Realities of Formalization: How Soviet Scholars Moved from Control Engineering to the General Theory of Choice

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1. Introduction: Being among Soviet Engineers

Postwar Soviet Union was, in a sense, a country of engineers—both due to its ambitions to engineer the new society and because for the tasks of expanding its state-owned (military-)industrial complex, a large number of engineering staff was a *conditio sine qua non*.

The involvement of engineers in solving technical tasks had a natural connection to economic challenges. Suffice it to say that the most well-known early Soviet economic growth model (Feldman [1928] 1964) was created by an engineer, Grigory Feldman (1884–1958). The engineers should also be credited with the priority in formulating the idea of more precise quantitative appraisals of investment projects—a crucial topic for the postwar Soviet economic discussion (Zauberman 1967: 140).

After the purges of the 1930s, which began with the infamous Industrial Party Trial, when the engineers of the older generation were accused of sabotage and forced to publicly confess their guilt, the state started to oversee and control this class that was responsible for—and, in a sense, was embodying—the technical infrastructure of the totalitarian machine.

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A mass production of engineers that began in 1930s is now analyzed with considerably less enthusiasm (Graham 1996) than it was perceived before. Their education was, for the most part, quite narrow (unlike the one of their predecessors), and their real work was limited by the daily technical issues of the specific industry they were trained for. The same goes for the mass outlook of the engineering intelligentsia: in the Stalin times, their interests were severely limited by purely technological concerns, and the "permitted" discourses never invoked any real social, political, or ethical considerations.

By the period of stagnation in the 1970s, a whole new generation of the ITR (*injenerno-teknicheskiye rabotniki*, engineering-technical workers) had emerged, with a diverse and sometimes more subversive weltanschauung. At the same time, the educational pattern faced a natural challenge of overproduction and mismatch. In the virtual absence or suppression of the private sector, almost everyone became an "engineer." This group with fuzzy borders, fulfilling a plethora of social roles, could even be characterized as coinciding with the Soviet middle class (see various approaches to the analysis in Kryshtanovskaya 1989; Lipovetsky 2013; Tamas 2013; and Abramov 2017).

Within the Soviet technical intelligentsia, a diversity of ideologies, intellectual commitments, and strategies of relations with the bureaucratic powers (Gerovitch 2008) was a norm—and this concerned its engagement with economic knowledge as well. In the context of severe ideological pressure, when the subject matter of standard economics (markets) was definitely not on the agenda for decades, it was only natural that economic ideas were developed in methodologically heterogeneous environments and were marked by a certain eclecticism (Boldyrev and Kirtchik 2017). One important tendency was the emergence of "economic cybernetics" and, generally, the growing legitimacy of "mathematical methods" in economic analysis, to which engineers definitely contributed. In what follows, I assume that "engineers" who are the focus of this article refer not just to "technical experts" but to those individuals who, having a background in the applied sciences / mathematics, became *academics* and got interested, along with engineering problems, in the fields directly or indirectly associated with economics.

I deal with one particular group of Soviet scholars led by a control engineer and an applied mathematician, Mark Aronovich Aizerman (1913–1992). So far, in the recent histories of Soviet cybernetics, mathematical economics, and related fields (Gerovitch 2002; Boldyrev and Kirtchik

2014, 2017; Hands 2016; Leeds 2016; Peters 2016; Rindzevičiūtė 2016), this group was not considered in detail. It was Olessia Kirtchik (2019) who first addressed its work focusing on the models developed by Emmanuil Braverman, one of the members of Aizerman's group. However, the contexts and the overall direction of its work deserve a closer examination. Why did Aizerman's group start doing research related to economics? What were the constraints they were facing? And what kind of academic culture emerged as a result of this engagement?

2. Aizerman's Group: The Beginnings

Aizerman was a key force behind the major intellectual and institutional developments this article is focusing on, so it makes sense to consider his career in more detail.

After graduating from the major Soviet engineering school, the Bauman Institute, in 1937, he started working at NATI (Academic Auto-Tractor Institute). In 1937, career tracks were characteristically "easy" (there was a lack of human resources due to emigration and the waves of repressions), and immediately upon entering NATI, Aizerman became "a head of the lab." He completed his first dissertation (analogous to a PhD in engineering—"candidate of technical sciences") in 1939, rejecting the offer to defend his diploma project as such a dissertation. While in 1937 the results were only the development prototypes of automobile engines based on liquefied gas, in 1939 Aizerman actually installed such engines on various existing Soviet car types.

In 1939, a new research institution within the Soviet Academy of Sciences was created (the one with which Aizerman would associate his whole subsequent career), the Institute of Automatics/Automation and Remote Control (Institut Avtomatiki i Telemekhaniki, or IAT; since 1969 Institut Problem Upravleniya,[1] or IPU), preceded by the lab that had been founded as early as 1934.[2] Aizerman was among the first researchers to join the institute, where he started working on his second—habilitation, "doctor of technical sciences"—thesis on the "stability of a class of nonlinear automatic control systems." (He would defend it only after the war,

1. The term *upravleniye* refers to "control" (also as a mathematical term applied in the theory due to Lev Pontryagin in the 1960s), but also, importantly, to "management" and "governance." See Kirtchik 2019. The standard rendering was "institute of control sciences."

2. Comparable institutions in the United States and Germany were created in 1936 and 1939, respectively (Bissell 1998).

in 1946.) The creation of the IAT was followed by an inaugural conference gathering various scholars who worked on the theory of automatic control and thus consolidating Soviet control engineering.

