Conclusion

Rationality Found and Lost? In Search of a New Historical Narrative of Optimal Planning

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The history of economic thought under communism can be portrayed as a long chain of human disasters. Economic theorists could end up in jail or be executed for a policy idea, a scientific method, or just a phrase blacklisted by the censors. In brighter times, repression "only" led to forced emigration, employment and publication bans, travel restrictions, and harassment at the workplace, which also could result in illness or death. Mathematical economists suffered from such sanctions until the late 1950s, and even afterwards. From the 1960s, their *métier* became much freer, attaining, in the worst case, a status of a semi-official discipline. Thus, the specter of tragedy invoked in the Introduction may seem like an exaggeration—unless it referred to the first postwar generation of mathematical economists in the Eastern Bloc. They were crestfallen after experiencing one failure after another in advancing optimal planning, their signature research program, which they hoped, with Panglossian optimism, would establish the best of all possible worlds in universal economic science.

Interestingly enough, while some disenchantment does transpire from the reminiscences of the elite of optimizers, their personal accounts seldom contain much self-criticism such as: borrowing the theory of general equilibrium, I ignored its philosophical and methodological underpinnings; I underestimated the Mises-Hayek arguments on the impossibility of rational economic calculation in a collectivist system; I put too much trust in the improvability of the planning regimes; I was blinded by the highest-level acknowledgement

coming from the West; I made too many concessions to the Central Planner and got stuck with the research program even after I had known that it was hopeless; I neglected cooperation with the market reformers and did not use my mathematical knowledge to work with them on a new theory of the planned economy, which could have relied on a critical analysis of its institutions including the party-state.

It can be heartbreaking for scholars to face the ruins of their lifetime achievement. But what explains that, until today, a majority of historical analysts have pulled their punches when writing the history of optimal planning? A systematic overview of the literature of the past sixty years should elucidate such a discretion and assist the reader in deciding whether our research group has managed to go beyond the state of the art. This will be a fairly unconventional review: instead of a dry summary of the main arguments, I will initiate conversation with my fellow historians.

CIRCLING AROUND THE SOCIALIST CALCULATION DEBATE (AND THE COMPUTER)

The positive biases of the first observers[1] originated in a long-awaited turn in communist economic thought, which liberated numerous groups of gifted researchers suffering under oppressive regimes. The usual orientalist prejudices were moderated by the respectable traditions of mathematical economics in Russia and the Soviet Union and the scientific discoveries made by scholars like Leonid Kantorovich, János Kornai, and Oskar Lange parallel to their counterparts in the West. In fact, early observers aired some concern about what might come after the stage of "hurray, optimal planning is here." Nevertheless, as indicated in the Introduction, they did not find fault with the lopsided (technique-oriented) takeover of the neoclassical paradigm, ignoring the Socialist Calculation Debate, or the statist leanings of the optimizers. They were also fairly uninterested in the ambiguous relationship between the "plan improvers" and the market reformers and insensitive to the principal moral dilemma of many Marxist (and a few non-Marxist) mathematical economists who cooperated with the communist government. These economists were tormented by the following: what if the leading communist officials agree to our advice and the planned economy turns into a *perpetuum mobile* supporting authoritarian rule with our help? In an era of "cyber-optimism" generating dreams about vast automated control systems, this did not seem an unfounded worry.[2]

In a sense, sympathy was understandable, and not only because of the Eastern European provenance or socialist commitment of many Western analysts.[3] Besides the hopes of convergence, the rise of the Soviet School

of Mathematical Economics and its influence upon the Eastern European research communities raised hopes about progress in economic thinking in the Soviet empire, notwithstanding the publication of the notorious 1954 textbook of political economy (Ostrovitianov et al. 1954). Peter Wiles' (1964, 16) sarcastic remark about optimal planners who would like to replace perfect competition with "perfect computation" was a rare bird. The sympathizers argued the following way: Soviet-style pseudo-scholarship has an encouraging alternative at last; its followers use our professional economic discourse but they are not rootless in their own scientific environment.[4] In Alfred Zauberman's (1975, 9–11) words, they had left behind the "five fingers plus abacus" technique long before. An authentic scientific movement came into being (powered by the *shestidesiatniki*, the generation of the 1960s), but—despite all support rendered by the Kremlin and symbolized by Stalin and Lenin Prizes—its protagonists faced resistance by both academia and politics time and again. Even those among the benevolent observers who were less enthusiastic about mathematical planning thought that things could not get worse than they had been under verbal planning safeguarded by the official political economy, the "discoveries" of which did not exceed Stalin's "basic law of maximum satisfaction of society's needs." They trusted in a (never materializing) future, in which planning would have a proper theory at last and no *apparatchik* can say that two plus two is five because of Party demands. John Michael Montias (1967, 244) went as far as to predict the imminent end of separation between Eastern European and Western economics.

