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Invisible Barriers in the Divided World: Episodes in the History of Soviet Game Theory

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In the 1960s, Soviet scholars working in cybernetics, mathematical economics and operations research found themselves in an ambiguous situation. They had enough support from the state, the general enthusiasm for mathematical methods applied to social sciences clearly overwhelmed Marxist ideological skepticism, this work quickly institutionalized in teaching and research, while Soviet mathematics experienced its heyday. At the same time, as is well known, navigating the landscapes of Soviet academia was difficult, because the Cold war regime of science implied a series of complex interactions between closure of the country and often unidirectional (West-East) flow of ideas, between the interdisciplinary openness and political conformity. But the overall tendency of mathematization revealed not only external challenges and political pressures, but also various *internal* intellectual tensions.

This paper traces these complexities by drawing on the emergence and organization of Soviet game theory, the field, which is only now beginning to attract attention of historians (Hagemann et al. 2016) beyond the existing internalist surveys (Zauberman 1975b; Maschler 1992; Vassil'ev and Yanovskaya 2006).

Drawing on a variety of sources – interviews, personal reminiscences, existing historiography – the paper aims to understand why this story ended in the way it did and how it could be compared to the development of game theory in the West (primarily in the US and Israel). It investigates the nature – and the agents – of the Western attention to Soviet game-theoretic literature, it explains how Soviet game theorists were entangled in their local communities and their respective epistemic cultures, and it asks, in particular, whether academic antisemitism or gender disparities could have played a role in these entanglements. It also discusses why despite a lot of talent Soviet game theory never became as powerful as in other comparable mathematical cultures.

The paper will compare the institutional and intellectual fortunes of two important game-theoretic ideas that emerged in the USSR: Olga Bondareva's work on the core of cooperative games and Mikhail Tsetlin's games of learning automata. Both research projects emerged in the 1960s, both were pathbreaking – indeed, game-changing – for their times, and both proceeded without any real dialogue and academic exchange with the rest of the world. Both Tsetlin in Moscow and Bondareva in Leningrad had powerful and intellectually influential mathematical patrons (Israel Gelfand and Yuri Linnik, respectively, Gelfand being also an important collaborator) and worked as members of active research groups. Tsetlin's project, a part of the broader cybernetic movement, ended in 1966 with his untimely death, and his book (Tsetlin 1969/1973) never gained traction in the international game-theoretic community (modeling automata only became important in game theory in the 1980s). Bondareva's work (1963/1968) also very quickly became known in the West, especially after Lloyd Shapley (1967) independently derived essentially the same result. But, due to her non-conformist position, she was not allowed to travel abroad in the 1970-1980s and died tragically once this opportunity reappeared after the fall of the Iron Curtain. Her colleagues, members of the Leningrad game theory school, could hardly work on applying their ideas in economics or other social sciences and focused on abstract mathematical problems, to which they were predisposed by their training.

The unique historical source on Bondareva's life and – somewhat less – on her work is the memoirs of her husband, Lev Gordon, published in 1992. Gordon's direct, emotional and sometimes even shocking narrative is balanced by the author himself: he gave the manuscript and the first edition of the book to a lot of protagonists and later let the readers hear their voices, too, by incorporating their responses to his book, that is, first and foremost, the work of mourning.

For Tsetlin, we have a nice and helpful biography written, in the 1960s, by an important Soviet and post-Soviet humanist Vyacheslav Ivanov (1982). But no contextualization of his game-theoretic work is available to this day.

Soviet Game Theory: The Beginnings

It is easy to locate where it all began. Game-theoretic work in the Soviet Union was initiated by one individual – Nikolai Vorob'ev (1925-1995). After studying in several technical schools, he turned to mathematics, majored in algebra in Leningrad university, and in 1952 got a PhD in mathematical logic (his supervisor was Andrey Markov Jr., who began as a topologist but then turned to logic and theory of algorithms). Remarkably, even the abstract work on game theory could only begin – as many other fields in applied mathematics – after Stalin's death. Vorob'ev's first short communication on game theory – that seems to be the first Soviet paper in the field, too – was published in 1955, while he was working on probability theory and random processes.

