L. V. Kantorovich and linear programming

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January 2001

I want to write about what I know and remember about the activities of Leonid Vital'evich Kantorovich, an outstanding scientist of the 20th century; about his struggle for recognition of his mathematical economic theories; about the initial stage of the history of linear programming; about the creation of a new area of mathematical activity related to economic applications, which is called sometimes operation research, sometimes mathematical economics, sometimes economic cybernetics, etc.; about its place in the modern mathematical landscape; and, finally, about several personal impressions of this distinguished scientist. My notes in no way pretend to exhaust these topics.

1 The "discovery" of linear programming

When attending a remarkable detailed two-year course of functional analysis taught by L. V. Kantorovich (in 1954–1955), I had never heard neither about his research in duality theory, nor about computations of Banach norms (his notes in *Doklady Akad. Nauk* published in 1938–1939), nor about linear extremal problems (the famous "veneer trust" problem) and the method of resolving multipliers suggested by him for solving problems that were later called problems of linear programming. I learned all this a little later. As to this course of functional analysis, L.V. taught it for several years; later it formed the basis of the widely-known book *Functional Analysis in Normed Spaces*, written by L.V. and G. P. Akilov, his main pupil in this field. At the time, it was undoubtedly one of the most exhaustive and deep monographs on functional analysis in the world literature, and simultaneously a textbook. Later I had a chance to make sure that abroad it was also very popular. By the way, the "Leningrad" functional analysis, initiated by V. I. Smirnov, G. M. Fikhtengolts, L.V., who served as the main engine, and, somewhat later, G. P. Akilov, had its specific feature: the influence of mathematical physics (S. L. Sobolev), complex analysis (V. I. Smirnov), theory of functions (G. M. Fikhtengolts, I. P. Natanson, S. M. Lozinsky) was stronger than in the Moscow or Ukrainian schools, which were more affected by operator theory, spectral theory, multiplicative functional analysis, representation theory, and Banach geometry. Before the war, L.V. also created a specific "Leningrad" research direction: functional analysis in partially ordered spaces. But the main contribution of L.V. in this field,

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unanimously acknowledged throughout the world, is related to applications of functional analysis to approximative methods (summarized in his famous paper "Functional analysis and approximative methods" published in *Uspekhi*[1]). These works were recognized by the Stalin Prize; they initiated an enormous amount of research in this area.

During many post-war years, problems of functional analysis were mainly being discussed at the well-known Fikhtengolts–Kantorovich seminar at the Department of Mathematics and Mechanics of the Leningrad State University, which I attended regularly since 1954 and up to its actual closing in the mid 50s. An active role in its organization, especially in the last years, was played by Gleb Pavlovich Akilov, my first scientific advisor, an original and independent man, a pupil, coauthor, and colleague of L.V. Once G. Sh. Rubinshtein, who was also in fact a pupil of L.V., gave a talk on best approximation and the problem concerning the structure of the intersection of a ray with a cone, i.e., essentially a problem of linear programming. But at the time it was perceived merely as a separate talk on a particular subject, and I do not remember L.V., or somebody else, giving any comments or saying in what context it should be understood. But I remember the impression of reticence remained after this talk.

Apparently, this reticence was due to the internal veto, whose reasons were well known to senior participants of the seminar, implicitly imposed on open discussions of this circle of L.V.'s papers. This veto ensued from the persecution of his ideas unleashed by the ideological bonzes soon after he had published the brilliant booklet *Mathematical Methods in the Organization and Planning of Production* (1939) and written, during the war, a book on economics, which was published almost 20 years later. This turn of events threatened to bury the whole research direction, as well as to bury his author, in the most direct sense. Only many years later it became known how serious were the accusations and threats of high scientific and ideological officials. This veto existed up to 1956. And it applied not only to economic matters, but even partly to the mathematical aspect of L.V.'s works. Many of these documents have been recently discovered by V. L. Kantorovich. It is of great importance to bring them into the open for all who are interested in the history of our science. Already at that time there were some vague conversations about the applied research carried out by L.V. in the post-war years: on optimal cutting with V. A. Zalgaller, on the mass transportation problem with M. K. Gavurin, etc. But, honestly speaking, I attributed all this to the boring category of "collaboration of science and industry" (a propaganda cliché of the time, which usually covered superficial or even meaningless works) and did not know the economic and mathematical seriousness of the matter. During the first years, V. A. Zalgaller, M. K. Gavurin, and G. Sh. Rubinshtein (one should also include in this list A. I. Yudin, a student of L.V. who died at the front, and maybe some others) were the closest L.V.'s assistants in applied economic studies and developed the theory of these problems. With M. K. Gavurin, L.V. wrote the famous paper on the mass transportation problem (written before the war, but published only in 1949). With V. A. Zalgaller, he worked on the problem of optimal cutting and wrote a book on this subject (1951); V.A. also tried to introduce optimal cutting at the Egorov Wagon Plant in Leningrad. For well-

[1]The journal *Uspekhi Matematicheskikh Nauk*.

known reasons, people with “flawed” biographies could find a job at civil enterprises (such as the Egorov Plant). Sometimes this resulted in the professional level at such a plant being above the average. For the same reasons, G.Sh. (patronized by L.V.) managed to get a job at the Kirov Plant, where he also tried to introduce optimization methods and simply reasonable approaches to problems of local planning. Note that G.Sh. graduated from the university at the time when he, being a veteran of the war and a successful student, could not enter a graduate school. Before the war, G.Sh. studied at the Odessa University and was a student of M. G. Krein, so that he successfully combined the knowledge of that part of the work of M. G. Krein and the whole Ukrainian school of functional analysis that was close to L.V.’s activities (the L-moment problem) with the good understanding of L.V.’s ideas in linear programming. There were also attempts to introduce optimization methods at the Skorokhod Factory, Lianozovo (ex-Egorov) Wagon Plant, Kolomna Locomotive Plant, etc. But, amazingly, this activity met the resistance of those who would seem to be most interested in it. At the time, as well as later, there existed a number of comical examples of reasons why some or another well-founded suggestion did not find support. For instance, suggestions on optimal cutting came into conflict with the bonus promised to those who collected more waste products for recycling, etc. Afterwards, optimal cutting was much studied by the Novosibirsk pupils of L.V., in particular, E. A. Mukhacheva.

Were there any serious reasons why this useful activity met such difficulties and was not on demand at the time? All of the few papers on this subject written at those “underground” years were meant for engineers and published in nonmathematical editions available for engineers. It would seem to be the best example of “cooperation of science and industry,” which opened wide horizons for scientific, mathematically-based, local and global planning in economics. At the early period (1939–1949), one might think that the reason of this antagonism was in the unpreparedness of people and their working conditions to comprehend these ideas and techniques, as well as in the deathening ideological dogmata and stupidity of the Party supervisors and ideologists. One might think that if the authorities were more enlightened, they would be able to appreciate, implement, and use the new ideas. Perhaps, L.V. also thought so. But the entire subsequent Soviet history showed that the situation was much worse... At the time, and even later, it was not thoroughly understood that the reason behind the failure of implementing the most part of new economic (and other) ideas was not in particular circumstances or in the stupidity of bureaucrats, but in the fact that the whole Soviet economic system, or, as it was called later, command-administrative system, is organically unable to accept any innovations, and no serious, big or small, economic reforms leading to stability just cannot be realized inside it.