Justifying a "Revolution"—the Founding Narratives

Let me first bring the example of seminal works published by three leading analysts from the West: Alfred Zauberman, Michael Ellman, and Pekka Sutela who dominated the scene of historical analysis of Soviet planning concepts from the 1960s. All of them were extremely knowledgeable about the field and well-supplied by their Soviet colleagues with insider information. Although they did not follow closely the evolution of mathematical economics in other Eastern European countries, their books stand out from the sea of journal articles of the time.[5]

Zauberman who, in the 1950s, had pioneered an interpretation of the *oeuvre* of Soviet mathematical economists, introduced the romantic term "mathematical revolution" in 1975[6] to signal the birth of a new methodology in Soviet economic thought, with a special emphasis on the research program of optimal planning (which he earlier called "planometrics"). He put faith in the program's "organic development" (Zauberman 1975, 52) irrespective of the fact that in the first half of the 1970s it entered, as he said, its "post-elation" phase (41). The pinnacle of criticism in his case was a mild disagreement

with Kantorovich who had hoped that the application of mathematical methods in central planning would result in a quick rise in the national product (45).[7] Instead of speaking of a failing project of social engineering or a utopia, Zauberman thought to witness just some unavoidable skepticism in the research community due to exaggerated expectations generated during the 1960s (52). Otherwise, he was convinced that the optimal models grew in sophistication, the computers became faster and faster, the ideological brakes got weaker, and the mathematical breakthrough was irreversible, also because Soviet economic science sought to become international (47). Official political economy was on its way to be pushed aside by the "relatively exact" discipline of mathematical economics (43–44). This would not make the labor theory of value disappear (19–20) since marginalism emerged in the USSR as a method of computation rather than a "subjectivist" philosophy of economic calculation. Apparently, the question of whether the application of the new algorithms could unleash (not just a mathematical but also) a neoclassical revolution at a certain point did not interest Zauberman.[8] He spoke of rationality in the context of a simplistic scheme of minimum costs versus maximum benefits (19), which would bring the concept down to earth from Marxist-Leninist political economy that considered rational economic behavior an innate property of the Central Planner (2–3).

Zauberman knew that (a) most of the optimal models were either mathematically correct or realistic; (b) in the Eastern Bloc not a single one- or five-year plan was built on optimal schemes (at most, the consistency of certain parts of some of them was checked by these). Nevertheless, he presumed that all was not lost that was delayed, hoping that even the political/ideological obstacles of the Brezhnev era would dwindle because the regime was doomed to boost productivity. Accordingly, the notorious lack of truthful economic information was not an unsurmountable *quality* problem originating in the very core of the planning system but a provisional difficulty owing to the still too large *quantity* of data demanded by the models and to the "inertia of the planning and controlling apparatus" (52) causing lags in providing the necessary data. Yet, healthy incentives and "closer and closer collaboration between the Soviet planner and the scientist" (53) should help. In sum, all economic actors involved with planning were portrayed as benevolent warriors of a common good, and "informational incongruities" (i.e., not severe systemic distortions) stemmed from organizational and cognitive bottlenecks rather than powerful vested interests at all levels of the planning hierarchy (41).[9]

The author did not cast serious doubts even on the most daring endeavor of mathematical economists in the Soviet Union, the establishment of a nationwide automated (self-adjusting) system of planning and control (35–36). According to him, the ultimate guarantee for success was the gradual shift in official economic theory thanks to co-opting the winning combination of

input-output analysis and linear programming. Undoubtedly, these models brought along intricate problems related to aggregation, the lack of dynamic and stochastic approach, insistence on linearity and so on, but these were, in Zauberman's view, purely *technical* difficulties that certainly would be overcome by the evolution of mathematics and computer science. Sooner or later, the Central Planner would be unable to ignore what *logically* derived from the process of optimization, namely, concepts such as "equilibrium," "social utility," "shadow prices," and "duality" in general. These would support the marketization program of verbal reformers advocating the monetization and decentralization of the planned economy (19).[10] The "market or computer" choice was out of date (34–37). The models would produce a world of consistency and feasibility, forcing the economic actors to "declare their hands." Even if the supreme leaders continued to take the final decisions behind the scenes, they would have to choose from among mathematically viable alternatives (18). They would need to refrain from rule of thumb and *ad hoc* decisions as well as from an obsession to overfulfill the plans, which would upset the harmony of optimality.

During the 1970s, Michael Ellman took up the torch from Zauberman in a less optimistic mood, sharing Kornai's skepticism about the neoclassical underpinnings of optimal planning. As early as 1973, he reproached Soviet mathematical economists for going too far in trusting in general equilibrium and the healing force of the market as well as attributing too little importance to mobilizing social support for their optimal plans (Ellman 1973, 176–90).[11] For him, mathematization without a proper neoclassical turn was not an odd episode to be explained but a desirable combination: he demanded less Walras (129) and more Keynes and Marx (179, 182–83) in terms of economic theory to attain, in the end, Jan Tinbergen's ideal of indicative planning in both the East and the West.[12] In this respect, he went far beyond Zauberman. Ellman overlooked the concessions the mathematical planners made by accepting the privileges of the Central Planner in defining the economic policy priorities of the optimal models and reserving the right to diverge from the plans at any time. To him, most of the optimal planners seemed to be reform-minded experts, covert or overt adherents of *khozraschet* as if Evsei Liberman and Leonid Kantorovich had merged into one individual.