Vorob'ev proved, among other things, what came to be called Vorob'ev-Kuhn theorem (and Vorob'ev-Kuhn algorithm, because it was simplified, a couple of years later, by Harold Kuhn) defining an equilibrium of a bimatrix game (Vorob'ev 1958; Raghavan 2002).[1] Very soon, in 1960, he prepared his second dissertation (habilitation), set up a research seminar, and, since 1961, headed a research division on game theory that was first associated with the Leningrad division of the Steklov mathematical institute, then the Leningrad Branch of the CEMI (the foremost institution in Soviet mathematical economics) and, finally, moved to ISEP (Institute of Socio-Economic Problems) created in 1974.[2] His subsequent work was that of a tireless proselyte and popularizer: he supervised dissertations, wrote review papers and a textbook in game theory, helped translate and publish multiple books on the topic, and organized All-Union game-theoretic conferences (1968, 1971, 1974).

Vorob'ev's interest in applied mathematics was probably conditioned by his background in engineering. But he combined these interests with the studies of pure mathematics and with some penchant for philosophical reflection.

Interestingly, in the 1960s, game theory, for Vorob'ev, was the part of cybernetics. The reason, Vorob'ev argued, was the crucial role of information in games (on the tension between game theory and cybernetics in the US see Erickson 2015). Later on, its disciplinary identity was made more precise: as a theory of optimal behavior in conflict situations, it was attributed to operations research, that is, to a theory dealing with mathematical models of optimality in decision-making (Vorob'ev 1970, 1971: 7) – and thus, in a broader sense, to mathematics.

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[1] Another prominent examples of the Soviet game-theoretic results is the early paper by Iosif Romanovskii (1962), who found an ingenious way to compute an equilibrium in an extensive form game, that was left unnoticed for many years (Stengel 2002).
[2] Its current name is different, see http://www.iresras.ru/. This move as not beneficial, the Institute looked at game-theoretic work with suspicion (Korbut and Yanovskaya 1996).

Philosophically, Vorob'ev was attracted to the idea of randomness inherent in game-theoretic understanding of rationality. He emphasized that, first, randomness can be a conscious decision, not the result of external indeterminacy and that, second, in human interaction, uncertainty, that is, the impossibility to attach any probability to future (re-)actions, is something real to be dealt with (Vorob'ev 1964). He mentioned the need to develop a theory of rationality for broader classes of games (with minimax solution being an obvious interpretation of rationality for zero-sum games). He tried to implement it in his subsequent work.[3]

The applicability of non-cooperative zero-sum ('antagonistic') game theory to social science depends, according to Vorob'ev, on the relation – to be determined – between the game-theoretic idea of a zero-sum game and the actually present antagonisms between persons, groups, or classes. These antagonisms are inextricably political insofar as they happen in societies and involve rational individuals (Vorob'ev 1970). Note that the primitive elements of the game were, for Vorob'ev, coalitions, not individual agents. Judging by the formalizations he developed, this was considered as a more general case.

1960s saw an explosion of the publications on game theory. As soon as 1958, the rather specialized book by Blackwell and Girshik's *Theory of Games and Statistical Decisions* (1954/1958), with a focus on statistics, was translated, followed by the first Soviet popular brochure on game theory (Ventsel 1959), the popular book by Williams' *The Compleat Strategyst* (1954/1960), McKinsey's *Introduction to the Theory of Games* (1952/1960), Luce and Raiffa's *Games and Decisions* (1957/1961), focusing on the applications of game theory; Berge's *Théorie générale des jeux à n personnes* (1957/1961); Dresher's *Games of Strategy* (1961/1964); and, finally, Karlin's *Mathematical Methods and Theory in Games, Programming, and Economics* (1959/1964). Important paper collections (mostly taken from Kuhn and Tucker's *Contributions to the Theory of Games*) were – with the help of Vorob'ev – translated as *Matrix Games* (1961); *Infinite Zero-Sum Games* (1963), and *Extensive-Form Games* (1967).