It was not until the middle of 1956 that L.V. started actively promoting this subject and giving lectures at the Department of Mathematics and Mechanics and other departments of the Leningrad University, at the Leningrad Department of the Steklov Institute of Mathematics (LOMI). This was the disclosure of a new, earlier forbidden, area. He told about the contents of his 1939 book, about resolving multipliers, various models and problems, etc. For the overwhelming majority of the audience, including myself, these topics were completely, or almost completely, new. Undoubtedly, the “declassification” of this subject had to do with the new hopes appeared after Stalin's death, Khrushchev's speech, and the beginning of the "Thaw." At this point, it is pertinent to recall V. I. Arnold's story about A. N. Kolmogorov: being asked by V.I. why in 1953–1954 A.N. suddenly started working on the classical and very difficult problem of small denominators (this was the beginning of what is now called KAM theory), on which he had never worked before, A.N. answered: "Some hope has emerged."

Undoubtedly, some hope had emerged also for L.V., the hope that he would finally be able to explain and implement his ideas and to overcome the Soviet dogmatism and obscurantism in economics.

When one says that the Soviet science (not the whole science, but, say, mathematics) was successfully developing and reached a very high level, there is no arguing, but one still must not forget about this and many other similar stories: ideological pressure, selection according to personal details, etc. never allowed a gifted man to realize his talent in full measure or even did not allow to realize it at all. The indubitable scientific achievements of the Soviet period is only a small part of what could appear in the conditions of freedom, and the losses caused by frustrated or forbidden discoveries and ideas are irreparable.

At this period (the late 50s — the early 60s) L.V. developed an enormous activity. His numerous vehemently delivered lectures, polemical talent and zeal ignited the audience. I remember the intellectual attack organized by L.V. (in about 1959) in connection with taxi tariffs. He was charged with this task by some authorities (apparently, this was a test). L.V. organized a team consisting of 15-20 mathematicians, each being given a separate problem. The situation required a brainstorm: the team had to carry out a detailed analysis of a pile of data and output its recommendations within a week. There were some exaggerations — sometimes L.V. could be carried away by his ideas and suggest unrealistic projects, but the task was fulfilled and L.V.'s recommendations on taxi tariffs (for instance, the idea of initial fare) were implemented in 1961 and have been used since then; moreover, L.V.'s predictions (the results of investigating the elasticity of demand) proved to be absolutely accurate.

Mathematicians listened to L.V.'s talks and series of lectures with enthusiasm. Gradually, the number of those who mastered these techniques at LOMI and at the Department of Mathematics increased. At first, the then Head of the Department S. V. Vallander took an active part in the popularization of L.V.'s ideas. The Department organized a series of lectures by L.V. for a wide audience. He also gave many talks at the LOMI General Seminar.

But L.V.'s lectures for economic audience encountered hostility or, in any case, scepticism. I remember comical and ignorant objections of political economists during his lectures at the Department of Economics. After the well-known Khrushchev's speech, the ideological blinders were somewhat weakened, and it became more difficult to defend stereotyped rubbish. One could see that the orthodox position was weakening, and among political economists and ideologists, people began to appear who wanted to understand. Once (in 1957) I encountered, in an unofficial situation, G. V. Efimov, an orientalist who was at the time the Vice-Rector for Research of the Leningrad University, a man not of a liberal type. However, to my surprise, he was captivated by my description of L.V.'s ideas and their possibilities as they seemed at the time.

The idea that turned out to be the most important for the whole economic theory is the direct economic interpretation of the dual problems formulated by L.V., and it is this idea that provoked the hostility of orthodox economists. The economic analog of the variables of the dual problem (resolving multipliers), which afterwards L.V. aptly called "objectively determined valuations,"[2] was, roughly speaking, the exact mathematical equivalent of the notion of prices, and one should call them so if it were not for the fear of ideological invectives of that time. The subtlety of the term invented by L.V. ("objectively determined valuations") was in the fact that, funny as it may seem, Marxists are defenseless against the term "objectively." The emphasis placed by L.V. on dual problems led to the most important economic conclusions and protected the common sense against standard dogmata, in particular, advocated natural rent, true estimate of costs, etc. This was his most important contribution, his main trump card in discussions, and the most irritating issue for his opponents, who, of course, accused him of revising Marx's "labor" theory of value, even more so because labor appeared in his theory, too, and had no difference from, say, any raw material. How much effort he exerted to defend himself against these fatuous attacks! One could write a book about this using the documents stored in his archive. Even A. D. Alexandrov, the then Rector of the Leningrad University, could not (out of prudence or following direct instructions) publish L.V.'s new book on economic calculation at the University Publishing House.

Here is another example of how the officials of that time were afraid of everything related to this subject. At about the same time (1957), I, with a coauthor, wrote a popular paper on mathematical economics for the newspaper *Leningradskaya Pravda*, having a preliminary agreement with a member of the editorial board, who was an acquaintance of mine. Nevertheless, the paper was never published. Having smelt something nonstandard, the editor required that the text, which was no more than a popular paper, should be agreed with the authorities, which I refused to do.

One can judge to what extent L.V.'s works were known to the scientific community by the following fact. Once, at the end of 1956, G. Sh. Rubinshtein wrote for me on a small piece of paper (I still keep it) the list of **all** literature in Russian on this subject, and this list contained only five or six items, including L.V.'s 1939 booklet and his joint book with V. A. Zalgaller on optimal cutting. Moreover, almost all of them were published in little-known and rare editions, and nothing (except two or three L.V.'s notes in *Doklady*) appeared in mathematical journals. It is interesting that in the well-known volume *Forty Years of Mathematics in the USSR* (1959), where the corresponding section was written by L.V. together with M. K. Gavurin, this topic occupies only one page and the same five references are given. In spite of all this, these were years of hope that progress, positive changes, and nondogmatic attitude towards new ideas were possible.

As often happened in the USSR, military specialists were the first to have an access to books on linear programming (Vajda), operation research (Campbell), etc., obtained through special channels, translated into Russian, and not yet published in the USSR. The interest of the military to this circle of problems had to do not with economic problems (e.g.,

[2] In the English literature, they are known as "shadow prices."

distribution of resources), though they were also of importance for them; but they were a part of the general theory of control of systems, which was afterwards called by the strange term "operation research." Undoubtedly, in those years many scientific ideas obtained additional support if for some reason they interested the military; operation research and, in particular, linear programming, is one of such examples.