Ellman saw the imperfections of verbal planning clearly but—similar to Zauberman—did not take the Austrian-style reservations about the rationality of "collectivist economic planning" seriously.[13] In Ellman's eyes, economic rationality was not threatened by optimization models that lacked vital information and were built on severely biased data and absurd mathematical assumptions such as the linearity of programming procedures and fixed coefficients of the I-O models (31), but rather by the fact that the optimizers were actually inconsistent market reformers. Allegedly, they wanted the planners to

apply capitalist categories like price, wage, interest, profit, and rent provided by their model calculations, as well as the resulting "objectively determined valuations"[14] of the resources, while other segments of the planned economy (above all the institutions including the incentives) would remain the same (57). Hence, a hybrid system would come into being, combining the disadvantages, as he wrote, of both the "administrative" and the "*khozraschet*" economies: waste, rigidity, and technological standstill with weak growth, slow structural change, and rising social inequality.

Discussing the social base of optimal planning, Ellman was appreciative of the state bureaucrats. He contended that optimization represented the vested interests of experts who wanted to crowd out the *apparatchiki* from the planning process despite the fact that this group administered "society as a whole and thus had to place the requirements of society as a whole above its own sectional interest" (136). Thus, optimal planning was depicted as an ideology rather than a scientific undertaking (139, 179) that included outmoded propositions anyway. Allegedly, it suffered from a "hypertrophy" of market orientation and rational organization of production, focusing on allocation and choice instead of growth and social cohesion (100, 178). "It was an attempt to replace one doctrine, political economy, which provides the ideological legitimation for rule by the bosses, by another doctrine, optimal planning, which legitimizes the rule of the white-collar intelligentsia" (141). In this sense, he also lamented the heavily mathematical discourse and the quixotism of the researchers who might have sought stronger social backing of their program, for instance, by lobbying not only for "the old bourgeois liberal program (civil liberties)" but also for workers' self-management (126, 175).

Insisting on an impartial interpretation, Ellman raised doubts about both the computer and the market, challenging the project of a nationwide automated control system while also blaming a few radicals (and disregarding the moderates, an overwhelming majority by the way) among the optimizers who, in his opinion, risked social polarization, inflation, and unemployment by introducing shadow prices, taxing capital investment, or demanding the closure of loss-making enterprises. These radicals (scholars like Igor Birman or Viktor Volkonskii), he claimed, did not even shy away from advocating a transition from directive to "consultative" planning, thereby irritating the "responsible officials" (127).[15] Yet, "if enterprises were simply instructed to maximize profits and given a free hand, the experience of capitalist firms suggests that they might well operate with considerable waste and inefficiency" (54). In the thick of such criticism, the reader can hardly find praise about the benign effects of the optimal planners' research program in enhancing the efficiency of investments in certain branches, improving production schedules and the location of industries, reducing shortages, stocks, and waste, as well as in enabling the Central Planner to temper taut plans and choose among plan variants (189–90).

Among the founding narrators Pekka Sutela (1984) was perhaps the most cognizant of the stagnation and decline of optimal planning in the Soviet Union during the 1970s. Prior to *perestroika* that depreciated mathematical economics and rehabilitated market reform, he nuanced Zauberman's concept of a "mathematical revolution" by emphasizing the continuity of verbal research programs. He challenged those analysts who squeezed the researchers into two camps: mathematical economists and textbook political economists. While Ellman spelled out the reformist inclinations of the former, Sutela stressed that the political economists also approached Western economics by accepting certain ideas of market reform. However, he disregarded a third camp, more influential in some other communist countries, the camp of verbal reformers who had left official political economy behind yet resisted the temptation of mathematics.

Did anything change that explained Sutela's detached attitude that lacked both Zauberman's admiration of and Ellman's suspicion about optimal planning? Yes, with time, it became clear that any refinement of the macro-level optimal planning models was insufficient to convince the Central Planner to implement them, trusting that their economic benefits would balance their political costs. The late Brezhnev regime was not ready to launch a marketization project similar to the Kosygin reform in the 1960s, which could have been combined with the optimization of planning on various levels of the economic hierarchy. Moreover, the institutional buildup of mathematical economics also slowed down during the years of *zastoi,* and the main strongholds and leading scholars of the discipline were arguing with each other persistently. Although Zauberman and Ellman did not cease to follow these developments during the 1970s and 1980s, they failed to revise their attitudes.[16] Thus, it was Sutela who realized that the program of optimal planning actually had withered away and only the fingernails of the dead continued to grow. "It is difficult not to judge [the program] as a failure," wrote Sutela (1984, 203) politely.

He was barely interested in the mathematical intricacies of SOFE (System of the Optimal Functioning of the Socialist Economy) that became the new official label for scientific planning in the Soviet Union by the 1970s. Rather, he wanted to examine the political economy of the program by focusing on the cultural background of its creation. As he put it, if earlier works "have . . . regarded SOFE as an alien body within Soviet economics, this study weighs the scales in the opposite direction. SOFE is regarded here . . . as part of Soviet Marxist thought (12)." In Sutela's—iconoclastic—opinion, the neoclassical principles of optimal planning fit well with official Soviet political economy (which he considered a pseudo-science), mainly because both

had a strong normative thrust and propensity for social engineering (87).[17] In contrast to what was customary to assume at the time, he blamed the neoclassical paradigm for its inherent normative bias toward the plan.