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[3] In particular, he argued that 'one of the most natural (and thus the most common) interpretations of rationality is that a rational course of action is one from which it is impractical, unprofitable, to depart in the course of the game. The commitments made by the player (or coalition) are the more real, the less reasons there are to violate them, the greater the insistence with which the player (coalition) will stiuck to them, the less the circumstances able to affect this persistence. Such circumstances may include some actions on the part of the coalition's partners and, in particular, violations of their commitments' (Vorob'ev 1967: 293).

By 1966, when Oscar Morgenstern, one of the creators of game theory, came to visit Moscow for the International Congress of Mathematicians, Soviet game theory had been already firmly established. Morgenstern was impressed: he initiated the translation of Selected Soviet papers in game theory that was published in 1968. When in 1971 *International Journal of Game Theory* was launched by Vienna Institute of Advanced Studies (an institution Morgenstern helped create[4]), Vorob'ev became member of its editorial board.

There is some evidence that game theory was interesting for the Soviet military. Thus, the collection of the papers on the military applications of game-theoretic models was published around the same time (Ashkenazi 1961, see the early overview of Soviet ideological debates around the applications of game theory in Robinson 1970). The central role was played by the military research institute NII-5 and David Yudin, an important operations researcher and a key expert in linear programming. Yudin was the editor of Luce and Raiffa (1957/1961) and McKinsey (1952/1960). Another group was associated with Yuri Germeyer, who was applying game theory for military problems in the Computing Center of the Academy of Sciences. Both Yudin and Germeyer also taught at Moscow State University.

However, the scope of application was much smaller than in the American case. In the very beginning of Cold war, Stalin and other high-level Soviet bureucrats had no confidence in mathematical methods as a way to perfect planning, including military operations. After Stalin's death, and especially after 1956, the situation changed dramatically, but still, no institution that would be comparable to the RAND corporation was created.

The Leningrad game-theoretic community (Olga N. Bondareva, Victor Domanski, Victoria Kreps, Natalia I. Naumova, Arkady I. Sobolev, Elena Yanovskaya, and many others) was by far the biggest in the country. Apart from Leningrad, in the 1970s and 1980s, a lot of game-theoretic research was done in Moscow (with Yuri Germeier (1976/1986) at Moscow University working on hierarchical games and, somewhat later, Vladimir Danilov (1992) at CEMI working on Nash implementation); Novosibirsk (where Valeri Vasil'ev was applying the theory of vector lattices to cooperative games while the group led by Alexander Granberg was building game-

[4] The Managing Editor of the journal, Gerhard Schwödiauer, was in 1971 deputy director, and in 1973-1979 the director of the Institute.

theoretic models in spatial economics); and Vilnius (where Vorob'ev's first student Eduardas Vilkas and his associates were quite active).

Olga Bondareva: Looking for a Coalition

Olga Bondareva (1937-1991) was a part of Leningrad game-theoretic community. She was interested in math in high school, came to the university to study mathematics (she joined a kruzhok, or study group, in 1950, so at the age of 13), and during the last three classes before the university got prizes at the mathematical Olympiads. Inspired by Vorob'ev's lectures, she moved from probability to game theory and did her PhD very early, at 25. When she was 33, she became an associate professor at the chair of operations research in Leningrad University. This looked in the beginning like a very successful academic career.