Aizerman's supervisors and major influences before and after the war were an important physicist and applied mathematician, Aleksandr Andronov, and an engineer working in automatic regulation theory, Georgy Shchipanov.[3] In 1939, the institute also became home for Nikolai Luzin, one of the greatest Soviet mathematicians, who was persecuted during the infamous Luzin affair in 1936 (Demidov and Levshin 1999) and was jobless at that time.

While the influence of Andronov and Shchipanov defined the initial direction of Aizerman's research at the IPU, Luzin reinforced Aizerman's mathematical sensibilities.[4] One can say that, without having a degree in mathematics, Aizerman became an applied mathematician (all his subsequent works, including those connected to economics, are fairly sophisticated from a mathematical viewpoint).

The war (1941–45) was a break in Aizerman's research—but also made him work on military tasks (such as studying the mechanical features of the trophy German tanks or improving the famous tank T-34) at the proving ground near Moscow. After the war, Aizerman was involved in automation and control theory, and in its application in introducing automation devices and systems into various industries.

Aizerman's primary interest was in the "theory of automatic regulation"—in fact, cybernetics of technical devices. While in the 1930s Aizerman's focus was on the automation of internal combustion engines—both for military and nonmilitary purposes—he later moved to theoretical problems of automatic regulation/control.

3. On Andronov, see Bissell 1998. Shchipanov was expelled from the institute in 1939 for the paper he had published establishing the attempt to build a system of automatic control in which external perturbations would amount to zero. For the ideological background, see Bissell 1999. Aizerman wrote a letter in support of Shchipanov and had a suitcase ready at home with clothes and other things necessary in case he was arrested (Aleskerov 2018). After the big purges of 1936–38, this was a common behavior. Quite uncommon, however, was the courage with which Aizerman defended his teacher.

4. Aizerman was doing individual studies with Luzin in 1939–41 by meeting him several times a week (Aleskerov et al. 2003: 28, 32). This connection is crucial, since Aizerman had never had any deep mathematical training before. Note that prominent Soviet mathematical economists were, for the most part, either mathematicians by training (like Leonid Kantorovich) or got additional mathematical training (examples are Victor Polterovich and Braverman, who, after getting a degree in engineering, obtained a second degree in mathematics at Moscow State University [see Boldyrev and Kirtchik 2014; and Kirtchik 2019]).

At the end of the 1950s, partly following the widespread interest in cybernetics in the Soviet Union, Aizerman started working on the more abstract theory of automata. In 1962, Laboratory 25 was created, headed by Aizerman and officially called "laboratory of theory and methods of constructing automata." The new members were coming from different schools, notably from the Moscow Institute of Physics and Technology (PhysTech).[5]

The theory of automata has been basic for cybernetics. Aizerman and his collaborators were mostly interested in what certain abstract logical machines could do. In particular, after having learned of Frank Rosenblatt's (1958) construction of the "perceptron"—the first learning machine imitating the working of the brain, they got interested in how this machine could actually work. It was Braverman who initiated a new research program on the question of how machines can *learn*.[6] Braverman's formalisms for the problems of image recognition and the "method of potential functions" (describing the impact of a given perceived point on the other points) suggested by another researcher of Lab 25, Lev Rozonoer, led to the new approach in data analysis and became a key research direction over the years to come (Aizerman, Braverman, and Rozonoer 1964).

For Aizerman, these learning mechanisms in producing the patterns and classes of arbitrary elements constituted the important element of the general cybernetic problem—the problem of control (Aleskerov et al. 2003: 127). From modeling the algorithms of visual perception and automatic classification (or cluster analysis / image recognition without a "teacher"), his interests moved to applying this idea to understanding the *living systems*, in particular, control in the muscles of animals and humans, as well as the mechanisms of human perception. This, of course,

5. Since 1953, Aizerman was teaching at PhysTech and in 1964–78 held a chair of theoretical mechanics there.

6. On Braverman, image recognition, and the role of this research for economics in the context of cybernetics, data science, and disequilibrium analysis, see a detailed account in Kirtchik 2019. Importantly, image recognition turned out to be very popular at the institute: at the same time, in another lab of the IPU, headed (till 1971) by Alexander Lerner, somewhat different methods were suggested by Vladimir Vapnik and Alexey Chervonenkis, who started to work in 1962 and made important contributions to computational learning theory. In fact, Chervonenkis's (n.d.) work was initially inspired by Aizerman's lectures at PhysTech. After the fall, Vapnik, who is now one of the world's most famous data scientists, went to the United States, and somewhat later Chervonenkis became a professor in London; they received prestigious awards for their contributions (e.g., John von Neumann Prize for Vapnik, who perfected his methods while working with AT&T in the 1990s, and partly incorporating Braverman's and Lev Rozonoer's insights).

was influenced by the work of Norbert Wiener, which was at that time already widely debated in the Soviet Union, followed by the visit of Wiener himself in 1960 (see Gerovitch 2002; Peters 2016).

But this new field required, among other things, a new methodology. "Being an expert in the theory of control in technical systems, [Aizerman] discovered, that an adequate language in studying the control principles in living systems is the language of biological experiments. This understanding involved a radical rethinking of the ways how an engineer should study the system of control" (Andreeva and Muchnik, quoted in Aleskerov et al. 2003: 162).

Instead of what we now call calibration, when the theoretical model is the first step in constructing and experimentally testing a technical device, the new methodology involved a direct experimental study of the living systems as its primary aim and its major source of information and theoretical insights. This was not the first interdisciplinary leap of Aizerman, but it was a crucial one, clearly involving the new experimental techniques and an extraordinary openness toward other sciences. In 1963, Aizerman launched a seminar called "Extension of the Capabilities of Automata," in which mathematicians, biologists, psychologists,[7] medical experts, and cyberneticians all shared their experiences.

