professor at the university I was also Head of the Department of Mathematics at the Institute of Mathematics and Mechanics of Leningrad State University (Smirnov was Director) and in connection with this carried out some administrative duties. Once some engineers from the veneer trust laboratory came to me for consultation with a quite skilful presentation of their problems. Different productivity is obtained for veneer-cutting machines for different types of materials; linked to this the output of production of this group of machines depended, it would seem, on the chance factor of which group of raw materials such machines were meant for. How could this fact be used rationally?

This question interested me, but nevertheless appeared to be quite particular and elementary, so I did not begin to study it by giving up everything else. I put this question for discussion at a meeting of the mathematics department, where there were such great specialists as Gyunter, Smirnov, Kuz'min, and Tartakovskii. Everyone listened but no-one proposed a solution; they had already turned to someone earlier in individual order, apparently to Kuz'min. However, this question nevertheless kept me in suspense. This was the year of my marriage, so I was also distracted by this. In the summer or after the vacation urgent, to some extent economic-like, engineering and economic situations started to come into my head, that also required the solving of a maximization problem in the presence of a series of linear constraints.

In the simplest case of one or two variables such problems are easily solved—by going through all the possible extreme points and choosing the best. But, let us say in the veneer trust problem for five machines and eight types of materials such a solution would already have required about a million systems of linear equations and it was evident that this was not a realistic method. I constructed partial methods for solving individual types of such problems and geometrical methods and was probably the first to report on this problem in 1938 at the October session of the Herzen Institute, where in the main a number of problems were posed with some ideas for their solution(1).

The universality of this class of problems, in conjunction with their difficulty, made me study them seriously and bring in my mathematical knowledge, in particular, some ideas from functional analysis.

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(1)In Kantorovich's archives a manuscript from 1938 is preserved on "Some mathematical problems of the economics of industry, agriculture, and transport" that in content, apparently, corresponds to this report and where, in essence, the simplex method for the machine problem is described. (Note by V.L. Kantorovich)

The question concerns the class of extremum problems in which an extremum point lies on the boundary of the domain under consideration. Such problems are characteristic of economics. A certain economic process can be described by two vectors: $x \in X$, the results of the process, and $y \in Y$, the resources used, where X and Y are linear spaces.

Consider the set of realizable processes T. To each value of the parameter $t \in T$ there corresponds a process that can be characterized by its cost and result (x_t, y_t). The set $\{(x_t, y_t) : t \in T\}$ is assumed to be convex, that is, together with every pair of processes it also contains their average. The point (x_0, y_0) is called an extremal state of the process if the intersection of the cone of positive elements with vertex at this point and the set $\{(x_t, y_t);\ t \in T\}$ is empty. In economic terms this means that there does not exist a version of the process in which the results would be greater $(x \geqslant x_0)$ and the cost less $(y \leqslant y_0)$.

To each extremal state there corresponds some linear functional that reaches an extremum at this point—a plane of support that divides this point from the internal ones. If a normalized equation of this hyperplane is written down, then to each point x, y there correspond certain numbers ξ, η for which $\xi x + \eta y = 0$.

Thus, to each extremal point x_0, y_0 there corresponds a system of numbers ξ_0, η_0 dual to it, which can be called the estimates of the separate coordinates in the spaces X and Y. The extreme points (or extremal states) are fully characterized by these estimates—each form of finite results and each form of cost of resources has its corresponding estimate. The existence for a given version of the process of such a system of estimates is evidence of its extremality, its unimprovability in the given conditions, that is, for the given resources and assigned finite results. The algorithm for finding the extremal state of a process also follows from this—if we have some version, we try to determine these estimates for it, and they are not found, then naturally a way is indicated, the direction of the displacement to internal points that in some sense gives a gradient direction for transition to the best state and thus by successive improvements to move to the required solution.

I have described the general problem. We mention two important particular cases, when the mapping $t \to (x_t, y_t)$ is linear. If the extremal state has to be found for given resources and an assigned assortment of finite production, that is, it has to be determined on a ray going from the surface of the given resources to an extreme point, then this is the so-called basic production problem. If for a given finite assignment a result is required with least waste of resources, in vector form or in the form of linear combinations of them, then this is the basic linear programming problem.