The Bondareva-Shapley theorem that very quickly made her famous was most probably proven in 1961 and published in Russian in 1963. What Bondareva (1962; 1963/1968) did was to provide relatively simple and straightforward conditions for the non-emptiness of the core (the set of allocations that, in a certain sense, is optimal) for a cooperative game (in which the agents can form coalitions). Bondareva managed to attach numbers (balancing weights) to the partition of the set of players and demonstrated, that in the world of balanced collections of players a simple linear inequality could help check whether the core is non-empty. Checking the existence of the non-trivial core became easy, and this condition was essentially connected with linear optimization, thus associating, now in the context of cooperative games, linear programming and game theory. In fact, Bondareva, as well as Shapley (1967), who arrived at the same result independently, refer to the duality arguments in linear programming. This was a remarkable result, very quickly noticed in the game-theoretic community,[5] and now considered to be a classical theorem in cooperative game theory, with many economic applications (Wooders 2008).[6]

In the Cold war world, the perception of Soviet scholars, Bondareva included, was that the mere publication of a result in Russian established its existence (and thus

[5] Bondareva's paper was published in English in 1968 in a collection, with a preface by Morgenstern.
[6] The proper historical work on Bondareva is as scarce, as that on the Soviet game theory. See, however, a short discussion in Klimina 2019.

priority), but this was not always the case,[7] and the independent discovery of the Bondareva-Shapley theorem was exactly the case in point. After Bondareva realized that her work remains invisible she began to care about its international reception. When in 1989 Northwestern University's Ehud Kalai founded an interdisciplinary journal *Games and Economic Behavior*, with almost all the major game theorists of the time serving at its editorial board, Bondareva was also invited (but not her advisor Vorob'ev). She was the only Soviet and the only woman in this group of 47 individuals, of which 8 are now Nobel Memorial prize winners in economics.

Back in 1971, her career seems to have been secured. But suddenly it drastically changed direction. In December 1971, a student submitted an application to emigrate to Israel[8] and, following the routine procedure, had to be expelled from Komsomol, Communist Youth organization. Bondareva, whom her husband describes as a very direct, principled, even stubborn person, was the supervisor of his group and of the game theory seminar in which he participated. Interestingly, at the Komsomol meeting Bondareva's quite loyal point was not to defend his decision, but to let him stay in Komsomol out of pity, because leaving one's homeland was a sort of a suicide. But her indignation was also spurred by the general antisemitic sentiment of the whole discussion. She was punished because the voting for expelling the student was not unanimous, and the authorities believed that was due to her intervention. The public character of late Soviet life, so vividly depicted in Yurchak (2005), played a crucial role here. People would support Bondareva, would share her beliefs, but the public expression of outrage and non-obedience was a no go. Note that, although Bondareva did keep the dissident literature in the house, she never had any dissident beliefs, unlike her husband.

Moreover, exactly at that moment Bondareva's relationship with Vorob'ev became strained, her independent position grew at the time,[9] while she was increasingly dissatisfied with Vorob'ev's actual academic work. So he did not support her in this conflict and, as Gordon suggests, even harmed her career further. Bondareva was demanded to leave the university, under the threat to fire Mark

[7] Interestingly, Kuhn built on Vorob'ev while suggesting the simplification of his algorithm. But this seems to be the exception rather than the rule.

[8] His elder brother emigrated, while he was facing troubles due to his Jewish background.

[9] In the introduction to the Russian edition of the *Theory of Games and Economic Behavior* (1970) (with Bondareva as one of the translators), completed in 1968 and trying to summarize the development of the *whole* discipline up to that moment, Morgenstern mentions only Bondareva's result and does not discuss any other Soviet contributions. Bondareva was one of the translators of the book.

Gavurin, the head of her chair (and the co-author of Kantorovich). So she agreed, signed a 4months contract as a researcher, and was fired after the contract was not prolonged. This was a major blow. In a letter to Kolmogorov in June 1972, seeking his support, she wrote:

'By signing my resignation letter, I was sure – as well as the others – that I would retain my position at the mathematico-mechanical department as a researcher. I cannot claim that cooperative game theory is necessary for the humanity, I can only state that I have been doing it since 1959, with some success.'[10]

Was there any gender aspect in this decision? There are reasons to believe there indeed was one. Gordon tells the story of another colleague, professor Nikitin, who did something worse: wrote an anonymous letter to the Party's Central Committee condemning the 1968 Soviet invasion into Czechoslovakia. His punishment, however, was less severe, he was temporarily suspended, and the university bureaucracy believed that was just a delusion, not a sign of a systemic disloyalty.[11]