Of course, none of the military specialists (among which there were engineers that knew mathematics quite well; some of them joined the Army after graduating from mathematical and physical departments of universities) had ever heard of L.V.'s works, and this is not surprising. I remember how, at the beginning of 1957, being on a business trip to the 5th Research Institute of the Ministry of Defense at Moscow, I told to D. B. Yudin and E. G. Golshtein, mathematicians working at this institute, about resolving multipliers and L.V.'s works, and showed them the above-mentioned small list of references. For them, who had just started to study the American literature on linear programming, this was a revelation. Later, they became the main writers on this subject, and their role in its popularization is quite important. Indirectly, their activity became possible exactly because they had once been involved in military research.

In the autumn of 1957, I asked L.V. to give a lecture at the Navy Computer Center, where I worked at the time. This large center was created in 1956 along with another two ones, the Land Forces Computer Center in Moscow and the Air Forces Computer Center in Noginsk near Moscow, on the tide of the rehabilitation of cybernetics and the belated understanding of the necessity to introduce first computers and modern mathematical and cybernetic methods into the Army. At this center there were many serious specialists in automatic control, theory of shooting, and other fields of military science. L.V. gave a public lecture on solution of some extremal problems, which was a success. One of its consequences was that military specialists, who had been thus far using only foreign sources obtained through their own channels, started to believe that in this field too the works of our mathematicians were pioneering. It was curious to see another piece of evidence that, in spite of the decades-long brainwashing about the priority of Russian and Soviet science (and, most likely, just because of this brainwashing), most people, for example, many military specialists I met, on the contrary, could not believe that something might have appeared in the USSR earlier than in the West. The humour of the situation was, exactly, that we had exchanged our roles: they, being well-trained in ideology, in every lecture harped on the ridiculous nonsense about priority. However, they were highly sceptical when I told them about the undoubted priority of L.V. And their sceptical attitude was quite understandable: they hardly believed in the common phrases about Russian and Soviet priority.[3]

On the other hand, everybody knew quite well that most often new and sensible ideas appearing in the USSR could not push through, or managed to push through only after a round-the-world voyage. This was partly the case with L.V.'s theory, as well as with many other ideas.

[3] At this point, one cannot help remembering the sad story of I. M. Milin, a well-known mathematician who worked at a military school in Leningrad and was fired only because at a lecture, after the obligatory mention of the priority of Russian mathematics in some elementary question, he took the liberty of making a humorous remark: "And now let's turn to serious matters."

L.V.'s offensive, which had begun in 1956, continued up to the mid 60s, when his economic theories if not started to be recognized, but at least stopped to be forbidden by the ideological and economic establishment.

Later, they even found some (not absolute) recognition: in 1965, L.V. (together with V. V. Novozhilov and V. S. Nemchinov) was awarded the Lenin Prize. From the very beginning, L.V. obtained support — in discussions, at conferences, etc. — from many venerable mathematicians (A. N. Kolmogorov, S. L. Sobolev) and some economists. Many specialists participated in these discussions, and, of course, they involved not only L.V.'s theories, but also many other topics (close economic theories, e.g., that of V. V. Novozhilov, cybernetics, the role of mathematics and computers, etc.). I remember a large conference of mathematicians and economists that took place in 1960 in Moscow, where many scientists, both venerable and young, spoke, and, with rare exceptions, in support of the new ideas. As a whole, it was undoubtedly the victory of sense, but L.V. wasted too much effort, taken away from mathematics and science as a whole, on this struggle. In fact, from the late 50s, when one of his latest mathematical papers was published in *Uspekhi*, he stopped working systematically in "pure" mathematics.

The history of L.V.'s struggle for recognition of his ideas is extensive and interesting both for a historian of science and a historian of the Soviet Union. It is poorly reflected in the literature, and, unfortunately, few people study it now. Meanwhile, both this experience and the economic principles advocated by L.V. are of importance today. It is not until 1998 that the volume *Essays in the History of Informatics in Russia* [in Russian], which, in particular, contains information on this epic, was published by the Siberian Branch of the Academy of Sciences (Novosibirsk).

In 1989, we organized a conference in Leningrad dedicated to the 50th anniversary of his classical booklet *Mathematical Methods in the Organization and Planning of Production.* An account of this conference was published in the journal *Ekonomiko-Matematicheskie Metody.* When preparing for this conference, V. L. Kantorovich found many interesting and unknown documents related to L.V.'s struggle for his ideas and, in particular, letters and instructions of ideological bonzes concerning his works. These documents must be published and become known to all who are interested in the sad and instructive history of our country. At that time, and especially now, all these facts are known very poorly.

Of course, the awarding of the Nobel Prize put L.V. in a unique position in the USSR (this was the only our Nobel Prize in economics, and, moreover, awarded simultaneously with the Nobel Peace Prize to A. D. Sakharov). Should it not mean general recognition and confidence? However, up to the end his position remained that of a captive rather than a principal expert, as it should be.

Though L.V.'s economic ideas were in a sense consonant with planned economy and could be easily interpreted in generalized Marxist spirit, the persistent hostility towards them should be explained in psychological rather than logical terms: the ignorance inherent in an ageing dogmatic regime is not psychologically prepared for intellectual renovation, no matter how clearly one tries to explain its advantages for the regime itself. A very simplified interpretation of the mutual relations between L.V. and the ruling ideology was given by A. Katsenelinboigen in a quite interesting paper "Does the USSR need Don Quixotes?" (*L. V. Kantorovich: a Man and a Scientist; His Contradictions* [in Russian], Chalidze Publications, 1990). I am not going to discuss deep and important problems related to the mutual relations between a scientist and society. In the Soviet period these relations were especially complicated and did not allow for straightforward and primitive interpretations. Of course, every conformist society rejects new, unusually looking, ideas unless they are inculcated by the authorities. This applies even if the advantage of the realization of these ideas is obvious. As a French Sovietologist said on a similar occasion, "The authorities do not like to be protected by means they do not comprehend." No wonder that a scientist wishing to advance his ideas is compelled to speak, at least partly, in conformist language. And in this respect L.V. sometimes went too far. But only those knowing and remembering that time and those people, who had survived the chilling horror of the late 30s, can correctly estimate some steps which would look strange in a normal human society. One should not dismiss the atmosphere of danger to life for those who dared to deviate a little from the prescribed ideological dogmata, and it is in this atmosphere that the generation of L.V. had spent the most part of life.

The famous paper "Marx, Kantorovich, Novozhilov" published in *Slavic Review* by R. W. Campbell demonstrated that some American economists had a quite good understanding of what was going on in the USSR with L.V.'s and V. V. Novozhilov's theories. This paper made quite a stir. It was classified secret and kept in special depositories of public libraries. And the authors (in particular, L.V.) had to prove that they did not agree with the "bourgeois" interpretation of theories and events suggested by Campbell. But in fact he gave a quite accurate description of the futility of the Soviet economic establishment, as well as the logical inevitability of the conclusions that L.V. had derived by systematically developing his *strictly mathematical* approach to concrete economic problems.