As I recall, in January 1939 I constructed a method of pivot factors, in which the solution of the system itself is changed in a certain sense, and is united with the problem of finding some factors and estimates that correspond to each form of production. The method formed was different to the close unification of the processes for solving the direct and dual problems; it was similar to the algorithm that Dantzig, Ford, and Fulkerson worked out much later [74].

What became clear was both the solubility of these problems and the great expansion of them, so representatives of industry were invited to a discussion of my report at the university. This report formed the main content of the pamphlet "Mathematical methods in the organization and planning of production" [75]. I gave an analogous report concerned specially with the construction industry at the Construction Institute. The university immediately published my pamphlet, and it was sent to fifty People's Commissars. It was distributed only in the Soviet Union, since in the days just before the start of the World War it came out in an edition of one thousand copies in all.

The number of responses was not very large. There was quite an interesting review from the People's Commissariat of Lines of Communication, in which some optimization problems directed at decreasing the mileage of wagons were considered, and a good summary was given in a pamphlet in the journal "The timber industry".

At the beginning of 1940 I published a purely mathematical version of this work in Doklady Akad. Nauk [76], expressed in terms of functional analysis and algebra. However, I did not even put in it a reference to my published pamphlet—taking into account the circumstances I did not want my practical work to be used outside the country.

In the spring of 1939 I gave some more reports—at the Polytechnic Institute and the House of Scientists, but several times met with the objection that the work used mathematical methods, and in the West the mathematical school in economics was an anti-Marxist school and mathematics in economics was a means for apologists of capitalism. This forced me when writing a pamphlet to avoid the term "economic" as much as possible and talk about the organization and planning of production; the role and meaning of pivot factors could be given somewhere in the margin of the second appendix and in the language of Aesop.

In my book in 1939 a whole number of problems were described—the distribution of production, distribution of work, rational cutting-out, some transport problems, and so on, that is, practically all problems of linear programming at its lowest level. In his book "Linear programming and extensions" [77] Dantzig noted that I had described almost all the range of applications of linear programming that were known in the USA up to 1960. Such a limitation to problems of the lowest level was partly connected with those quite dangerous objections that I have already mentioned.

At the same time, by developing models and interpreting them in more large-scale planning problems I began to understand the significance of these models for developing the principles of pricing, estimating effectiveness, at all events, the effectiveness of investment, that is, the basic features of the theory of linear economics for a socialist economy were created and reflected in mathematical language.

To some extent problems of non-linear programming were also touched on, but my attention was concentrated on the discovery of concepts and quantitative relationships of the basic characteristics of socialist economics, although there were evident possibilities of applying the apparatus developed to mathematical problems as well, for example, to the approximation of functions, but I considered that this was now of secondary importance.

At that time I wrote a number of articles and manuscripts on this theme that remain unpublished. I gave a report in Moscow, at the Mathematical Institute, on one series of applied questions, and the report aroused great interest. In particular, in deciding the question of the formation of the Leningrad Branch of the Mathematical Institute (LOMI) the existence of this complex of applied questions played a definite role—LOMI was opened in March 1940.

The first of the papers in this field that I completed at LOMI was a paper on a transport problem with Gavurin, who had not long before defended his Ph.D. thesis. This problem arose by itself with us, but we soon found out that railway workers were already studying the problem of planning haulage on railways, applied to questions of driving without a load and transport of heavy lorries. There was Tolstoi's pamphlet on this theme [78], and there was several attempts to introduce this work by the appropriate department of the People's Commissar of Lines of Communication. However, there was neither a mathematical formulation of this problem nor an effective method for solving it (in 1941 Hitchcock gave a mathematical description of the problem but without analysing it or indicating a method of solution).

In a paper with Gavurin [79] effective methods for solving the problem were given in an expanded form (this was a version of the method of pivot factors, but a special one—the method of potentials), criteria were given for the optimality of the solution, and some more general problems were posed that were not soluble at that time by the computational means that existed. An economic interpretation of these parameters was discovered as territorial prices for a given load, and the problem of the rational distribution of industry was considered. In printed form this was heavily shortened in comparison with the manuscript version that was unfortunately lost. This paper was presented in January 1941 to the mathematics section of the Leningrad House of Scientists.