Overall, in this research, image recognition was tied to the problem of using the learning mechanisms in the systems of control. The challenge was to understand how this can work in a machine—and how to actually build the machine capable of image recognition. Animals and humans served as living models for this research.

Despite his primarily theoretical interests, Aizerman was willing, throughout his career, to combine the pure and the applied, and to make the one inform the other. For example, in the 1960s, Aizerman's lab cooperated with NIITeplopribor—a research institute associated with the Soviet Ministry of Mechanical Engineering and Instrumentation—in helping create automation devices for various industries. In particular, with several colleagues he created and implemented a unified system of elements for industrial pneumatic (using the compressed air energy) control. In 1964, together with his colleagues from the research division he was heading, and with some others, Aizerman was awarded the prestigious Lenin Prize for the advancement of technology.

7. Aizerman was even in contact with Alexei Leontiev, the head of Soviet school of "activity theory" in psychology (Aleskerov et al. 2003: 148).

3. Lab 25 Turns to Economics

The IPU in general and Aizerman's lab in particular were—both geographically[8] and conceptually[9]—not so far away from Soviet mathematical economics. Aron Katsenelinboigen at the Central Economic-Mathematical Institute (CEMI) and Braverman at the IPU were those individuals who were insisting on building and working with mathematical models of economic processes. In 1974, "control of complex socio-economic systems" became part of an official research agenda at the IPU (Kirtchik 2019).

What kind of mathematical modeling was involved? On the one hand, Braverman was enthusiastic about data science: the techniques for finding patterns in the data, such as cluster analysis, were a natural outgrowth of his interest in automatic classification. On the other, he was seriously engaging in fix-price disequilibrium economics. Braverman found a kindred spirit in Boris Mirkin, at that time an applied mathematician working in the economics research institute in Novosibirsk. (He would later become an internationally acclaimed computer scientist.)[10]

Somewhat unexpected in this story, however, is that neither the algorithms for data analysis nor the cybernetic disequilibrium fixed-prices models were to become the focus of Aizerman's research. Instead, he turned to the field that defined the major research direction of the lab till the end of his life and beyond: *the theory of choice*.

It was Mirkin who provided the first book-length treatment of the collective choice problems. Mirkin's book was inspired by Braverman, who felt that while the lab was going to change the topic and move closer to "mathematical social sciences," no ingenious and authoritative summary of the work done in the field was available. Mirkin collaborated with another Lab 25 researcher, Andrei Malishevski, who was as fascinated with the topic as Mirkin himself and was a meticulous editor of Mirkin's manuscript. The book was completed in 1972 and published in 1974.

Inspired in part by the approach of John G. Kemeny and J. Laurie Snell (1962), the book he had translated into Russian, Mirkin (1974) treated the

8. The institute's building is not far from the building of CEMI, the major research institution in Moscow devoted to mathematical economics.

9. Vadim Trapeznikov, a head of the institute in 1951–87, participated in the public economic discussion as early as 1964 (Kirtchik 2019).

10. One of the Lab 25 fellows, Ilya Muchnik, came to Novosibirsk for a joint project on data analysis with the sociologist Tatyana Zaslavskaya (again, a soon-to-be central figure in Soviet sociological research) and quickly saw that Mirkin was interested and competent in measurement theory and classification algorithms. See the recollections in Rozonoer et al. 2018.

issue in quite a general (and much more extensive) way. For him, collective choice was a specific way to frame the *aggregation problem*, whereby the nature of aggregation could remain unspecified. It could be an aggregation of votes—which would lead to a formal political theory, or an aggregation of individual preferences—which would amount to the analysis of aggregate demand. Expert judgments, optimization criteria, or classification parameters (in factor analysis)—all these sets of data could become inputs in the system performing the "collective choice," that is, essentially, aggregation of "individual" heterogeneous data. He termed it "group choice" to stress that *collective* involves active bargaining, coalitions, and, generally, interdependency of its individual parts, while *group* is just a set of fully autonomous elements (Mirkin 2019).

Mirkin actually disliked mathematical economics, but was well versed in the theory of binary relations and was fascinated by the opportunity to use qualitative mathematics to analyze sociological and psychological data. In fact, he first encountered this problem while trying to find an approach to the economic problems he was confronted with in Novosibirsk: how to aggregate the data coming from different divisions of an enterprise and how to form an integral parameter or an index in the analysis of sociological data from surveys (Mirkin n.d.).

Mirkin's book contained quite up-to-date material on preferences, voting, Arrow impossibility theorem, as well as general equilibrium and game theory.[11] (The most recent results in mathematical economics were treated as special cases or problems in the text.) Again, decision theory here emerged out of the work in data analysis, and only in discussing game-theoretic methods did Mirkin (1974) grudgingly assume that here, when dealing with humans, we need to take into account their "reflexive" ability to change their states.[12]

This book, which became widely known in the Soviet community even beyond mathematical economics, along with a clear interest of various leading members of the lab (mostly Braverman and Malishevski) and the institute as a whole, motivated Aizerman to finally turn to mathematical economics and choice theory. However, he was not happy with this move and hoped to avoid the "ideological" field of economics (Mark Levin,

11. It was very quickly translated into English and edited by the American decision and utility theorist (and a researcher at AT&T Bell) Peter Fishburn, who at that time was also working on general choice theory. See Mirkin 1974.

12. In an introduction, Mirkin also mentions "Western" normative notions of welfare economics and its importance for a socialist system.

pers. comm.): in fact, doing "economics" and not political economy in the Soviet Union was obviously an ideologically suspect activity. Still, both Braverman and Malishevski had more than a theoretical interest in modeling human behavior and social systems.