In the 90s, on many occasions I had to retell the epic of linear programming in the USSR abroad, and it was extremely difficult to explain, even by this example, the "wonders" of the Soviet system, which rejected the achievements of its own scientists because of absurd ideological prejudices. Perhaps, only the reference to the story of T. D. Lysenko, well known in the West, helped the audience to understand at least something.

I would like to make another general remark. When we recall the biographies of really outstanding Soviet scientists, we are threatened by two extremes. The first one is to turn them into an icon, to remember only their scientific achievements and good deeds and forget about their compromises with the authorities (such as signing obsequious letters, participating in "collective" campaigns, etc.). The second extreme is to accuse them of open subservience to the totalitarian regime because of the very usefulness of their work for the society. Now, when one can write openly, when there is no censorial pressure, it is especially important to understand that for many (not all) outstanding scientists of that generation, their position in the Soviet society was, if not an inner tragedy, at least a source of agony. Therefore no one of these extremes allows one to fully understand the very complex and tragic nature of this situation, the position of a talented man under the pressure of total control. Some deeds of these people may be regretted, but it is not merely that their scientific achievements outweigh all the other things; one must also remember that the life of a talented Soviet scientist was, above all, devoted to science, and sometimes, for the sake of science and the realization of his ideas, he had to compromise with the regime, which used his authority for its momentary purposes and usually did not understand even its own advantage from his activity as a whole, treating him, unless he had entirely become its property or its adherent, with suspicion or even hostility.

Turning back to linear programming, I think that the story of how the veneer trust problem considered by L.V. in 1938 led to the theory of optimal resource allocation is one of the most remarkable and instructive stories in the history of the science of the 20th century. The same story can serve as an apology for mathematics. This attitude towards L.V.'s works has gradually become common among mathematicians; it was shared by A. N. Kolmogorov, I. M. Gelfand, V. I. Arnold, S. P. Novikov, and others. One cannot help admiring the naturalness and the inner harmony of L.V.'s mathematical works on duality of linear programming and their economic interpretations.

2 Mathematical economics as a branch of mathematics and its connections with other fields

2.1 Connections of linear programming with functional and convex analysis

Already before the war, L.V. was a recognized authority in many fields of mathematics, especially as one of the founders of a school of functional analysis. No wonder that linear programming in his interpretation was also related to functional analysis. The same viewpoint on these problems was shared by J. von Neumann: his fundamental theorem of game theory, models of economics and economic behavior, and other results in mathematical economics have a strong flavor of conceptions of functional analysis and duality.

Like most part of the mathematicians belonging to L.V.'s school, I originally conceived the mathematical aspect of optimization econometrics in the functional-analytic framework. In other words, the duality scheme was being considered, in a natural way, in terms of functional analysis. Undoubtedly, there is nothing more acceptable from the conceptual viewpoint. Convex analysis, which was developed after the 50s on the basis of optimization problems, gradually absorbed a significant part of linear functional analysis, as well as of the classical results of convex geometry. It is in this way that I constructed my course of extremal problems that I taught at the Leningrad State University for 20 years (1973–1992): it included general (infinite-dimensional) separability theorems, the duality theory of linear spaces, etc.

Historically, the first known connections of L.V.'s theory were connections with the theory of best approximation and, in particular, with M. G. Krein's work on the L-moment problem. M. G. Krein was one of the first to notice this. The real consequence of this observation was the gradual understanding that the methods for solving both problems are essentially similar. The first such method goes back to Fourier. Later, in the 30s–40s, important studies were carried out by T. Motzkin and M. G. Krein's Ukrainian school (in particular, S. I. Zukhovitsky and E. Ya. Remez). However, the method of resolving multipliers and the simplex method were new for the theory of best approximation. From the point of view of principle, the crucial issue was the very interpretation of the problem of Chebyshev approximation as a semi-infinite-dimensional problem of linear programming. Infinite-dimensional programming was also the subject of several research works of my students at the Department of Mathematics and Mechanics of the Leningrad State University (M. M. Rubinov, W. Temelt) and some Moscow mathematicians (E. G. Golshtein and others).

The duality theory of linear spaces with cones provides a natural language for problems of linear programming in spaces of arbitrary dimension. Paradoxically, this was grasped by N. Bourbaki, who was far from any applications. Looking closer, in the exercises of the 5th volume of *Elements of Mathematics* (rather an abstract opus!) one can find even the alternative theorem for linear inequalities and a number of facts close to duality theorems of linear programming. And this is natural. Such fundamental theorems of classical linear functional analysis as the Hahn–Banach theorem and linear separation theorems are purest convex geometric analysis. The same is true for general duality theory of linear spaces.

The classical Minkowski–Weyl theory of linear inequalities appeared in modern form in H. Weyl's work of the 30s, a little bit earlier than L.V.'s theories, and this link is especially transparent. Alternative theorems, Farkas lemmas, the Fenchel–Young duality in the theory of convex functions and sets, all this was not combined with the theory of linear programming until the 50s. Apparently, L.V. did not learn about all these links till somewhat later, but his contribution is that he found a unified approach, based on the ideas of functional analysis and revealing the heart of the problems. Simultaneously, this approach provided a basis for numerical methods of solving these problems. One can say without exaggeration that functional analysis became the basis for the whole mathematical economics. A great number of problems of convex geometry and analysis (from Lyapunov's theorem on the convexity of the image up to the theorems on the convexity of the moment map) are also related to these ideas and their generalizations.

Later there appeared related works on the theory of linear inequalities (S. N. Chernikov, Ku Fan), convex geometry, etc., whose authors did not always know about the previous results; and now one still cannot say that the research in this area is properly summarized.

2.2 Linear programming and discrete mathematics

However, linear programming has serious relations with discrete mathematics and combinatorics. More exactly, some problems of linear programming are linearizations of combinatorial problems. Examples are the assignment problem and the Birkhoff–von Neumann theorem, the Ford–Fulkerson theorem, etc. At first, this aspect of the theory was not noticed by our mathematicians, and its understanding came to us from the foreign literature somewhat later. The fundamental theorem of the theory of zero-sum matrix games (the minimax theorem) was brilliantly connected to linear programming by J. von Neumann, see G. Dantzig's memoirs cited in the paper "John von Neumann" by A. M. Vershik, A. N. Kolmogorov, and

Ya. G. Sinai in vol. 1 of the book J. von Neumann, *Selected Works on Functional Analysis* (Nauka, Moscow, 1987). Dantzig writes about his striking conversation with von Neumann. Within an hour, von Neumann described the connection between duality theory and theorems on matrix games and outlined a method for solving problems of linear programming. This connection was not mastered at once. I remember that at first, the Leningrad specialists in game theory did not take into account that the problem of finding the solution of a zero-sum matrix game is a problem of linear programming; the (undoubtedly beautiful) method for solution of games belonging to J. Robinson was considered almost the only numerical method for finding the value of a game. The final proof of von Neumann's minimax theorem (the first proof was topological and used Brauer's theorem) essentially contained duality theory. Later, the equivalence of a game problem and a problem of linear programming has been intensively exploited.