The publication of this paper met with many difficulties. It had already been submitted to the journal "Railway Transport", but because of the dread of mathematics already mentioned it was not printed either in this or in any other journal, despite the support of Academicians Kolmogorov and Obraztsov, a well-known transport specialist and first-class Director-General of circulation.

Fortunately, I gave an abstract version of this problem—a note on the displacement of masses in a compact metric space [80], in which the criteria and method of potentials were mentioned. At the end two problems were cited—a problem on railway haulage (with a reference to my article with Gavurin that was with the publisher) and a problem on the levelling of an aerodrome surface that also has an applied character. This paper, published in 1942 in Russian and English, was apparently the first from which western specialists got to know of my work on linear programming, but this was not until the beginning of the fifties.

The same fate also befell my paper on three-dimensional rational cutting of wood into timbers of highest price [81]. It remained in the editorial office of the journal "The Timber Industry" until 1949 and only came to light then—this was the year when I received the State Prize, though for other papers.

My acquaintance with Novozhilov dates roughly to the summer of 1940. He was one of the most qualified economist-statisticians of our country, who had been educated in economics and had a good mathematical training before the revolution. He worked at the Polytechnic Institute and studied problems on the most effective use of investment by considering it as an extremal problem. The mathematics of it were not very interesting, because this is a single parameter case and the problem is solved by the introduction of one pivot factor. But, in any case, his knowledge of economic theory and practical experience were considerable. In general, Novozhilov was an economist with a wide profile interested in many problems. Our acquaintance began on the initiative of Novozhilov, on whom my pamphlet had made a great impression—the use of some pivot factors allowed him to give a broader presentation of the problems that he had been studying earlier.

In 1940-1941 we led a seminar together at the Polytechnic Institute, in which a number of young fellows of the Institute took part, where he and I gave a number of reports on describing actual problems of a socialist economy. The reports led to interesting discussions. I remember that when it came to Novozhilov defending his doctoral thesis his main opponent was Academician Strumilin, who although subjecting him to sharp criticism of the mathematical approach still valued the high level of research and spoke in favour of the award of a degree.

Even in the final days before the beginning of the war meetings were called at factories, devoted to attempts to introduce particular tasks. Of course, when the war started everything changed.

I have not said that going on in parallel, of course, there was official work, including another mathematical paper. My text-book "The definite integral and Fourier series" [82] was published. At the military institute I gave a course on probability theory and wrote a text-book that was original in the sense that it was designed for the general mathematics classes for second-year students of higher education technical institutes and contained a number of interesting examples from the military field [83]. Publication continued of several papers on the aforementioned trends. Besides papers on functional analysis at that time some new theorems on Ritz's method were given [84] and a study was made of the convergence of variational methods and the method of reduction to ordinary differential equations [85], [86]. In part this touched on the well-known paper of Krylov and Bogolyubov [87]. These papers were completed as part of the preparation of a second edition of a book on approximate methods that was radically reworked and enriched and came out with the title "Approximate methods of higher analysis" [88] just after the war started.

In studies of the convergence of approximate methods essential use was made of the methods of the constructive theory of functions. Later, in the fifties these ideas received further development in papers by my students Kharrik and Il'in [89]-[91].

I also made separate notes on economic problems, but under conditions of blockade and military service this work could not be carried on so intensively, the more so as I was drawn into specialist themes.

After leaving Leningrad in January 1942 for Yaroslavl', to where the Higher Engineering and Technical School of the USSR Navy had been transferred, to some extent regular activity—lectures and research—resumed. It was then that I wrote the long manuscript "Economic calculation of the most appropriate utilization of resources", the title of which I afterwards changed several times. With the support of Academician Sobolev, who was then a deputy to the Supreme Soviet of the RSFSR, this work was sent to Gosplan and considered by some of its leaders—Starovskii and Kosyachenko, the then Deputy President of Gosplan—but it did not meet with approval. At the same time I gave a report on this paper at the Moscow Institute of Economics at a seminar led by Ostrovityanov. A number of leading economists took part in the discussion—Atlas, Notkin, and others. It must be said that the discussion at Gosplan was quite sharp at times.

I also gave a report of this paper in Kazan, where the Mathematics Institute of the Academy of Sciences and LOMI had been relocated during those years. At this time I also got to know several economists.