While Braverman, in his disequilibrium analysis, was willing to create algorithms and to literally do "economic cybernetics," in line with his previous work in pattern recognition, and using the notions of feedback, learning, and adjustment, Aizerman was less enthusiastic both about immediate applications of this work and about their (essentially neoclassical) theoretical foundations.[13]

Thus, while Braverman and Malishevski served as bridging figures between Lab 25 and other institutions of formal economics in the USSR, Aizerman's style of work was quite different from standard interests and concerns of Soviet mathematical economists. In particular, he was skeptical of Leonid Kantorovich's theory of optimal planning and other models that were being developed at the end of the 1960s and the beginning of the 1970s, predominantly at CEMI. Instead, he was inspired by the ideas of Herbert Simon that were much closer to his interdisciplinary vision.[14]

What kind of vision did Aizerman subscribe to? In 1974, he gave a talk at the All-Union conference on automatic control, titled "Human and Collective as Elements of a Control System." He framed this talk as a reflection on the new tendencies in control theory, in which one moves from treating humans as controllers to looking at them as elements of the system itself. In this, Aizerman proceeded to what he called "behavioral models"—and referred to a broad range of approaches authored both by Soviet and by non-Soviet mathematical economists. In these models, the most important issue for him was the idea of a *scalar criterion in decision-making*. Whether decisions are taken following a scalar, or a vector, of various factors, could be settled only by experimental research, but Aizerman's (1975: 87) position was rather skeptical: "Although the experimental work in this field is only beginning, even the first results make

13. Braverman's untimely death in 1977 was important for Lab 25's subsequent development: although his work was taken up by Mark Levin (Braverman and Levin 1981; Makarov, Levin, and Rubinov 1995), Levin has not become a researcher at Lab 25; its research profile changed and since then was mostly defined by Aizerman and Malishevski.

14. Simon's "The Sciences of the Artificial" (1969) was translated into Russian as early as 1972, but Aizerman could have heard of Simon's approach already in 1956, during his first visit to the United States (Aleskerov 2019). Indeed, Simon's wide-ranging interests in control theory, learning processes, and rationality paralleled those of Aizerman and his team. However, I have not found references to Simon in Aizerman's published work on choice theory.

rather plausible the claim that the situations, in which one can really assume the existence of one criterion (and even several criteria!) when choosing an alternative, are relatively rare and of special nature." It is this lack of a singular criterion governing an action that, for Aizerman, distinguishes humans and groups as elements of the control systems from technical apparatuses, and thus social systems from technical ones. Other important aspects of this fundamental distinction are, for Aizerman, the possibility of opportunism ("cheating the system," in his parlance); the option to create coalitions that would defy the expectations of the system; and the ability of agents to take account of the system as a whole. Characteristically, the first two features are illustrated with the examples of contemporaneous economic theory (Aizerman refers to Allan Gibbard's [1973] theorem on the inevitability of cheating for the case of individual opportunism, and to the theory of cooperative games, in the case of coalitions), while the third one, which sounds like a version of rational expectations / efficient markets story, is only illustrated by the work of a Soviet (and US) social scientist Vladimir Lefebvre (1936–2020).

Aizerman's research in the theory of choice began—and, in a sense, ended—with addressing the most fundamental question of (micro)economics: the basic formalisms of rationality (Aleskerov 2018). The major instrument for him was, however, not the standard utility functions but the so-called choice functions, in the sense of Arrow 1959, Sen 1971, and Plott 1973.[15] While for microeconomists these issues were important primarily as a foundation for demand theory, Aizerman recognized the conceptual difficulties of what he later described as a "classical" theory of choice and preferred the more general language of choice functions. While the "classical" approach cares only for those choice functions that could be generated by a "rational" (in some sense) preference ordering and unambiguously connected to a maximization of certain utility function and/or representation of a binary relation, for Aizerman, some problems required a more general formulation. In particular, he was thinking about the examples of choice in which a simple binary comparison is not adequate, because the preferences between two alternatives depend on alternatives *from the same choice set*. Hence the choice functions Aizerman investigated, following Sen 1971, were set-valued, rather than "element-valued." Contrary to the "classical" approach focusing on how to preserve the exclusive dependence of choice on the specific features of the

15. Simply put, a choice function maps a set of alternatives (a choice set) onto the set of those alternatives that are actually chosen.

chosen alternative—and in this sense, context-independently[16]—Aizerman suggested that a more general formulation is needed.

With his collaborators, notably with Malishevski, a decision theorist and operations researcher, Aizerman created the new classification of the choice functions and the new language for the general theory of choice (Aizerman and Malishevski 1981). One of the examples they worked with were so-called hyperrelations, an object purportedly more general than standard binary relations in pairwise comparisons of alternatives. This analysis involved the possibility of comparing one alternative with a set of other alternatives (Aleskerov et al. 2003). Aizerman called it "non-classical" choice: "Classical logic of choice, which in its pure form is embodied in an abstract pair-dominant mechanism, }relies on binary structures. . . . In contrast, examples of non-classical logic of choice . . . have as their structures more complex, *n*-ary relations" (Aizerman and Aleskerov 1990: 117).[17]

Using this logic, the standard microeconomic problem of preference revelation (and, inversely, of preferences rationalizing choice) was also reformulated, with the choice being defined not by preferences reducing it to a set of pairwise comparisons but by a set of "elementary" choice functions, into which the one we "observe," the "revealed" choice function, can be decomposed (Aizerman and Aleskerov 1990: 67–68).