In the first years, emphasis on the connections with discrete mathematics and combinatorics prevailed in the most part of foreign articles on linear programming, while Soviet authors emphasized the connection with functional and convex analysis and developed numerical methods.

From the point of view of linear and convex programming, the most important parts of combinatorics are the combinatorial geometry of convex and integer polytopes and the combinatorics of the symmetric group. The most important works of the first period include a book by B. Grünbaum and papers by V. Klee on the combinatorics of polytopes, and the studies of G.-C. Rota and R. Stanley in general and algebraic combinatorics. Simultaneously, close topics appeared in singularity theory (Newton polytopes), algebraic geometry (toric varieties and integer polytopes), etc. Later, extensive connections were discovered with the symmetric group and the combinatorial theory of Young diagrams, one of the main subjects of the "new combinatorics," as well as with posets and matroids. It is interesting that I. M. Gelfand, who called combinatorics the mathematics of the 21st century, almost simultaneously (and independently) arrived at a number of close problems (matroids, Schubert cells, secondary polytopes). At present, new combinatorial problems play a key role in various fields of mathematics.

In the first years, my interest to linear programming arose quite independently from my mathematical favorites of the time and, in particular, not only because I was a student of L.V. in functional analysis and listened to his first exciting talks on linear programming and its applications in economics. At that moment, this interest was rather practical than theoretical. The point is that, having graduated from the university and refused, for some reasons, to enter a postgraduate school, I worked at the Navy Computer Center and got interested in the problem of multi-dimensional best approximation as an applied researcher. One of the problems I worked on at the Computer Center was computer representation of firing tables, and I suggested to approximate them instead of storying in the memory of a computer. I formulated a kind of generalization of the problem of best approximation, namely, the problem of piecewise polynomial best approximation (we had then never heard about splines) for functions of several variables. Later, when I started to work at the university, my first graduate students investigated this problem. Even more later, I wrote a detailed paper on this subject. Gradually, my interest to the problem of best approximation turned into the interest to the methods that allowed to solve it. And one of them was exactly the method of linear programming. G. P. Akilov advised me to discuss these questions with G. Sh. Rubinshtein. During our conversations, G.Sh. complemented L.V.'s talks with accounts of the related work of other mathematicians. Undoubtedly, at that time G.Sh. was one of the best experts in linear programming and this circle of L.V.'s ideas in general. It was not until somewhat later that we learned about the American works (the simplex method). For us, the principal method was the method of "resolving multipliers." It fitted, as a special case, in what we called the simplex method, but our understanding of the term was wider than the American one; the classical Dantzig's simplex method was also a special case of this, more general, class of methods. Unfortunately, as it is often the case, the Russian terminology was not well thought-out and fixed, and the term "simplex method" admits a lot of various interpretations.

The school of numerical methods of linear programming in the USSR was extremely strong, and it is undoubtedly the merit of L.V. and two his principal assistants of the first generation, V. A. Zalgaller and G. Sh. Rubinshtein. Later, an important contribution was made by I. V. Romanovsky and his group, V. L. Bulavsky, D. B. Yudin and E. G. Golshtein in Moscow. Afterwards, with the development of computer techniques, numerical solution of problems of any reasonable dimension became available.

2.3 Kantorovich metric

Once, in the spring of 1957, G. Sh. Rubinshtein told me that he had finally understood how one can use L.V.'s theorem on the Monge problem (now it is called the Monge–Kantorovich problem) proved in his 1942 note in *Doklady*; namely, how one can use the Kantorovich metric, i.e., the optimal value of the objective functional in the mass transportation problem, for introducing a norm in the space of measures, and how L.V.'s optimality criterion becomes a theorem on the duality of the space of measures with the Kantorovich metric and the space of Lipschitz functions. In fact, this was an important methodological remark, since the metric itself had been already described in L.V.'s note. But it is the paper by L.V. and G.Sh., appeared in *Vestnik Leningrad Univ.* in 1958, in the volume dedicated to G. M. Fikhtengolts, that contained the general theory of this, now famous, metric, which is sometimes called the Kantorovich–Rubinshtein metric, or the transportation metric. By the way, in the same volume I published my first paper, written jointly with my first scientific advisor G. P. Akilov, devoted to a new definition of Schwartz distributions; and we also considered this metric, which had just appeared, as one of the examples. A less frequently remembered fact is that the same paper by L.V. and G.Sh. contained a criterion of optimality of a transportation plan formulated in the dual terms of Lipschitz functions or potentials. Since then I turned into a permanent propagandist of this remarkable metric, and convinced very many mathematicians, here and abroad, of the priority of L.V. and the importance of this metric. It has been rediscovered a lot of times (e.g., by L. N. Wasserstein or D. Ornstein, who did not know about L.V.'s work) and thus has many names; the method of introducing this metric is known, e.g., as coupling, the method of fixed marginal measures, etc. It has wide applications in mathematics, especially in ergodic theory, mathematical statistics, operator theory, and, in the last years, the theory of differential equations, geometry, and, of course, statistical physics (see the 2007 Addendum below). Books are written about it, which still do not exhaust all its aspects. The Levỳ–Prokhorov–Skorokhod metric, well-known in probability theory, is its close relative. The possibility of its further generalization to a wide range of optimization problems was understood somewhat later; this is the subject of my 1970 paper in *Uspekhi*, which is further developed in a joint paper with M. M. Rubinov.

Simultaneously, in 1970, I applied this metric in an important problem of measure theory and ergodic theory (in the theory of decreasing sequences of measurable partitions). There I had to consider the (wild at first sight) infinite iteration of this metric (a "tower of measures"). At about the same time, D. Ornstein rediscovered this metric and introduced it into ergodic theory under another name ("$\bar{d}$-metric").

The history of this metric and all related topics is an excellent example of how an applied problem (in this case, the mass transportation problem) can lead to introducing a very useful purely mathematical notion.

2007 Addendum. The last five or six years have seen a qualitative jump in the development and applications of the mathematical theory of mass transportation problems, which we cannot but mention here. Instead of a rather modest progress in the theory of Kantorovich's transportation metric in the 70s–80s and the appearance of several papers a year on this subject (not including many papers of purely applied or computational nature), several hundred papers and a number of books were published starting from the late 90s. As an example we can mention the recent (December 2006) six hundred page survey book *Optimal Transport, Old and New* by C. Villani with six hundred references, mainly to papers of the last years. There are several reasons for this outburst. The first one is the sharp increase in the area of application of mass transportation methods: now it includes not only the traditional fields such as mathematical economics, statistics, probability theory, ergodic theory, but also partial differential equations, differential geometry, hydrodynamics, some areas of theoretical physics, etc. The second one is that the notion of the Kantorovich metric itself has been generalized: its p-analog k_p, $p \geq 1$, $p \in \mathbb{R}_+$ (which coincides with the ordinary Kantorovich metric for $k = 1$), which did not attract much attention until recently, has come into use. The quadratic transportation metric k_2 has proved to be the most important; the simplex of probability measures equipped with this metric apparently can become one of the most important and useful infinite-dimensional manifolds. Fruitful connections with the Monge–Ampère problem, geometric measure theory, Ricci flows (which have become very popular), and other fields made the study of mass transportation problems one of the central areas of the modern analysis. Another, quite recent, facet of this subject concerns already the Kantorovich–Rubinshtein norm. Note that the p-metrics for $p > 1$ do no longer generate any norm on the space of measures. However, quite recently it became known that the Kantorovich–Rubinshtein norm on the space of measures of an arbitrary (not necessarily compact) metric space has a simple characteristic property: it turns out that it is exactly the *maximal norm* on the vector space of real compactly supported measures with bounded variation on a separable metric space, in the class of all norms that agree with the metric;