The engineers were completely indifferent to this work. Thus the military establishment where I worked in 1943 was trying to put it forward for the Stalin Prize. Since it was not competent enough in these matters I proposed to submit it through Leningrad University. It was sent to Professor Fikhtengol'ts, who introduced Voznesenskii to the paper. The Rector went all the way to Moscow and then decisively rejected this possibility.

Everyone was saying that it was necessary to leave this work for the time being. It was dangerous to continue it—as I subsequently found out, my fear was not unfounded. Of course, this was a severe blow to me as I had put great faith in it. For some time I was even in a state of depression

—I began to doubt that I could successfully study science—this work had to be deferred and from then on I remained in mathematics.

This was now in the final months of the war, and I returned to Leningrad with the military college in which I worked. The university had also returned to Leningrad, and the return of LOMI was expected. I resumed work at the university but it was more difficult at LOMI.

It is interesting that in the mid-fifties my work was again sent to Gosplan and other agencies and again rejected, though less severely.

Computation, machines, programming

As early as 1943, when I was on a study visit to Moscow at the invitation of Lyusternik, I went to a seminar where the problems of using machines for large computations were being discussed. Some such calculations were going on in Moscow at that time. At first talk was about countably-analytic calculators that had been acquired for the 1939 population census and after this were hardly used. Apparently these machines were first applied to numerical calculations by Professor Yanzhul from the Astronomical Institute in Leningrad. The possibilities for using these machines for other calculations were discussed at the seminar. They were very slow—the tabulator took half a second for an addition, and for multiplication between five and eight seconds. They were casually talking about initial developments in electronic computers and countably-analytic calculators constructed on the same principles (of type Mark I and Mark II in the USA). I was very interested in these questions and at the seminar suggested a number of ways of applying them.

After returning to Leningrad I was initially commissioned to a group at LOMI and then the department of "Approximate computation", where we had to study the development of numerical methods and carry out large real computations provided by some organization or other (usually connected with physics).

We established a link with the Leningrad calculator factory; this was studying very simple book-keeping calculations on machines that were also left over from the population census. Gavurin and I suggested some new schemes for using these calculators. The basic principle for their effective use was paralleling of analogue calculations, thanks to which there appeared the possibility of introducing simple program changes on the switchboard (by hand, of course).

Thus, methods were proposed for fast sampling from tables and a method for calculating a scalar product was proposed not by multiplication but by addition on the tabulator, in which case one of the multipliers was formed not in base ten but in the binary system [92].

A serious actual achievement was the calculation of tables of Bessel functions up to order 120 on a large interval using this primitive technology [93]. The most interesting thing here was the paralleling of calculations of the differential equation for Bessel functions for integrating on these machines. Paralleling was achieved by the fact that the integration interval was split into several intervals and functions of different numbers were calculated simultaneously on each of the intervals. Therefore a sufficiently large number of identical operations were obtained that were effectively performed on these machines.

This work was carried out by Gavurin and Fadeeva with my participation. It was interesting that tables of Bessel functions were being calculated in parallel in the USA on their Mark and ENIAC machines. Our work began two years later and was still completed half a year before the publication of the American tables was finalized.

The computer technology itself also attracted my attention. I had an idea—to perform automatically the important operation often met with of sampling the values of a function from tables. A special device was worked out for this—a "function generator" in simple electromechanics, of copper-oxide and selenium semiconductors, that included over ten thousand semiconductors. At the request of the Mathematics Institute and other interested organizations some copies of this device were manufactured at the calculator factory in Moscow. It allowed the operation of calculating the value of a function—sampling of the basic value from a table and calculation of the correction—to be done in a short time. Each function required its own system of commutation. However, as I have already said, there was also a board with universal switching that allowed ten different functions to be calculated simultaneously but with less accuracy. Later the "Function generator" was registered as an invention [94]. Time went by, but it was already time for transferring to computer technology. Our first computers appeared—the "Strela" and "BESM" with which our device could not compete. But this was probably the first computer in the world in which semiconductors were widely used.

In the early years computer technology was not very productive, expensive and accessible only to some priority establishments. Meanwhile, calculations were carried out at many organizations—scientific, planning, geodesic, and others, in which hundreds of calculator operators worked, and it was by them that mass calculations were actually carried out. These operators worked on mechanical calculators—adding machines. The most exact of the desk-top adding machines were at that time the "Mercedes-Euclid" and the "Reinmetall" that were imported from abroad.