In the beginning of the 1980s, Aizerman started to work on the theory of collective (social) choice. When Fuad Aleskerov came to the lab in 1975, two topics were suggested to him: theory of optimal control and collective choice theory. Aleskerov had just earned a degree in mathematics, and while the optimal control theory was familiar to him (by that time, it had been already the part of curriculum), he remembered his shock after learning that economic—and, generally, social—problems could be addressed as rigorously as he felt was necessary. So Aleskerov opted for the choice

16. The key to this independence is the so-called independence of irrelevant alternatives axiom, first formulated by John von Neumann and Oskar Morgenstern (1944) and later widely used in choice theory, in particular, by Arrow (1951). For Aizerman and his colleagues (1977), analogous to this in the standard microeconomic framework was the weak axiom of revealed preference. In a slightly different context (in dealing with individual preferences and not with their aggregation), Aizerman's approach was perceived in the social choice community as rejecting the strategy of imposing the conditions of the "internal consistency of choice" independently of any context. As Sen (1993: 499) argues, "Being consistent or not consistent is not the kind of thing that can happen to choice functions without interpretation—without a presumption about the context that takes us beyond the choices themselves," that is, brings us to motivations, norms, values, objectives, and the like.

17. Pairwise domination implies that "any 'reasonably arranged' choice mechanism should be equivalent to the mechanism of choosing the 'best' (dominating) variants according to some binary relation of superiority (domination)" (Aizerman 1985: 239).

theory.[18] Together, Aizerman and Aleskerov formalized collective choice in terms of choice functions and formulated the "locality" condition (analogous to Arrow's independence axiom). Overall, their major idea was to formalize the *context-dependence of choice* in the general case, to demonstrate that the standard rational choice procedures of elemental comparisons (underlying the standard optimization techniques and algorithms as well) should be regarded as a special case of a more general construct. This allowed them to redescribe important issues in the theory of voting, such as the "menu-dependent preferences" (Sen 1995).

4. Lab 25 as a Collective Machine: The Elements of the Research Culture

How did Lab 25 actually work? To better understand its singular role in the Soviet academic context, we need to briefly touch on the research practices and modes of communication among its members.

Perhaps the most salient feature of the academic culture fostered by Aizerman was a collective way of doing research. Never a member of the Communist Party, Aizerman, who in his youth participated in one of the early communes and retained humanistic socialist views till the end of his life, was a true *communist of ideas*. For any new set of problems, a huge new research direction was explored by creating a seminar (Aleskerov et al. 2003: 112), a practice Aizerman might also have inherited from Andronov's weekly seminars on the theory of automatic control.

For Aizerman, a careful and an efficient organization of research groups belonged to the science proper (Rozonoer 2003: 245). He would often say that the value of a scientist is a value of "his" lab. Within the lab, as many of its former and current members say, there was no real competition or fear of plagiarism. Rather, the researchers cooperated and supported each other, while Aizerman himself was contributing decisively to everything written/published by the lab members. At the same time—perhaps in line with Arrow's impossibility theorem he so thoroughly investigated—Aizerman could be a rather authoritarian person, and while he focused on the maximum efficiency in the work of the lab (and supported all the members accordingly), the criteria, the aims, the direction of research in the lab were clearly defined by Aizerman alone.

18. Aizerman himself was not very enthusiastic about applying Pontryagin's optimal control theory to economic modeling and did not pursue it further himself—perhaps following, as suggested to me by Roy Weintraub, a general dissatisfaction with these topics in the West.

Another element of this culture was *interdisciplinarity*. Aizerman considered himself as someone who maps the new territory, gets some new results, and then switches to another topic. These changes of perspective that, as indicated above, could be quite radical also clearly involved constant learning. The scope was literally breathtaking: "As long as I can remember, he [Aizerman] learned the basics of mathematical logic, theory of stochastic processes, electrophysiology, principles of mathematical economics, and elements of relativist physics" (Rozonoer 2003: 246).

This learning was also collective: people were appointed to study new fields and the relevant literature to subsequently deliver lectures in small groups. This division of labor was believed to enhance academic efficiency. This type of teamwork could also be associated with some features of Aizerman's research program: to unify and codify the language in the general theory of choice.[19]

Aizerman's lab was not just growing: when it was clear that a certain topic outgrew the capacity of a single lab and the interests of colleagues began to diverge, a new lab was created, while Aizerman continued to work with a smaller collective on a set of issues that was on the agenda.

It was this outstanding curiosity and constant search for new topics that partly explains why Aizerman, unlike other Soviet operations researchers, never engaged with any big military or related projects after the war—such as missile regulation or the space program headed by Sergei Korolev (Boris Petrov from the IAT was collaborating with Korolev on that in the 1950s–1960s), and did not allow the researchers from his lab to work on them (Mark Levin, pers. comm.). These projects would have meant much for a career, but would have limited the freedom to choose the subjects for his work and to travel abroad. This was even more important in the specific Soviet context, to which I now turn.

5. Permeability and Constraints of the Iron Curtain

Soviet science was notoriously isolated from the international scene. However, this general situation and the well-known complications encountered by the majority of Soviet scientists when trying to contact—or, even more so, to receive recognition from—their foreign peers played out differently in different contexts.

19. Various individuals in a group thus started speaking the same theoretical language that further reinforced the need to streamline and standardize the terminology. I thank Salvador Barberà for making this clear to me.