the latter condition means that the norm of the difference of two delta measures is equal to the distance between the corresponding points: $\|\delta_x - \delta_y\| = \rho(x, y)$. This provides a clear geometric interpretation of the norm and a link to root polytopes of Lie groups and so-called rigid metric spaces, introduced and investigated in the recent paper J. Melleray, F. V. Petrov, and A. M. Vershik, Linearly rigid metric spaces and Kantorovich type norms, *C. R. Acad. Sci.* **344**, no. 4, p. 235 (2007).

I would like to turn once again to the history of the question, being forced to do this by the striking unanimity in using incorrect terminology that has gradually seized the whole literature, especially in the West. On the one hand, L.V.'s priority in the mathematical formulation of the mass transportation problem, the definition of the transportation metric, and the introduction of the optimality criterion was universally recognized long ago. Usually, this mass transportation problem of Kantorovich is called the Monge–Kantorovich problem, because G. Monge, indeed, considered the plane problem of transporting a pile of soil to an excavation fill as a transportation problem with a similar estimation of costs. By the way, L.V. did not learn about this work till the mid 50s, when Monge's collected works were published on the occasion of his 200th birthday; L.V. wrote a small note in *Uspekhi* in which he explained how to fit Monge's problem into the required framework and how the optimality criterion works in this case. Monge's voluminous work contained no mention of the metric and, all the more so, the optimality criterion, so that the term "Kantorovich metric" as applied to the transportation metric is absolutely justified, and he had no other predecessors in considering this problem. Perhaps, the name of the mass transportation problem itself, the Monge–Kantorovich problem, can also be accepted, though with great reserve, because in Monge's times there were no general metric spaces, let alone that the very idea to set the problem in such a generality and apply the duality method borrowed from functional analysis is highly nontrivial, and it was an outstanding achievement of L.V. We should also take into account that for a number of objective reasons (the War, the separation of the Soviet mathematicians from the West, etc.) the 1942 note in *Doklady*, as well as other L.V.' works in mathematical economics, for a long time remained unknown to the mathematical world. As written above, for a long time L.V. did not popularize his work in mathematical economics because of the pressure of the Soviet obscurant ideological censorship. Because of this forced delay, or for other reasons, a number of authors working in various fields, sometimes not connected with mathematical economics, rediscovered and applied this metric in some or other concrete situation (most often, in less general setting than that of L.V.), not knowing about the works by L.V. and his successors. Here one can mention L. Wasserstein, D. Ornstein, and others. Sometimes, it was not an easy task to convince such an author or his colleagues that his discovery was already known, but eventually one managed to do this. But it is certainly incorrect to attribute this metric, e.g., to L. Wasserstein or to other rediscoverers, as it is being done by many authors. Moreover, this is also incorrect from the formal point of view: none of the subsequent works that contained, explicitly or quite implicitly, the definition of this metric, was of such a generality, clearness, and fundamentality that made this 1942 paper of L.V. classical.

To repair this injustice is difficult but necessary.

In 2004, an international conference dedicated to the 90th anniversary of L.V. took place in St. Petersburg. Its program and many of the talks are published in *Zapiski Nauchnyh Seminarov POMI*, vol. 312 (2004); the English translation of this volume appeared in *Journal of Mathematical Sciences (New York)*, vol. 133, no. 4 (2006). It also contains a reproduction of L.V.'s 1942 note and a number of comments on this circle of problems.

2.4 Connections with calculus of variations and Lagrange multipliers

Linear and convex programming is a natural generalization of the theory of Lagrange multipliers to nonregular problems (problems on polyhedral domains, or, as we would say now, on manifolds with corners). The fact that resolving multipliers are a generalization of Lagrange multipliers was noticed by L.V. at the very beginning. Nonclassical multipliers appeared also in other areas, first of all, in the theory of optimal control in L. S. Pontryagin's school. This theory also generalized conditional variational problems to the case of nonregular constraints, and thus it should be compared with problems of (in general, nonconvex, but in most important cases, convex) infinite-dimensional programming. This link did not become clear till somewhat later. One should say that aesthetically Pontryagin's theory is inferior to L.V.'s one, though the former is substantially more complicated (only because its problems are originally infinite-dimensional). There is a lot of literature on the connections of linear and convex programming with optimal control. However, for a number of reasons, these connections have not been elaborated to a sufficiently deep level. The principal reason is that the form in which one usually considers problems of optimal control is insufficiently invariant. An intermediate position between classical calculus of variations and optimal control, lying closer to geometry and the theory of Lie algebras, is occupied by nonholonomic problems. They also involve nonclassical constraints, like convex programming and optimal control, but of another, smooth, kind. I started to study nonholonomic problems in the mid 60s, when I begun to think about the works on invariant formulations of mechanics (by V. I. Arnold, C. Godbillon, J. E. Marsden, etc.), much popular at that time. Having seen in nonholonomic mechanics, which is a stepdaughter of classical mechanics, a nontrivial optimization problem, I understood how to state it in modern form. In those years, at LOMI we had an educational seminar for young people (its participants included L. D. Faddeev, B. B. Venkov, me, and others), at which we studied differential geometry, representation theory, Lie groups, and all other things. One day it emerged by pure accident that L.D. also thought about nonholonomic mechanics, and we decided to get to the bottom of this matter together. We wrote first a brief note for *Doklady* and then a large paper on the invariant form of Lagrangian and, in particular, nonholonomic mechanics. These papers are still abundantly cited; they contain a dictionary of correspondences between the terms of differential geometry and the notions of classical mechanics. Now this subject has become very popular; it is a remarkable intermediate between classical and nonclassical calculus of variations. In this setting, Lagrange multipliers appear in yet another new form, namely, as variables corresponding to constraints and consequences (Lie brackets) of all orders. Here one also cannot help remembering L.V.'s resolving multipliers.