Bondareva's patron, Yuri Linnik,[12] was prepared to offer her a job as a researcher in the LOMI, Leningrad branch of the Steklov Mathematical institute, but his sudden death did not allow this project to be realized. Finally, Sergei Vallander, who was the dean of the mathematics department (and very supportive of the mathematization of economics), helped appoint Bondareva in the economics-mathematical lab of the economics department.[13]

For Bondareva the academic, this was difficult: economics was never in high regard among her peers, the department was deemed 'ideological', and her ambitions were still elsewhere. Worse still, due to the strained relations with Vorob'ev she was banned from Leningrad game theory community and continued her work by collaborating with another coalition – Moscow colleagues, especially Yuri Germeyer

[10] The letter, as well as the rest of the biographical details, are taken from the book by Gordon.

[11] There was another bad story, as a background of the rest, which I do not touch upon, but which could explain the general atmosphere: Bondareva's affair with Moscow topologist Yuri Smirnov, whose wife was calling to Vorob'ev after the news that Bondareva got pregnant and asking Vorob'ev (sic!) whether they should divorce. With Vorob'ev and Vallander willing to get Smirnov, at that time a promising mathematician from the group around Alexandrov and Komogorov, to Leningrad, this all looked like a bad joke, with supervisors overseeing theorems and bodies, and matching the bodies to better prove theorems.

[12] Linnik was the head of the theory of probability chair, while Bondareva was working there as an assistant.

[13] The lab was created in 1959, its first academic supervisor was Viktor Novozhilov (1892-1970), one of the foremost Soviet mathematical economists, a supporter of Kantorovich and linear programming methods. As testified by Nikita Moiseev's letter to Lev Gordon, Vallander's position could have been fragile, too, he did not have much resources to fight for Bondareva.

(Bondareva met Germeyer and his group in 1966), by visiting the Institute of Control Sciences, and by informally meeting her former students and colleagues Natalya Naumova and Tatiana Kulakovskaya.[14] Nikita Moiseev, an influential Soviet operations researcher, was also supportive. The obituary states that Bondareva was a link between Moscow and Leningrad groups in game theory (Naumova and Kulakovskaya 1992).

The document of rivalry between Vorob'ev and Bondareva – and a remarkable document on its own right – is Bondareva's short book on game theory and its applications to economics, published in 1974. Vorob'ev, who was teaching a course in game theory at the economics department, published his own, much more extensive lecture course, in the same year. Needless to say this course had no references to Bondareva's work.

Bondareva's book – in fact, a brochure – is almost exclusively based on cooperative game theory and considers several real-life economic situations, which can be elucidated by game-theoretic solutions: a market with three agents, voting schemes, coalitions of workers deciding on the payment for a job, inventory policy, and optimal routes. It is remarkable that the two last problems were suggested by Bondareva's colleagues at *Giprospecgaz* (a research institute working on designing oil pipelines) and *Giproavtotrans* (a similar organization designing constructions connected to motor transport, its maintenance, repair, etc.).[15] However, in Bondareva's narrative these problems serve mostly as illustrations. 'Unfortunately, we are not aware yet of any serious enough economic problems that were solved with this theory (of *n*-person games), but this situation might change in the nearest future' (Bondareva 1974: 37). She has been working on different economic tasks throughout her tenure at the lab (this involved solving linear programming problems under various contracts), but could never embark on broader applications (and probably never wished this[16]).