Aizerman's lab was from the beginning a part of an international research network. Despite the fact that Aizerman was of Jewish origin and never was a party member, his authority both in Soviet science and among numerous colleagues abroad, as well as his visible loyalty, helped him to organize international communication and to travel a lot himself.[20] In 1960, the first huge world congress of an important association of control engineers, International Federation of Automatic Control (IFAC), was held in Moscow. At that time, its president was Alexander Letov, an engineer and an applied mathematician who was one of the founders of IFAC. It was there that Norbert Wiener gave a keynote; Rudolf Kálmán first presented the idea of the Kálmán filter, estimating the state of the system over time; a leading Soviet mathematician, L. S. Pontryagin, and his team presented their theory of optimal control and the maximum principle (Pontryagin et al. 1962);[21] and an American operation researcher, Richard Bellman, gave a talk on his results in dynamic programming.[22] At that time, Aizerman established a contact—and developed a friendship—with Bellman. In fact, Aizerman was among the first Soviet visitors to the RAND Corporation, where Bellman worked in the 1960s.

Aizerman established an international reputation quite quickly: his lectures on the theory of automatic control were translated into English (Aizerman 1963), and in the 1970s, his colleagues immigrating to the United States were getting positions in prestigious institutions.[23] After he changed the direction of his research, new colleagues were to appear. In 1967, Aizerman organized an international symposium on image recognition and related problems and gathered lots of scholars from all over the world. Since the 1970s, when choice theory was on the agenda, his international partners had been Charles Plott, who then became one of the founders of modern experimental economics, and other mathematical economists and social choice theorists (Salvador Barberà, Bernard Monjardet, and Amartya Sen). Further contacts included the mathematical economist David

20. However, he claimed that the "energy conversion efficiency" in trying to obtain permissions for travel did not exceed 20 percent (Isaev 2003: 278). Rozonoer (n.d.) explains this by Aizerman's good relations with the administrative bureaucracy of the institute whom he always invited to go abroad with him, thus creating an "incentive compatibility."

21. Despite his increasing anti-Semitism, Pontryagin was on good terms with Aizerman (cf. a letter of recommendation and evidence that he was a visitor at Aizerman's seminars in Aleskerov et al. 2003: 57, 280)—as he was with Aizerman's teacher Andronov (Dahan 2004).

22. Judy Klein, Marcel Boumans, and Béatrice Cherrier and Aurélien Saïdi (all in this volume) discuss the relevance of these authors to postwar developments of economics in the United States.

23. Interestingly, they all landed in engineering departments (Anatoliy Yashin at Duke, Semyon Meerkov at Michigan, and Muchnik at Rutgers).

Gale; the mechanism designer Theodore Groves; the game theorist and political scientist Steven Brams; and the political theorists Richard D. McKelvey, Norman Schofield, and Thomas Schwartz. They exchanged working papers and offprints, and, very rarely, would visit each other's institutions or meet at the conferences. The publication issue was somewhat more tricky: even in the 1980s, the culture of preparing and sending papers to international professional journals (in mathematical economics and social choice/decision theory) was not firmly established: many results appeared in Soviet outlets, and the work of the group was thus not really visible in the international research community. This clearly marked a limit to its integration into the mainstream.

At the same time, despite Aizerman's loyalty, the political constraints were pervasive. Suffice it to say that the letters Aleskerov and others received from abroad had been opened before they reached them. Someone reported to have seen a book sent around 1985–86 by Brams with a dedication to Aleskerov, in the semiclassified division of the INION library.[24] It was redirected there without notifying the receiver.

Of course the IPU group's interest in collective decision was anything but innocuous from the ideological point of view. Importantly, the analysis of collective decision-making quite naturally led Aizerman's group not just to social choice but also to the formal political theory—a discipline that could never be openly practiced in the USSR, where political science was replaced by "scientific communism." Two anecdotes should illustrate the contexts of Aizerman's work.

The first one is that of subversion. At the beginning of the 1970s, Malishevski, who was close to the Soviet dissidents and, in particular, to Andrei Sakharov,[25] proved a theorem demonstrating that if one uses a "total" majority rule (a group decision is taken if all members but one are in favor of it), one can always redistribute a finite amount of resource by taking it from one agent, giving most of the resource to the others, and retaining a small share with the one performing redistribution. Reiterating this procedure yields what Mirkin (1974) calls a "total majority path" (the

24. All-Union Institute of Scientific Information in Social Sciences. Brams never inquired about the book, believing that Aleskerov did not reply simply because he had not liked it (Aleskerov 2019).

25. Katsenelinboigen recollected that in the 1960s, Malishevski made a copy of the Russian edition of Samuelson's *Economics* (which was published in 1964, but the number of copies was limited and the book was available only in academic libraries) and was popularizing it among his colleagues at the IAT (Malishevski 1998: 521), as if inviting them to go all the way from *Economics* to the *Foundations of Economic Analysis*. Note that J. R. Hicks's *Value and Capital* was first published in Russian translation only in 1988.

term was Malishevski's suggestion): at each step a different agent is "robbed" by the collective majority vote, each step is preferred by the majority to the previous one, and the system itself might even become "poorer," since every time a share of resources may be appropriated by the one who manages the vote. Only high-brow mathematical formalism seemed to have saved Mirkin from the fury of censors.[26]

Another anecdote demonstrates how pervasive ideological fear was even into the 1980s. In 1981, Aizerman together with Aleskerov started to work on the issues surrounding Arrow's impossibility theorem. When discussing the paper draft, Aizerman insisted that the terms *dictator* and *oligarchy*—used by, among others, Sen (1970) and even Mirkin (1974)—be replaced. Aleskerov did not understand the reason for this excessive caution, and he remembers Aizerman eventually saying, with a note of sadness: "I have spent 70 years with this regime, and I will not allow you to ruin your life and career because of the two words that will surely be misunderstood" (see the story in Aleskerov 2005). In the published paper (Aizerman and Aleskerov 1983), the terms used are *decisive voter* and a *decisive group* (although in the footnotes, they do refer to the standard terminology).