2.5 Linear models and Markov processes

Since in the 60s L.V. worked intensively on economic models, not necessarily related to optimization, one cannot but mention, at least briefly, the connections of the theory of models of economic dynamics (works by J. von Neumann, W. Leontief, L.V., and others) with dynamical systems. Here I would only like to emphasize one connection, still insufficiently studied; namely, that these linear economic models are directly related to a particular type of Markov processes, in which a special role is played by the notion of positivity in the set of states. Turnpike-type theorems and Markov decision-making processes, as well as theories of multivalued maps, continuous choice problems, etc., are most directly related to this circle of problems. Apparently, these problems are now losing their importance for applications, but they are undoubtedly interesting from the viewpoint of mathematics, like any theories of multivalued and positive maps. Recall that before the war L.V. created the theory of partially ordered spaces (K-spaces), which soon closed on itself and ceased to interest him and anyone who did not work directly in this field. But partial ordering in a more general sense has always been a subject of special interest for mathematicians of the Leningrad and Ukrainian schools.

2.6 Globalization of linear programming

Attracting ideas from topology and differential geometry led also to another synthesis, namely, to the notions of fields of polytopes, cones, etc., playing an important role in optimal control, Pareto optimum (S. Smale's conjecture and works of Y.-H. Wan and A. M. Vershik–A. G. Chernyakov), etc. I mean problems with a smooth parameter that ranges over a manifold at each point of which there is a problem of linear programming. Fields of polytopes, or fields of problems, also arise in the theory of smooth dynamical systems. Another area in which one uses similar tools but with another goal is the problem of estimating the number of steps in various versions of the simplex method (works of S. Smale, A. M. Vershik–P. V. Sporyshev, etc.); solving this problem involved ideas of integral geometry ("Grassmanian approach"). The obtained estimates gave another evidence of the practical value of the simplex method and the method of resolving multipliers. In the 80s, a strong impression was produced by the works of L. G. Khachiyan and N. Karmarkar which gave a polynomial (in a sense) uniform (in the class of problems) bound on the complexity of the ellipsoid method for solving problems of linear programming. Nevertheless, this method in no respect replaced various versions of the simplex method. The bounds mentioned above lead to linear or quadratic bounds on the complexity that hold only statistically. On the whole, it is still (2001) unknown whether the problem of linear programming belongs to the class P of polynomial problems.

2.7 Linear programming and computational methods

Another research direction that was initiated by L.V. and has not been properly developed is the problem of applying linear programming to approximate solution of problems of mathematical physics (two-sided bounds on linear functionals of solutions). L.V.'s paper on this subject (1962) contained a very fruitful idea, and several related works were carried out at the Leningrad University. L.V.'s approach can also be considered as an alternative approach to noncorrect problems. This problem is very important in geophysics, and L.V. discussed it with V. I. Keilis-Borok.

3 L.V. and personnel training

One of the important initiatives of L.V. of that period (50s–60s) was to start training of specialists in mathematical economics. Already in the 50s, L.V. had a number of students and pupils working in this field, but in comparison with other his numerous studies, the number of pupils in this field was rather small. In earnest this training began in 1959, when the so-called sixth year of education was organized at the Department of Economics of the Leningrad State University; it allowed students to become acquainted with mathematical economics and L.V.'s ideas. Many well-known economists completed this sixth year: A. A. Anchishkin, S. S. Shatalin, I. M. Syroezhin, etc. This course (which existed only one year) became the center of mathematical retraining of economists. It is not out of place to recall that the most part of prominent economists of the 70s–90s have in some way learned from L.V. or communicated with him. Among those closest to him, I would like to mention A. G. Aganbegyan and V. L. Makarov. Soon, in 1959, the Chair of Economic Cybernetics was organized at the Department of Economics. At the first stage, a very active role in the organization of the new chair was played by V. V. Novozhilov, an old companion of L.V. in battles with conservative economists and the author of fascinating economic conceptions. The mathematicians that most actively participated in the organization and teaching in the first years are V. A. Zalgaller and, somewhat later, L. M. Abramov. Among political economists one should mention I. V. Kotov, the future Head of the Chair, and V. A. Vorotilov, the then Head of the Department of Economics, as well as I. M. Syroezhin, the head of the Laboratory of Mathematical Economics. As a matter of fact, the mathematical "intrusion" into the Department of Economics had far-reaching consequences not only for economic cybernetics, but for the Department as a whole. Since then mathematics held a firm place at the Department of Economics, and mathematical education there became comparatively good; mathematical courses were mainly taught by professors from the Department of Mathematics and Mechanics and at the same level. L.V.'s flying visits from Novosibirsk, though not very frequent, were nevertheless very fruitful: the most important decisions concerning the new speciality were, to a certain extent, taken on his behalf. Somewhat later (already after L.V.'s departure for Novosibirsk, but with his participation), the same was done at the Department of Mathematics and Mechanics: first (in 1961–1962), the speciality "operation research" was organized in the bosom of the Chair of Computational Mathematics, and then, in 1970, the Chair of Operation Research was created. The principal role in the development of this chair was played by M. K. Gavurin and I. V. Romanovsky, who, since the 60s, taught his seminar on optimization with a bias to computational problems.

Economic cybernetics quickly found its niche. The necessity to mathematize and renew the dilapidated (of course, this fact was not officially recognized) economic science, to study the functioning of economic structures and to optimize them quite naturally required a new type of specialists. The new chairs of economic departments were intended to train such specialists.

At the same time, strange as it may seem, there were certain difficulties in finding the place of this speciality within mathematics itself. The new speciality organized at the Department of Mathematics and Mechanics of the Leningrad State University was one of the first in the country, it was organized almost simultaneously with a similar speciality at the Novosibirsk University. The difficulties were in the fact that, important as they were, the models and methods of mathematical economics did not constitute a new area of theoretical mathematics. The mathematical aspects of the theory created by L.V., or W. Leontief, or J. von Neumann, fitted well, on the one hand, in the framework of functional (more exactly, convex) analysis, the theory of inequalities, etc., and, on the other hand (from the practical point of view), in the framework of the theory of computational methods (in which L.V. also was one of the leaders) for solving extremal problems. As to the theory of linear programming, it was a spectacular and natural generalization of classical methods (Lagrange multipliers, conjugate problems, duality, etc.). Anyhow, all this (plus optimal control) could be called new directions, new areas, but not a new mathematical science, as was the case with economic cybernetics, or, more exactly, mathematical economics, in the framework of economic science. As was said above, at first (since 1962) the speciality "operation research" was taught at the Chair of Computational Mathematics. I remember well one of the conversations of L.V. with the then Head of the Department, for which I was invited (being yet a postgraduate student). The Head, who did not thoroughly realize the purely mathematical weight of the new area, tried to persuade me to work entirely on the mathematical questions related to L.V.'s ideas, and L.V., who supported my candidacy for a position at the chair, answered that it was not quite enough for me from the point of view of "pure mathematics." After much foot-dragging, mainly of nonscientific nature, I was nevertheless hired by the Department, but not by the Chair of Mathematical Analysis, from which I had graduated and obtained my Ph.D. degree, but by the Chair of Computational Mathematics, specially for teaching the new speciality. There was indeed some vagueness in the position of the Chair and the speciality itself, since it had not its sharply defined specific characteristics (say, as the Chair of Algebra, or Geometry, or even Computational Mathematics) and thus was compelled to become interdisciplinary and partly applied. Its research and teaching area had intersections with the areas of various chairs: the Chair of Differential Equations (variational problems), the Chair of Mathematical Analysis (convex and functional analysis), the Chair of Algebra (discrete mathematics), and, of course, the Chairs of Computational Mathematics and Software Engineering). But its own area was not sufficiently wide to become an independent theoretical mathematical specialization. This predetermined both strong and weak sides of the future chair and speciality. Let me add, in parentheses, that I was, and remain, against partitioning mathematical departments into chairs; this old German tradition did not survive in any of the leading mathematical countries. Now (and for a long time) it only impedes necessary changes in the system of mathematical education. As far as I know, the efficiency of education at the Department of Mathematics and Mechanics has not been seriously studied, but I am afraid that the form of education that has not undergone any changes for so long just cannot turn out to be good. Because of this, the speciality and the Chair did not attract particularly strong students.