Accordingly, the optimal planner and the textbook political economist equally claim to be able to define a Good Society, the economy of which is rationally organized, balanced, and maximizes some sort of social utility. The former devises, under the supervision of the rulers, the objective function of a programming model while the latter—like Stalin—the "basic economic law of socialism." Nevertheless, Sutela did not mind that within this framework of official political economy SOFE proved unable to develop its neoclassical features into a full-blown theory because—following Kornai—he was convinced that such a theory could not meet the triple requirement of being theoretically sound, realistic, and also acceptable for communist rulers. Apparently, while Ellman denigrated the optimal planners by presenting them as agents of some erratic marketization, Sutela tarnished their fame by presenting them as experts who were deep down textbook Marxists, if not diehard Stalinists (116).[18] He knew that both political economy and mathematical economics had multiple shades in Soviet scholarship. However, he did not bother to engage in a thought experiment about the opportunity for optimal planners to modify their research program to become genuinely neoclassical (aborting, for instance, the labor theory of value), less normative, and more instrumentalist and even perhaps more realistic.

Sutela carefully mapped the institutional environment of mathematical economists in the Soviet Union, focusing on the political context of their scholarly work. However, he did not explain why these scholars failed to elaborate a coherent—mathematically equipped—theory of the planned economy despite the fact that, as he noted, they had already started checking the applicability of game theory to such an endeavor. Why did they stop short of exploring the institutional conditions of the planning process if—as top advisors—they were daily winners or losers of conflicts among the various power centers of central planning and knew the interests, strategies, and routines of the economic actors firsthand? Why did they not venture to trace the institutional games of planning, ranging from petty bargaining over pieces of information serviced by the firms, through the ongoing improvisation of the planning bureaucracy and its arbitrary intervention into model building, all the way up to the *placet* given by the Politburo to the five-year plan (or to the changes in the mandatory planning targets two months later)? Why did the optimal planners shut their eyes to an orgy of irrationality that did not recede for decades? Were they scared, tired, or both, or did they trust in incremental improvements or—on the contrary—in a gradual delegitimization of central planning as such? Did they believe that their mathematical algorithms (stochastic methods, simulation and so forth) would be able to cure the millions

of fake data fed into their models (126–27) or discipline the economic actors who were eminently interested in secrecy, cheating, and falsification? Did they expect the Central Planner to be happy about the curtailment of its own power and to disclose say, the statistics of military production to the model builders in the hope of receiving a less inconsistent five-year plan in exchange? Did the top rulers have reliable figures about top-secret matters at all? Why did a number of optimal planners begin to be attracted by radical market reforms during the 1980s (107) and work on econometric rather than programming models?[19]

Sutela let most of these questions pass, although he got very close to the answers in discussing the selection of planning goals by the optimal planners. He recognized that the choice of the objective function of the programming task was a crucial criterion for the intellectual historian in identifying the position of mathematical planners on the axis stretching between being an opportunistic advisor to the communist regime and its brave critic. Sutela reported that, even in the early 1980s, the majority of "Sofeists" agreed to the party's leading role in determining the common goals of society (98). Only some experts like Aron Katsenelinboigen and Nikolai Petrakov proposed that either the model itself should generate the objective function or citizens at large should do so through their market preferences and/or following some democratic procedure (cf. "compositional" versus "decompositional" goal formation [187–88]). The latter solutions would have been tantamount to a kind of liberal-democratic decision-making (like in the propagandistic ideal of Yugoslav self-management in the 1970s[20]). Sutela considered these options so unlikely to materialize in the USSR that he did not pay special attention to them.

As regards the future of SOFE, Sutela's interpretation was pessimistic but permissive. According to him, the research program "became an appendix of traditional planning methods, a compensation for the economic reform that had miscarried" (121). "It has really not shown what an optimal socialist society might look like. It has certainly not provided for a strategy of transition to such a state, nor has it persuaded Soviet decision-makers of the need and possibility of such a transition. Furthermore, it has not provided us with an economic theory of really existing socialism" (203). All criticism notwithstanding, he did not deem the research program theoretically flawed *sui generis* but only infeasible in the context of the Soviet planned economy of the time. He alluded to a chance for continuation with these two cryptic sentences: "The basic alternative to the normative and abstract SOFE would certainly be a positive and critical social analysis but there is no evidence of circumstances having become any more favorable for such an orientation. SOFE may now be seen as a dead end, but finding a workable new course may prove difficult" (154).[21]

Austrian challenge

As we will see, some components of the founding narratives determined the way in which history-writing has approached optimal planning until today. Continuity was not broken, even by powerful interventions by members of the New (or Contemporary) Austrian School of Economics during the 1980s and 1990s. Yet, they did their best to reinstate the Mises-Hayek thesis into historical analysis, noticing that optimal planning revived the promise of rationality, which they thought had been disproved by the "old" Austrians for good in the Socialist Calculation Debate almost half a century before. This was a vital probe indeed because the School's propositions about the impossibility of both rational calculation and exact computation of the state of equilibrium were contested by cutting-edge models of optimal planning and the rapid development of computers. In addition, unlike in the 1930s when the Lange models were less mature in mathematical terms and enchanted only a few specialists such as Abba Lerner and Fred Taylor, the Soviet School of Mathematical Economics used highly complex algorithms and had a considerable entourage among scholars and state officials in the Eastern Bloc. The latter were willing to engage in large-scale experiments to optimize central control of their national economies. Finally, the heavy artillery deployed by Mises against the labor theory of value seemed to become expendable as many optimal planners slowly let go of this theory.