Thus, despite the porousness of the Iron Curtain and despite Aizerman's exemplary academic reputation both inside and outside the country, the Soviet epistemic regime did pose sufficient constraints on the ways that the ideas were formulated and communicated. But what kind of knowledge did these forms of academic life eventually create?

6. Experiments: The Missing Link?

If one tries to make sense of Aizerman's—and thus his lab's—evolving interests and commitments, two aspects turn out to be worthy of attention. First, the change had a specific direction. From modeling the *mechanical* systems, Aizerman switched to the analysis of *living* systems and abstract problems of cybernetics. Then, in the 1970s, living systems receded to the background (the group working on these issues became a separate lab), and,

26. "Formalism" was an attitude that helped Aizerman as well. When asked to sign collective letters denouncing something, he found refuge in demanding materials to be able to get acquainted with the phenomenon he was asked to publicly condemn together with his colleagues. Not being a Communist Party member helped, too: he was not beholden to the "party discipline" (Rozonoer 2003: 248). This "formalistic" attitude enabled Aizerman to stay away from the Soviet collective campaigns without becoming a dissident. Interestingly, the formalism as a methodological strategy could also have nurtured the collective research culture of Lab 25—in a way similar to the logical positivist context described in Daston and Galison 2007: chap. 5. I am grateful to a referee for this reference.

after some experimentation, the focus moved to modeling *social* systems.[27]

Second, and related to that: the interdisciplinary agenda of Aizerman's lab was of quite specific nature. On the one hand, Aizerman was open to virtually any kind of problem. Moreover, he posed the problems in non-standard ways. But when it came to actually solving them, the work was following the patterns characteristic of deductive mathematical thinking: those of formalization and generalization. In various fields—from control engineering to cybernetics of living systems to choice theory, Aizerman himself and his colleagues were actually attracted by *theoretical* challenges.[28] But while the link between the pure and the applied was quite straightforward in the case of mechanical systems and more or less understandable (although somewhat less so) in the case of living organisms (the lab was involved in several projects in "medical cybernetics"—both exploring the control processes in muscles and creating the first algorithms to analyze the medical data), the social interactions posed the biggest problem. What was the approach of Aizerman and his colleagues?

As I have shown above, the ambition was to build a more general theory and to formalize the insights that suggested this generality. The apparatus of choice functions was the language Aizerman found most appropriate for addressing this problem. Thus he both questioned the validity of the "classical" rationality and used a deductive approach: from the more abstract, more general perspective that would incorporate the classical idea as a special case.

This process might seem curious for a student of twentieth-century economics, since we know that the many challenges to the standard model of rationality eventually led to the rise of behavioral and experimental economics. But neither Aizerman nor his collaborators were—or could become—economic experimentalists.

At the same time, experimental research did play a role in Aizerman's career. Experiments were part of his research activities from the beginning: to study and to regulate the pressure in the liquefied gas engines,

27. Aleskerov does not share my interpretation and believes instead that, although this rendering is accurate, it does not reveal any teleology. Rather, the change of interests was due to Aizerman's exceptional curiosity and propensity to change the disciplinary frameworks. Perhaps our positions can be reconciled in that Aizerman himself might not have seen or planned this transformation of interests in this particular way.

28. At the IPU, Aizerman also had the reputation of a theorist, which attracted to him individuals with similar sensibilities, such as Rozonoer (n.d.).

one needed to perform lots of experimental work. Aizerman's "bionics" program—the study of the control processes in animals and humans—was also based on the experiments the lab researchers performed for many years. But this was still not exactly the experimental *economics* in the form it was initiated by Vernon Smith and Plott in the United States.

In 1986, upon returning from a visit to Plott in Caltech, Aleskerov suggested that they do experiments as well.[29] Aizerman fully supported this initiative: the lab members ran several experiments and published a summary of results in a conference proceedings (Aleskerov et al. 1986). Thus Aleskerov could be considered one of the first Soviet experimental economists. However, he never went further.

Why, despite Aizerman's support and, generally, perfect initial conditions, did the group not adopt the experimental program—following the path of Plott himself, who turned from doing pure social choice theory to experimental economics? One reason is, clearly, Aizerman's own proclivities.

In March–April 1987, the Committee on Contributions of Behavioral and Social Sciences to the Prevention of Nuclear War, represented by its members, the behavioral scientists Herbert Simon and William Estes, as well as the political scientist Robert Axelrod and a couple of staff members, visited the Soviet Union and talked to the major scholars in eight research institutes, including IPU. In the report they prepared, they summarized their meeting with Aizerman in the following way.

> Aizerman's research is on the theory of choice, voting, and agenda control. It is mathematical and computer modeling work on the logic of choice, and is expanding classical rationality from comparisons of pairs (which have problems of transitivity) to more general choice functions. He is interested in the problem of organizing agendas for collective discussion so as not to manipulate voters as [at?] the USSR institutes voting procedures. Almost all the group's work is published in English as well as Russian, and some involves collaborations with U.S. scholars, such as Charles Plott and Thomas Schwartz. In response to questions, Aizerman said his group is concerned with attentional effects, which he described as the "effect of context"; no empirical work is now going on, but he

29. The 1985 visit to Caltech left some important traces. In particular, Leonid Hurwicz (1986: 75), who also presented at a conference there, acknowledged that he was inspired to pursue the choice functions approach by Aizerman and his team in one of his papers devoted to the implementation of social choice rules. Barberà (email to Ivan Boldyrev, July 6, 2019) recalls how both men quickly established contact and engaged in an intensive dialogue.

hopes to begin some. Aizerman wants to extend the work to the problem of cooperation, which he calls the "theory of agreement."[30]