Bondareva's troubles did not end when she changed the departments. Her habilitation was a nightmare, too. The first version was submitted in 1979 at the economics department. With a lot of problems – both with the discussion of the dissertation and with the renewed threats to fire her from the lab – she finally

[14] Bondareva's students – such as Tatiana Kulakovskaya – were attracted by her work via the mathematical *kruzhok* (group for extra-curricural study) at the Young Pioneer Palace in Leningrad.
[15] Both organizations were founded in the 1930s and exist to this day.
[16] In the second dissertation, she begins the motivation part with invoking an application of game theory

defended it in 1984 (at Moscow University). The atmosphere of the department was so unbearable that she left to work in the continuum mechanics lab of the department of physics. She was trying to get back to the mathematics department in the subsequent years, but not with much success, and even Iosif Romanovskii, who began his career as a game theorist, did not support her application. Only in 1989 could she really return there.[17]

In the twenty years following the international recognition of her result (1968-1988), Bondareva could hardly travel abroad, despite multiple invitations.[18] In fact, until the late 1980s, Soviet mathematical economists, including Kantorovich, were routinely denied visas. Unlike Kantorovich, however, Bondareva did not get to the top of the Soviet academic hierarchy and was facing lots of pressure. In 1990, she said to her husband, after the never-ending storm of invitations, travels, and visits, that she had been doing her work all by herself for so many years, and only now began to realize there was a community interested in what she was doing – an invisible coalition she had no access to.

Even though the community of Soviet game theorists also quickly realized the significance of Bondareva's results, these groups were isolated not only from the West, but also from any significant economic applications (the contrast with Shapley is very revealing in this respect). It is difficult to say, however, that only the social environment was responsible for this isolation of game theory. In any case, the last grant application Bondareva completed not long before her death was titled: 'Analysis of real pricing under the conditions of shortage'.

Mikhail Tsetlin: Automata Learning and Playing

Another significant and previously unknown episode in this history is the work of Mikhail Tsetlin (1924-1966), who would become a pioneer in cybernetics and machine learning (Ivanov 1982; Gerovitch 2002), and inspired Israel Gelfand, one of the greatest Soviet mathematicians, to work in applied mathematics.

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[17] One should probably notice that some very real rivalries and animosities (non-ideological) between Bondareva and her colleagues were also present. Bondareva was not an easy-going character, one of her former students remarked that it was a blessing she never had students stronger than herself.

[18] In fact, her last travel was in the autumn of 1970, when she taught in Leipzig (East Germany). After 1971, no travels were possible.

Tsetlin was indeed one of the foremost Soviet scholars in cybernetics. He got interested in mathematics already at high school and, got a prize at the Moscow mathematical Olympiad, and met Aleksey Lyapunov, the main actor behind the Soviet cybernetic movement. The war disrupted Tsetlin's academic plans, in 1942 he joined the army, having studied at the department of physics for about 2 months. He could resume his education only in 1947, when he met Gelfand. Gelfand reassured Tsetlin, encouraged him to continue his studies in physics, and collaborated with him till the end of Tsetlin's life. Their first joint work with Gelfand was in algebra (representation of groups), that was, however, motivated by their studies in physics – the connection, for which John von Neumann was responsible. The result was quickly recognized as a mathematical breakthrough.

After graduating in 1953 Tsetlin went to work at a radio engineering plant and fully engages with practical tasks, but also solved applied engineering problems and made some inventions. His employment in academic institutions was, at that time, difficult due to widespread academic antisemitism.

Throughout this time, and beginning with his undergraduate years, he was interested in the theory of automata, the main subject of technical cybernetics. Already around 1954 Lyapunov, in a draft note to the Academy of Science, mentions Tsetlin's name (along with that of Leonid Kantorovich) as a possible member of the 'technical-economic' team engaged in strengthening the formal presence of cybernetics within the Academy. The tasks of cybernetics being 'delineating the flows of information, finding out the ways of coding it, finding algorithms of processing it, and studying the structure of devices that process, transmit, store and receive information.'[19]

In 1956, he went back to the physics department as a graduate student and very quickly defended his first dissertation. In 1957 it became possible for him, thanks to the invitation by Lyapunov, to join the Applied Mathematics section[20] of the Steklov mathematical institute (the very same, big institute, in which Vorob'ev was working). At the same time, he participated in the engineering work on medical equipment, including the invention of prostheses and cardio-synchronization device. In 1958,