The arrival of new, technically well-equipped and politically influential discussion partners did not prompt Mises and Hayek or neo-Austrian scholars such as Don Lavoie and Peter Boettke either to prepare a comparative historical survey of the real-socialist planning concepts or to map the mathematical features and the political/sociological background of those concepts.[22] Instead, they revisited the key message of their own school,[23] the emphatic rejection of the possibility of rational calculation (planning). They tried their best to protect that message against the pro-Lange discourse of eminent Western economists such as Abram Bergson, Frank Knight, Joseph Schumpeter, and Benjamin Ward, a discourse that enhanced the legitimacy of Eastern European optimal planners a great deal (Lavoie 1981). The decline of optimization attempts did not surprise the neo-Austrians at all. This was what the Austrian School had always expected to happen. Thus, the members of its new generations did not feel the need to write the second act of the drama of optimal planning. How could we speak of a tragedy, they might ask themselves, if the hero's aspirations were fatefully flawed from the very outset? Why should we indulge in dissecting the new calculation procedures (be they "non-competitive" or "competitive") if Mises and Hayek had already proven the basic fallacies of any such procedure?

Similarly, the neo-Austrian experts were uninterested in the scope and quality of neoclassical elements in the optimal models since they did not hold the general equilibrium paradigm in high esteem.[24] They missed a dynamic/evolutionary and institution-centered view of the economy, which focuses on property, incentives, entrepreneurship, and the like and finds disequilibrium where mainstream economists search for perfect equilibrium, nothing else (cf. Lavoie 1981; Kirzner 1988). As a final trump, they repeated the Hayekian question addressed to Lange in the 1930s: why bother with simulation if real thing exists? Why fabricate a (less efficient) socialist market if one can borrow one from a (more efficient) capitalist economy? Ironically, they extended their doubts to the market reformers who actually were quite close to them in terms of favoring institutional analysis, praising rivalry and entrepreneurship, and playing around with private ownership. With no scruples, these reformers were put under the heading of "social engineers" next to the optimal planners.[25]

Reinstating the Mises-Hayek arguments implied a rearrangement of its internal proportions. Like the labor theory of value, the issue of computation lost its former significance. Owing to the progress of electronic computers and the invention of decomposition methods, the thesis of the impossibility of *computing* the state of equilibrium was overshadowed by the impossibility of *calculating* it. The neo-Austrian theorists also bracketed the old—fairly scholastic—debate whether rational calculation was deemed by their predecessors impossible in theoretical or practical terms or both. Rather they reached for the reasoning of Mises and Hayek, claiming that all efforts of optimization stumble upon a lack of reliable data.[26] In a planned economy (a) the actors, be they planners or those whose economic behavior they plan, are interested in concealing and distorting information due to their respective shares in informal property rights in the world of formally social ownership (Lavoie 1985, 143–44, 173–78; Boettke 1995; Boettke and Anderson 1997); (b) even if—against their own incentives—they were willing to provide accurate statistics, they would be unable to do so because a large part of economic knowledge/information, for example, data on change in technology and consumers preferences, are by definition inarticulate, tacit, contextual, or simply unavailable to them (unlike in a capitalist economy where these data emerge and spread in the market process, i.e., in a competition between agents of private property) (Lavoie 1985, 103–104, 160–61, 171–72; 1986, 8–10); and (c) even if they possessed true-to-life information ex ante, these pieces of dispersed (local) knowledge could only be centralized with the help of market prices.[27] However, provided that the authors of the planning models want to avoid these crucial quandaries by simulating the market, they must tell how exactly the process of simulation is to be organized. How will the Walrasian *tâtonnement* function in the real world?

The Austrians had regarded Lange as just another Marxist utopian thinker already in the 1930s. Witnessing the rise of hopes for computer-based planometric control from the late 1950s, they could not but smile when they saw he had taken it much further in the meantime. Famously, Lange ([1964] 1967) claimed that the market works as an obsolete computer coordinating supply and demand in a cumbersome way.[28] In response, the neo-Austrian analysts refined Hayek's views on the essential "unrealism" resulting from the artificial design of communication between the planning office and the companies, that is, of the trial-and-error process that was assumed to clear the market. They asked, for example, how central plans could adjust flexibly to changes in the economic environment if production started only after all iterations of matching supply and demand were completed and the plans were supposed to remain untouched until the new series of iterations were terminated. How can the optimal planners feed data into their models, when much of the data only emerge (have to be discovered) during the very implementation of those models? This paradox suggested that the truly impossible undertaking would not be the solution but rather would be the formulation of the simultaneous equations of the programming tasks (Lavoie 1985, 91). As a final blow, referring to Leonid Hurwicz, they added that in dual systems, such as the one devised by Lange, it is the plan that would adjust (*ex post*) to the market and not conversely as expected by the optimal planners (95).