So in 1987 still no empirical work was being done. While Plott was interested in testing the existing economic theories and—somewhat later—testing new policies and improving market design, in the Soviet case, apart from obvious ideological limitations, two essential factors were missing. First, there was no established community of economists potentially interested in experiments and grouped around some kind of a mainstream paradigm of human behavior/rationality (Boldyrev and Kirtchik 2017).[31] Second, there were no markets and no real competitive political system to apply any insights either from market design or from social choice models. Around 1975, Mirkin publicly asked his Novosibirsk boss, Abel Aganbegyan, whether the analysis of group choice, voting theory, and the like can ever be applied in the better design of collective decision-making—and received a definitely negative answer that discouraged him. In 1983–86, when Aizerman was presenting the first results on collective choice theory in various academic institutions, his lectures attracted hundreds of colleagues (Aleskerov 2005). The interest in formal political theory was amazing, which probably reflected the situation of growing uncertainty and growing demand for various new discourses in the social sciences (in particular in political science) so characteristic of perestroika and the first post-Soviet years.

Absent any theoretical or policy interest, the research funds to perform experiments were also missing. Despite that Aizerman, Malishevski,[32] Aleskerov, and other members of the team were driven by much more general theoretical concerns, the contexts mattered as well: once the Soviet Union ceased to exist, Aleskerov contributed to the work of the first huge center of experimental and behavioral economics in Russia

30. Report, Committee on Contributions of Behavioral and Social Sciences to the Prevention of Nuclear War, May 4, 1987, p. 11, Series VI: Consulting—1942–2000, box 48, folder 3694, Herbert A. Simon Papers, Carnegie Mellon University, Pittsburgh, PA.

31. For example, Plott (2012: 296) came to the insights very similar to those of Aizerman—but inspired by the US public choice tradition: "Buchanan was right [in his criticism of Arrow]: there is no need to restrict the theory to processes that operate on two-element sets and once that assumption is discarded . . . a completely different frontier is opened." Another example is Arrow's work in the economics of R&D and medical care discussed by William Thomas in this volume.

32. Malishevski suffered from depression and committed suicide in 1997. His and some others' premature deaths mark huge missed opportunities for new research during the post-Soviet period.

organized by Alexis Belianin at Moscow's Higher School of Economics, which became a site of collaboration between economists, psychologists, and political scientists.

7. Conclusion: The Engineering Cultures in Soviet Mathematical Economics?

Overall, Soviet economics was an isolated form of knowledge. Even in its mathematical mode, it could not be a part of an increasingly internationalizing field—be it general "economics" or its particular branches.[33] However, what was true for the whole was not true for some of its parts, such as the Aizerman group. Its members were not just well versed in the most up-to-date literature but were also contacting the "bourgeois" economists and political scientists and publishing in English. Despite all the constraints discussed above, Aizerman's lab was one of the most internationalized groups in Soviet mathematical social sciences. By the beginning of the 1980s, it had already established a reputation among social choice and decision theorists. Although Aizerman's team has never been a group of "full-fledged" economists, the work it was doing stood on a par with the most advanced economic and decision theory in the West.[34]

So why did this group of engineers embark on the study of economics? The story told here has revealed a plethora of different motivations—the sheer curiosity and the feeling that new results may be obtained; the ambition of reformulating the whole theory of choice and thus the foundations of the behavioral science; the search for new methods in data analysis; and the move from the cybernetics of mechanical and living systems to that of a social system. Of particular importance was Aizerman's tendency to regularly and entirely reorient his research agenda.

It is curious, however, that Aizerman and his colleagues, being open to virtually any kind of question and keen to do interdisciplinary work, produced for the most part results that were very abstract and followed the mathematicians' quest for ever higher generality and formalization—working on the *foundations* of (micro)economics and rationality. This work could be placed at the heart of the formal theory discussed in the

33. Several notable exceptions (see, among others, Boldyrev and Kirtchik 2014) only confirm the rule.

34. The group members were not the only ones doing social choice theory in the USSR, but the other authors were isolated and driven by other (sub-)disciplinary contexts. See Aleskerov 2005.

aftermath of Arrow's impossibility theorem. As a political theory, however, it sounded even more subversive for the Soviet context than "mathematical economics," which still could legitimize itself with the recourse to Kantorovich's theory of "optimal planning."

The formalisms suggested by Aizerman's group, although being critical of the "classical" rationality, which nowadays seems to be abandoned, and moreover, being accepted by the economics mainstream, never really gained traction and have not become a standard in the social choice literature.[35] Aizerman also could not—and perhaps did not want to—be a part of the "experimental turn" in economics, although this type of "economic engineering" could have been the way to fully integrate his and some of his colleagues' engineering background, on the one hand, and their theoretical aspirations, on the other. To engage with experimental markets and to apply the insights of mechanism design—the very essence of economics as "social engineering"—were hardly possible in the period of Soviet stagnation.

Thus, judging with hindsight, one of Mark Aizerman's most successful (social) engineering achievements was the team he established at Laboratory 25. After his death, Lab 25, headed now by Aleskerov, remains one of the most productive centers at the IPU and belongs to the very few research groups in the post-Soviet world actively working on formal political science, social choice, and decision theory.

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35. One reason might be, as Sen (1995: 95) argues in the context of voting theory, that "using choice functions as inputs is much more demanding on voters and on the system of 'counting' than is the use of rankings of each voter with menu-dependence assumed away. In fact, using choice functions as inputs may be infeasible in practice in many types of exercises." See also Aizerman and Aleskerov 1986 (this paper first emerged as a Caltech working paper); Aleskerov 2002.

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