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[19] Предложения ОПМ о включении в план Академии наук СССР проблему кибернетики - изучение процессов управления и управляющих систем. Lyapunov Archive. http://odasib.ru/OpenArchive/Portrait.cshtml?id=Xu1_pavl_635766969644249164_8291

[20] This section, founded in 1953 and headed by mathematical physicist Mstislav Keldysh, later became a separate institute of applied mathematics. Gelfand was head of one of the divisions, while Lyapunov headed another one. Lyapunov was fully aware of the importance of game-theoretic work. In fact, he wrote an introduction to the translation of Luce and Raiffa (1957/1961).

together with Gelfand, he created a seminar on physiology discussing the modeling of heart activity, the physiology of movement, and, in particular, the control of movements, that Gelfand and Tsetlin sought to restate in terms of optimization.[21] About the same time, he starts teaching a course on game theory and related topics at the physics department of Moscow State University, and a seminar on modeling control and biological systems. In 1959, Tsetlin becomes a secretary on the newly created Council of Cybernetics, an organizational center of cybernetics within the Academy of Sciences.

After a series of collaborations devoted to the optimization techniques, and modeling of biological systems Gelfand and Tsetlin[22] got interested in game-theoretic description of simple behaviors and its possible formalizations. This was happening in the mid-1960s, at the highest point in the development of Soviet mathematical economics.

Tsetlin's major object of inquiry, since 1961, was automata — objects with the deliberately simplistic forms of behavior. In fact, Tsetlin is considered to be a pioneer in the research on learning automata (Narendra and Thathachar 2012). Automata could interact with their milieu ('nature') or with each other and aim at a certain payoff from this interaction. Once the environment ceases to be stationary (so the probabilities of payoffs change over time), automata have to learn to adapt to their environment.

From the games with nature Tsetlin moved to the interaction of automata. This modelling of the forms of collective behaviour had a name in Tsetlin's work: the games of automata. Tsetlin learned game theory from the introductory books that appeared precisely at the moment he began pondering about the topic: Blackwell/Girshik (1954/1958), McKinsey (1952/1960), and Luce/Raiffa (1957/1961).

Tsetlin thought about his theory as differing from the standard theory of games, because he did not assume the players to have a prior information about the payoff functions. In a dynamic fashion, they choose their strategies during the game. Note that most of the games discussed in the introductory literature Tsetlin was exposed to are precisely of this static nature.

[21] Tsetlin was in contact with the famous Soviet neurophysiologist of the older generation, Nikolai Bernstein.

[22] Tsetlin was a major motor of their joint work: after his death, Gelfand did not come back to this research program.

Of course modeling the behavior of automata prompted biological applications more than the economic. But, very much following the general sentiment in cybernetics, Tsetlin thought of his theory as being more general than the theory of physiological movement or control in the living organisms. Hence the idea that the games of automata can teach us something about the human social behavior, too.

Tsetlin's examples are extremely revealing. In the key informal presentation of his ideas, the lecture he gave in 1965, he dwelled on several examples of control. There the market mechanisms suddenly emerge:

A store sells pork and beef, and housewives, it would seem to me, prefer to buy beef. If it is necessary to increase the consumption of pork, then it is possible, of course, to send out agents to people who would explain the advantages of eating pork over beef, and who would tell people why it is in the interest of society to give preference to pork. One could approach each housewife individually and explain the matter to her or one could gather all housewives together and talk to all of them at the same time. One can also, however, do this: change the prices. (Tsetlin 1973: 109f.)

When describing the groups of automatons, Tsetlin formulates the questions he was concerned with in the following way:

it is very interesting to ask whether people who cannot see one another, and cannot talk to one another, can nevertheless reach a point where their collective behavior will be expedient. (Tsetlin 1973: 113).

One had to wait until Gode and Sunder (1993) demonstrate precisely that: even zero-intelligence traders may arrive at rational – or, in Tsetlin's parlance, 'expedient' – results while relying on an impersonal market mechanism. That could not be the case in Tsetlin, of course. Rather, he tried to demonstrate how the individual 'memory' of the interacting automata matters for the collective outcomes.