The situation in theoretical economics was quite different. There, the new ideas had attracted the most fresh and healthy forces, and subsequently L.V. became the doubtless leader and teacher of a whole pleiad of our economists. It would not be an exaggeration to say that all a bit educated modern Russian economists are either L.V.'s pupils, or pupils of his pupils, or have somehow absorbed his ideas. Of course, this is an important subject for a special historical investigation. It is difficult for me to tell about the Novosibirsk and Moscow periods of his pedagogical and scientific activity: this is quite another epoch (and even two epochs), which is apparently not similar to the Leningrad period.

4 Several personal reminiscences

The personality of L.V., his qualities as a teacher and a scientist, are worth a separate paper. Here I will restrict myself to several remarks.

1. My first meetings, conversations, and contacts with him especially impressed me, and my friends, with the speed with which L.V. grasped what was being said to him, anticipating and immediately calculating what was going to be said. Later I read the same about J. von Neumann; by the way, before the war he had a correspondence with L.V. on a circle of problems related to partially ordered spaces. The very first L.V.'s works (joint with E. M. Livenson) on descriptive set theory, from which his fame had begun, impressed the Moscow mathematicians, who had been working in this field for a long time, with technical skills and deep insight. His versatility and the exact understanding of the essence, whatever was the matter, were also striking. The speed and depth of his mathematical thought were on the edge of human capabilities (at least, those known to me).

I remember a discussion of a series of papers of American authors on automata theory, very popular at the time, that took place in the 60s at a seminar at the Leningrad House of Scientists. In particular, L.V. commented upon the paper "Amplifier of intelligence" by W. R. Ashby, in which the author substantiated the obvious idea of necessity to speed up the mental process. L.V.: "Of course, the speed of thought is different for different people, but it can differ from the average by three, well five, but not by 1000 times." I think L.V.'s coefficient was much greater than five.

2. At the same time, he read his lectures at a slow but quite uneven pace, reacting very vividly to questions. Each lecture began with the sacramental question "Are there any questions on the previous lecture?", which used to be pronounced by a rolling loud voice.

But sometimes, during a lecture, this voice lowered almost to a whisper. At seminars, he frequently slept, but nevertheless, by some miracle, he interrupted the speaker at appropriate moments, looking far ahead of what had been said. His comments were always useful and instructive.

3. But as to lectures of fundamental importance, he delivered them brilliantly. He was an extremely experienced polemicist and could always find exact objections that were at the heart of the matter. I remember well a number of his speeches mentioned above. It's a pity that at that time there was no videotape recording.

4. According to my observations, his attitude towards mathematics varied. Before the war and in the first post-war years, he undoubtedly belonged to the small number of leaders of functional analysis (other ones were I. M. Gelfand, M. G. Krein). This became especially clear after his famous paper "Functional analysis and applied mathematics" published in *Uspekhi*, for which he was awarded the Stalin Prize, very important for his subsequent stability in troublesome times. His well-known book written jointly with G. P. Akilov summarized the activity of the Leningrad school of functional analysis. Later, having turned to economics, he went slightly away from mathematics, but, in my opinion, he well understood that the construction of the functional analysis that had been created and successfully developed in Leningrad in the 30s–50s was already almost completed, and tried to popularize in Leningrad new research directions. I remember well his interest to the theory of Schwartz distributions; once, in 1956, on his and G. P. Akilov's request, I gave a talk at the Fikhtengolts–Kantorovich seminar on various definitions of generalized functions, and one of the first definitions was that of L.V. from a note published in *Doklady* in 1934, earlier than Sobolev's works, etc.! Later, on numerous occasions he told me about the role of I. M. Gelfand in mathematics and expressed regret that the latter was not yet elected to the Academy.

It seemed L.V. was sorry that after the 50s he had in fact left mathematics, but, in my opinion, his choice between mathematics and economics was predetermined.

5. But L.V. could also serve as an excellent example of a scientist who could be called an "applied mathematician." His flair for applied problems and the most extensive contacts with engineers, military specialists, economists made him very popular among those who applied mathematics. In his own words, he felt himself not only a mathematician, but also an engineer. His successful work in engineering, programming, industrial computations is an excellent illustration of this thesis.

6. In the professional environment he was almost always surrounded by general admiration and attention. If he was in good form, his appearance at seminars, lectures at once animated the atmosphere, "brownized" it, as one says. I believe that this was acknowledged by everybody — by his friends as well as by his enemies. In the last years, having already gone away from mathematics, he was friends with the leading Moscow mathematicians of the next generation: V. I. Arnold, S. P. Novikov, etc. I hope that they will sometimes write about their conversations with him.

In conclusion of this essay, I wish to say that we (my generation of mathematicians grown up in Leningrad), and me personally, were incredibly lucky with our teachers and we were lucky to witness and even slightly participate in creating new scientific directions, and to learn from their creators. Here I would like to emphasize the role of L.V., which is not yet thoroughly understood and appreciated. At first sight, as he himself used to say (but here one should make a natural allowance for internal and external censorship), his theories were adjusted to planned economy. But this is only an outward appearance. The main things, such as taking into account hidden parameters (rent), a unified approach to constraints (labor is only one of them), and all the ensuing consequences, make his economic applications universal and indispensable now. Upon the whole, the main result of the great experiment of Kantorovich is that the most modern mathematical tools can not only be successfully applied in economics, but they can serve as a basis for creating new purely economical conceptions. This does not mean that his conclusions will fully work today. But this undoubtedly means, and in this respect L.V. was perhaps the first (J. von Neumann did not work in economics at such a deep level as L.V.), that the mathematician's talent can radically alter and transform economic thought. Most unfortunately, L.V. did not live until the 90s, when his experience, intuition, and authority could be used with much greater effect than in Soviet times. I have no doubt that he would be able to caution the reformers, whose theoretical (and even practical) skills were at an insufficiently high level (which made them listen to dubious advises) against serious mistakes. Alas, when it was necessary, in Russia there turned out to be no experienced economist of L.V.'s magnitude.