In the liberal *Zeitgeist* of 1989, discussions on optimizing the central plan became a research topic almost as untimely as the controversies about improving mercantilist regimes in the eighteenth century. When at the beginning of the new millennium, the tide turned and the communist past regained some academic interest, the historians already lived in another *Zeitgeist* that was often critical of liberal doctrines. However, those who disliked the Austrian arguments have proved unable to integrate and complete the founding narratives to explain why and how optimal planning actually failed. They tried to provide a richer history of the research program by amalgamating economic, political, social, and intellectual history-writing as well as applying "thicker description" and "closer reading." Nevertheless, their works suffered either from anti-neoliberal resentment or—on the contrary—from forced impartiality.

A "Neoliberal Conspiracy"

The stubborn attempts at optimizing central planning started rehabilitating key notions of neoclassical economics such as rationality, scarcity, choice, marginal utility, equilibrium, that is, notions that almost had been eradicated at the end of the Soviet twenties. Following 1989, the process of rehabilitation gained momentum. The upsurge of neoclassical economics under

post-communism was an enormous accomplishment (regardless of whether one liked it or not) after decades of indoctrination against "subjectivist economic theories." A witch hunt seemed to end, which connected Nikolai Bukharin's ([1919] 1927) vitriolic assault on the "economic theory of the leisure class" with the last—maybe less arrogant—textbook of political economy published in any of the communist countries in the second half of the 1980s.

A peculiar novelty in the post-1989 literature on the evolution of optimal planning was the appearance of authors like Johanna Bockman (2007; 2011; 2012; Bockman and Eyal 2002; Bockman and Bernstein 2008) and Gil Eyal (2000; 2003) who did not consider the landslide victory of neoclassical thought in Eastern Europe during the 1990s a laudable development at all. They reinvented Ellman's arguments against the "hypertrophy of market orientation" under the influence of the writings of Philip Mirowski (2002; 2009; Mirowski and Plehwe 2009) on neoclassical economics (especially its links to cybernetics) and on what he described as the "neoliberal thought collective." They also borrowed heavily from the anti-neoliberal literature of the early 2000s produced by scholars such as David Harvey, John Kelly, Dieter Plehwe, and Monica Prasad. Fearing the advent of a "neoliberal hegemony," Bockman and her co-authors were captivated by two—alleged—traits of neoclassical theory: its socialist origins and evolution into neoliberalism. They challenged neoclassical economics *not* on Austrian grounds[29] but because they assumed that neo*classicism* cultivated by mathematical economists in the communist era had been a catalyst for the revival of the Mises-Hayek tradition often labelled by them nonchalantly as neo*liberalism*.

No matter how far they left behind the earlier narrators of the optimization story in terms of research methodology, these analysts did not tell the second part of the story. In their view, optimal planning was sentenced to death at the moment *Homo Oeconomicus* (in whatever disguise) appeared in the first models of the research program. Like Ellman, they lamented that—although general equilibrium theory also can be used to justify the rational allocation of resources by the state—it paved the way for the planned economies to the capitalist market as a result of cooperation (bordering on conspiracy) of academic, economic, and political elites, both Eastern and Western.[30] Allegedly, these wove strong transnational networks cross-cutting the Cold War divide. Consequently, state fundamentalism was replaced by market fundamentalism, instead of choosing a "third way" that—unlike market socialism—would be immune to capitalist temptation.[31] Moreover, the pre-1989 liberal awakening in Eastern Europe and China (however sluggish that had been) came to be regarded by these observers not only as a manifestation of neoliberal wrongdoing but also as one of its sources and testing grounds.

The authors of this strand mostly were uninterested in the twists and turns of the evolution of mathematical economics. They put the optimal planners in the same pigeon hole as the market reformers whom they also considered proto-neoliberal thinkers. According to Bockman (2011, 1), the neoclassical theorists in Eastern Europe were exploited if not cheated: "neoliberal capitalism was a parasitic growth on the very socialist alternatives it attacked." To increase confusion, she called these theorists socialists or leftists (whatever these words mean). Allegedly, they eagerly wanted to have their research program "translated" (Latour) into mainstream neoclassical economics in the West and, at the same time, to catch up with that mainstream, which was—somewhat paradoxically—co-produced by them in "Eastern Europe as a laboratory for economic knowledge" (Bockman and Eyal 2002). To put all this in the language of cultural anti-imperialism, they were depicted as self-made "Reagan robots" (Bockman 2011, VII) who, obsessed with the goal of self-colonization, did not realize that Western neoliberals used them as useful idiots to prove the popularity of their own teachings. This interpretation overlooked the expressly collectivist/statist attitudes of the mathematical planners (and the fact that they often obediently advised communist leaders). Alternatively, it was presumed that these advisors, just like supposedly all neoliberals, loved strong states led by authoritarian-minded "social planners" if those pursue free-market policies (218, 220).