In 1965, upon returning from the research visit to the USSR where he met Gelfand, Tjalling Koopmans informed American game theorists about this work (Düppe 2016), but the moment was not right, and Gelfand and Tsetlin could not – and were not much motivated to – promote their results among economists.

In describing the problems of control in biological systems, Tsetlin states that

The restrictions on the time spent on solving a problem are particularly important. The difficulty is that practical problems (for example, those occurring in physiology) typically involve situations that vary with time, so that a delayed solution may turn out to be outright erroneous. In this sense even a relatively rough approximate solution that is obtained rapidly may be preferable to a more exact, but delayed solution (Tsetlin 1973: 133f.)

It is interesting that the formulation very much reminds of Simon's idea of bounded rationality formulated in mid-1950s.[23] But Tsetlin worked in a somewhat different disciplinary environment, and when, in the 1980s, Simon actually came to the USSR (Boldyrev 2020; Petracca 2022), few traces of this work remained.

The very idea of using theory of finite automata in game-theoretic modeling was then taken up much later, and developed, in various contexts, and following various theoretical traditions, e.g. by Rubinstein (1986)[24] or Roth and Erev (1995). Both Tsetlin's premature death and the difficulties in academic communication did not allow the field to emerge in the 1960s, although the theoretical concerns (integrating bounded rationality and learning into game theory, understanding the interaction of agents following simple decision rules etc.) had been already present at that time.

Conclusion

So, what are the lessons from the stories told here, apart from the need for these forgotten or suppressed voices to be heard? There are many, I believe, but I'd wish to dwell on one. If we ask the question: why did Soviet game theory develop the way it did and why, despite a lot of mathematical ingenuity and proselyte effort did it fail to thrive in the way it did in the US, Israel and, to a somewhat lesser extent, in Western Europe? From the history of economics perspective, one simple issue is timing: Beyond ideological pressure and conceptual limitations (the attempts to apply game theory in Soviet planning (Zauberman 1975) were too few and inconclusive), game-theoretic instruments have been widely applied in economic modeling only since the beginning of the 1980s, when the high moment of Soviet mathematical economics was behind.

The preferences of Soviet planners differed from those in the US who were responsible for military and strategic application of game theory in the social sciences

[23] Ivanov (1982) reports about Tsetlin's aversion towards too much erudition and his tendency to read the literature on his problem after solving it, not before.
[24] Rubinstein (1986) followed Simon's idea of bounded rationality, as well as the heuristic suggestions by Marschak and McGuire, but was apparently unaware of Soviet developments.

(Mirowski 2002, Ericson 2015). Soviet mathematical economics was more oriented towards input-output techniques and, for the most part, did not put much value in micro-founded choice-theoretic modeling (Boldyrev and Kirtchik 2017). Thus, internal disciplinary context mattered. Romanovski's evolution is significant here: he got disillusioned with game theory and went in other directions, hence he never seriously believed that Bondareva's contribution was actually of value.

Soviet philosophy, despite some important developments in formal logic and despite many real opportunities of collaboration in the wake of the cybernetic movement (Gerovitch 2002), could not really embrace and develop game-theoretic approaches.[25] Soviet psychology was developing along completely different lines, as pragmatist and dialectical activity theory, disentangled from most work in Western cognitive science. The interactions of Gelfand and Alexei Leontiev, the foremost Soviet psychologist, or Herbert Simon's travels to the USSR (Petracca 2022) testify to the real differences in research priorities and values.

All in all, the paper demonstrates that Iron Curtain might have been one of the multiple barriers to communication in interdisciplinary interactions and was not the only factor that eventually shaped the development of Soviet game theory. These barriers, the paper argues, only emerge once the attempted communication does take place or once we imagine it by tracing parallel discoveries (typical for Soviet mathematical economics), by contemplating missed opportunities, and appraising sporadic intellectual exchanges.

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[25] One important exception was a small book by a philosopher and mathematical linguist Arkady Blinov (1983) on game theory and semantics.

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