Attempts at particular production of similar machines met significant difficulties—higher quality materials and very accurate machining were needed. Only a few hundred of these machines were produced, and they worked unsatsifactorily. Meanwhile, the foreign machines also had faults—they were not very reliable, needed frequent and difficult repair, and had dangerous parts, such that a qualified mechanic was needed to every five working machines.

Here our experience in the compilation of elements, computational methods, and construction of devices, built up during our work on "function generators" and certain other experimental models, proved useful. On the basis of this we constructed original models of desk-top calculators using techniques that had not been applied before either here or abroad. This was registered as an invention [95]. The machines constructed were somewhat more productive than mechanical automata, and very simple to use and repair—only one of the several types of elements needed to be changed.

Introducing them into practice turned out to be a more difficult matter. Both in Moscow and in Leningrad they refused to manufacture them but, fortunately, during the time of the Economic Council both a factory and a construction bureau that were underutilized turned this invention into a reality. All the same, they did not start operation for a year or a year and a half, hoping to make an analogous machine but from a particular invention. However, the work went to plan, a time-table was agreed, and drawing plans were developed. In quite a short time the manufacture of these machines was begun in three factories, including the machine "Vil'nyus" in Vilnius.

Over ten years nearly forty thousand machines were turned out; in the main, they satisfied the needs of the country and allowed us to stop imports to a large extent. The production of desk-top electronic keyboard calculators was only begun in the seventies and at first they were much dearer and less easy to use. But progress was inevitable.

Almost immediately after the appearance of electronic computers work began on the simplification and automatization of programming. We in Leningrad were also included in this work. In my opinion the main fault of the existing system was the sharp distinction between machine language and the mathematical language used to describe an algorithm. In a mathematical description symbols, large-scale operations, and different mathematical concepts were used, but in a machine program all this had to be done in standard operations on numbers. Of course, the system of control and transmission, in whose formation von Neumann played a large part, was a great achievement.

The first innovation of our system was another description of the computational scheme, namely, using not only simple arithmetic operations but also many bigger mathematical operations. Of course, another thing was the fact that the elements of the scheme were not individual numbers but whole ensembles of them with descriptions of their arrangement and so on. As a result the calculation scheme was described from a computational plan with logical connections and transfers that was quite short and visible.

For the bigger operations—ordering, scalar product, operations on matrices, and so on—subprograms were created to perform them. Special operations were separated if they were often met with in some sort of calculation. In this case instead of machine addresses inventory addresses were used that made it possible to access the data of an array or an operation.

This system, in accordance with what was then the fashionable terminology used in the project, was called "macro-block programming".

These ideas also had another embodiment—thanks to their use in a logical scheme for describing the process of a calculation it was possible to operate with these schemes and to use them not only for numerical but also for analytic calculations. Let us say, the program allowed analytic differentiation of any function made up of elementary or special functions to be carried out. This work started in 1953, but the first publications were only possible in 1956 [96]-[99].

I did not manage to continue this work to its conclusion and create a total system of automated programming. As is well known, the creation of such a system requires hundreds of calculations. Moreover, in our system, control operations occupied a somewhat bigger place, so that because of the lack of machine time then it was advisable to use the machine only for mass problems. The schematic writing of computational plans came into use in the creation of the "Mir-3" machine in Kiev, and afterwards was used by physicists for carrying out complex analytic operations by machine [100].

At the same time programs were written (for command and automatized execution) for solving linear programming problems, in particular for the transport problem. The method of potentials was realized in a compact and fast program published in 1958 by Yakovleva [101]. My students Petrova, Bulavskii, Yakovleva, and Zvyagina worked on these programs and the system of "macro-block programming".

Functional analysis and approximate methods

I have already talked about my research on functional equations in partially ordered spaces, where I was the first to link functional analysis with the field of computational mathematics [72], [73]. However, I was already able to implement this to its full extent in the post-war years. In a long article that covered several phases of my research, the very name of which then sounded paradoxical—"Functional analysis and applied mathematics" [102]—the wide possibilities for a variety of applications of the ideas of functional analysis to the development of mathematics were indicated and different links between them were established. This paper contained the general theory of approximate methods and the following groups of methods were studied: 1) the method of greatest descent and other gradient methods; 2) Newtonian methods; 3) the principle of majorants and methods of successive approximation.