These analysts were right to assert that seen from a global perspective "the majority of mainstream neoclassical economists have not advocated neoliberalism" (215). Furthermore, they also claimed correctly that Eastern European optimizers contributed to the development of the neoclassical paradigm in certain fields. Yet, it might have been sound to refer to the split egos of these theorists and portray them as *half-hearted* importers or (re) inventors of *selected* neoclassical ideas rather than full-blown Walrasian thinkers. Undoubtedly, through general equilibrium theory one could borrow the *language* of market competition and rational calculation. Nevertheless, according to the creed of the overwhelming majority of optimal planners, in the real world both competition and calculation could be organized by the communist state as well, and moreover, better than by the capitalist market.

Even with such limitations, the thesis of the neoclassical-neoliberal nexus seems to be a huge overstatement. To put it bluntly, should we suppose that those, who the day before yesterday had begged the communist Central Planner to apply shadow prices, asked the "neoliberal social planner" to privatize the pension system yesterday? It would be, I believe, a more plausible assumption that it was not the minority but only a miniscule faction of optimal planners who could not wait to see the coming of "neoliberal dictators" ready to follow their advice once the communist dictators fell. Similarly,

is it not a hasty generalization to equate communist authoritarian rule with early post-communist liberalization even if it was directed from above?

Revisiting the Soviet Case

Approaching our contemporary period, one encounters a growing number of historians who seem somewhat dissatisfied with the militantly anti-neoliberal discourse of researchers like Bockman and Eyal (Leeds 2016a, 369) but agree with them on refuting the widespread truism that both neoclassical and neoliberal economic ideas were imported from the West.[32] Trying to prove the "homegrownness" of these ideas in the USSR, they also reveal political and sociological curiosity and explore plenty of archival and oral sources. As ex-post participant observers, they often portray the research strategies and institutions of the mathematical economists with anthropological precision. Nevertheless, they can be reproved for being "completely apolitical. . . . What is lost in this cultural-institutional sociology of science is the sound of the grinding wheels of institutional competition, political coalition building, and their associated economic outcomes" (Feygin 2017, 214). To be sure, the criticized members of the group adhere to the founding narrators not only in forming political opinions cautiously but also in an insightful and accurate reading of original texts.

The group includes younger scholars such as Ivan Boldyrev, Till Düppe, Yakov Feygin, Olessia Kirtchik, Adam Leeds, Benjamin Peters, and Eglė Rindzevičiūtė but also more senior scholars like Vincent Barnett, Richard Ericson, Slava Gerovitch, Wade Hands, and Joachim Zweynert. Many dozen cross-references as well as several joint publications and conferences show a remarkable intellectual cohesion among them. Working on the evolution of economic thought in Russia and the Soviet Union, many of these analysts focus on mathematical economics, with a special interest in cybernetics and, in turn, optimal planning. They borrow a great deal from Slava Gerovitch (2002), Philip Mirowski (2002), Roy Weintraub (2002), and Erickson et al. (2013) and attribute a great importance to the Cold War in modernizing economic thought in the Soviet Union.[33]

In their writings the optimal planners are not portrayed as steadfast Western-type neoclassical thinkers who in the second half of the 1980s finally gathered enough courage to show their true colors as neoliberals. Boldyrev and Kirtchik (2017, 6–8), for example, coin the term of "latent neoclassical" economists and Leeds (2016a, 51–58) writes about "spectral liberals" to show the ambiguities and intellectual constraints of the research program. Boldyrev and Kirtchik (2014, 436) argue that the Walrasian paradigm of general equilibrium could not be "<simply> extended to a different intellectual space . . . extension requires a work of interpretation and adaptation to a new context."

Hands (2016, 16–18) goes further by pointing to essential differences in economic philosophy and methodology[34] between Walras's original theory and its dominant interpretations in the Soviet Union: "on the Soviet side, the goal was to use Walrasian equilibrium to help model a centrally planned economy with a single representative agent . . . . On the Western side, the goal was to use individual optimization to help model the general equilibrium of a perfectly competitive economy . . . . Walrasian theorizing was primarily demand- and utility-focused, while Soviet mathematical economics was supply- and production-focused. . . . Western literature was not computationally oriented; it was more concerned with <how possibly> than <how actually.>"[35] In his view (6–7), the compatibility of Leontief and Neumann with Marx does not mean that Marx is also compatible with Walras.[36]