Of this series of papers the most important to me was a paper on the general theory of approximate methods [103]. There are very many distinct methods for different classes of problems and equations, and constructing and studying them for every actual case presents considerable difficulty. Therefore the idea arose of constructing a general theory that would make it possible to construct and study them from a single source.

This theory was based on the idea of the connection between the given space, in which the equation to be studied is specified, and the more simple one into which the initial space is mapped. On the basis of studying the "approximate equation" in the simpler space the possibility of constructing and studying actual approximate methods in the initial space was discovered. General theorems were successfully proved that allowed the solubility of the approximate equation and the convergence of the approximate solution to the exact solution to be established on the basis of information about the exact solution, and also theorems that allow the existence of an exact solution to be established and the domain of its location to be determined on the basis of the solubility of the approximate equation.

Besides the large number of effective calculation methods with an accurate characterization of the speed of convergence generated on this basis, it gives a number of important tools for theoretical analysis. Using this in a number of cases it is possible to give a strict proof of the existence of solutions and to establish the domain of uniqueness and certain properties of the solution. The approximate solution can be obtained not numerically but in a formal way on a computer. This new field of mathematics was called "proof computations".

The theory had numerous applications to many questions. I shall mention papers by Vladimirov on "transfer" equations [104], by Kalandiya [105], and by Karpilovskaya [106]. In particular, in the last paper the convergence of the collocation method for a differential equation, which could not be proved directly, was proved on the basis of the general theory.

It seems to me that the basic idea of this theory is of a general character and reflects the general gnosiological principle for studying complex systems. It was apparently used earlier, and it is also used in systems analysis, but it does not have a strict mathematical apparatus. The principle simply consists in the fact that a given large complex system in some space corresponds to a simpler, smaller-dimensional model in this or a simpler space by means of a one-to-one or one-to-many correspondence. The study of this simplified model turns out, naturally, to be simpler and more practicable. This method, of course, presented definite requirements on the quality of the approximating system.

There are different means for constructing such simplified systems, let us say in economics—the study of small-dimensional problems or global models. One of these general methods for constructing simplified systems is the method of aggregation. An extremely urgent question for economics is the study of the closeness of a solution, taken on the basis of an aggregated model and located in an approximating space obtained by means of the aggregation of variables, to the desired solution in the initial space. Another general method is a mapping based on the use of the elements of a partially ordered space as a norm in the initial space. Let us say, a function is normed not by its maximum on the whole interval but by some choice of numbers—by maximal values on subintervals. It is evident that such a norm much more accurately characterizes the function.

In any event, large systems are simplified to systems of smaller dimension but still sufficiently close to them. In the theory of approximate methods the general principles and actual theorems allow conclusions to be drawn about the initial large system on the basis of the study of small, simpler systems—the existence of a solution, its uniqueness, asymptotic properties, and finally the obtaining of numerical estimates. It appears to me that a generalized theory of the type of the theory of approximate methods must rightly have wide application in systems analysis and in economic research.

Relying on the results and theorems of the general theory, it was possible to study and interpret many approximate methods for solving complex equations. In particular, on the basis of the study of functional equations in Banach spaces, the conditions of convergence of the method of greatest descent were obtained [107], and curiously in such a generalized treatment the estimates of convergence turned out to be of limited accuracy. The method of greatest descent also had interesting theoretical applications. Thus, in the paper [108] by my post-graduate student from Hungary, László Czách, the formerly difficult theorems of Giraud on partial differential equations were proved; they relied on both the study on the method of greatest descent, and on some results of the constructive theory of functions.

A number of profound results were obtained in studying the generalized Newton method for functional equations [109], [110]. Accurate estimates for convergence and its domain of solution were given, and other analytical facts were established. The fact that this theory was applied not only to the numerical solution of problems but also to a qualitative analysis of them was of fundamental importance. This paper found wide application, in particular, in the fundamental research of Kolmogorov and Arnol'd on the motion of a celestial system, in papers by Maslov and others, and also in many applied papers. Research on Newton's method was published in many text-books.

An even simpler foundation and some more accurate estimates were obtained on the basis of theorems on majorization connected with a normalization of the initial space by elements of partially ordered spaces [111], [112].

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