A detailed comparison with other countries of communism or with the work of verbal economists, be they official political economists or market reformers, is not among the top priorities of these analysts.[37] Rather, they carefully reconstruct the different types of mathematical economists by making distinction not only between input-output analysts and linear programmers or between builders of equilibrium and disequilibrium models but also between experts who favored all-encompassing automated systems of hierarchical state control and who advocated a certain degree of decentralization and/or marketization (e.g., Leeds, 2016a, 346–47). Symptomatically, only the fans of automatization are labeled by them as utopian thinkers. Regardless of the *sui generis* interventionist position of the optimal planners and their strong advisory links to (and partial cooptation by) the *nomenklatura*, they are merely depicted as "techno-scientists" (Rindzevičiūtė 2010, 289–91; Leeds 2016b, 636–39), "partisan technocrats" (Boldyrev and Düppe 2020, 264–73), or members of a "Technocratic International" (Feygin 2017, 260). According to Leeds (2016a, 58), their expert knowledge helped mill the Soviet regime from inside (from "the heart of the state") step by step. There is a consensus among these historians with regard to the amorphous epistemic culture and disciplinary identity of the mathematical economists, their proximity to natural and technical sciences as well as their controversial relationship with cybernetics as a strange umbrella concept and cover discourse (Rindzevičiūtė 2010; Leeds 2016a; Boldyrev and Kirtchik 2017, 2–6, 8–9). They were "stuck between the method and the discipline," writes Boldyrev and Kirtchik (2017, 8–9), suggesting that the application of mathematical techniques does not necessarily make someone a genuine mathematical economist in its Western sense, that is, a neoclassical theorist. However, the question of how this intermediary position between politics and science, and among various scientific disciplines, helped conserve the interventionist/collectivist attitudes of the optimal planners does not seem to provoke the observers' mind.

The flipside to the lack of anti-neoliberal fervor is a weak interest in the Austrian problematic. Apart from identifying some cybernetic fantasists among Soviet economists at the time, these authors do not claim that the optimal planners were cherishing utopian dreams about the rationalization of the planned economy. They barely deal with the fact that even those among the mathematical economists who were not blind to institutionalist approaches got stuck with a—rather neutral—concept of economic mechanism (Leeds 2016a, 173–82; Feygin 2017, 243) instead of leaving the program of regulating/planning the market for that of privatization. The fact that Soviet planning experts kept on propounding state-collectivist views is often overlooked[38] and makes it difficult for the reader to gauge the real depth of both the neoclassical and the liberal commitment of those experts. As a result, one might get the impression that the insistence of optimizers on bettering the central plan stemmed from a fear from retribution rather than from the "stickiness" of their collectivist attitudes.

A promising development has been that some of the authors mentioned above started bridging the gap in literature, which divided the proliferation of optimization attempts during the 1960s and their disappearance with the advent of *perestroika*. In other words, the second act of our drama has begun to be written. For instance, Ericson (2019) coins the term "marcescence" to cover the stagnation and decline of SOFE. The poetic expression (meaning leaves that wither without falling off) denotes the devastating effects of the ideological and political interference by the party-state on the research program but does not refer to the ultimate impossibility of properly designing and implementing rational central plans for the economy as a whole. It suggests that the green leaves were still fresh and healthy in spring. True, Ericson (173–74) talks about the "unrealizable dream" and "unresolvable issues" of optimal planning. Nonetheless, alluding to the informational chaos and incentive incompatibilities of the planned economy as well as to the indeterminate nature of the objective function of any society (unless it is ruled by a dictator), he only calls these "practical problems" that are "highly unlikely" to overcome. Like the founding narrators, most of the analysts in this group consider the difficulty with the objective function crucial. As Leeds (2016b, 355) puts it, "the objective function is nothing other than a name for the economic sovereign." Rindzevičiūtė (2010, 303–4) rather stresses the problems of formalization, the lack of powerful computers, and the slowness of gathering information: "it took two to three years to collect information for a branch optimizing model and about two years for a district model and about five years were needed to collect the information for a more complex model."

Although other members of the group offer thought-provoking studies of the work of leading Soviet mathematical economists like Emmanuil Braverman, Leonid Kantorovich,[39] and Viktor Polterovich, (cf. Boldyrev and

Kirtchik 2014; Kirtchik 2019; Boldyrev and Düppe 2020) as well as elaborate case studies of cybernetic research and its co-evolution with the economics of planning (Rindzevičiūtė 2010; Leeds 2016b), the Kosygin reform (Feygin 2017), or the anthropology of Moscow economists (Leeds 2016a), a number of main actors and scenes of the play are still absent, not to mention the simultaneous plays staged in other communist countries. Also, a comprehensive narrative of the consecutive phases, the external and internal drivers and the alternative ways of decline (marcescence) has not been offered yet.[40] Nevertheless, valuable fragments waiting for a synthesis already have been produced.

Reading the texts of these historical analysts, one sees repeated attempts made by Soviet mathematical economists, which result in repeated fiascoes (theoretical and/or practical), ranging from the dynamization and stochasticization of equilibrium models, through the introduction of game-theoretical schemes of planning, concepts of disequilibrium and non-price control, all the way down to experimenting with man-machine systems. Sometimes, the fiascoes led to a reversal of the history of economic thought: while earlier mathematicians moved to economics, a few decades later the mathematical economists sought refuge in mathematics, building increasingly abstract models. Alternatively, one could abandon the normative use of mathematics,[41] leave behind the domain of planning, and start applying formal models based on one's econometric knowledge acquired in solving optimization problems, in the analysis of the communist economy and the forecasting of its performance.[42] However, as Feygin (2017, 243) remarks, one also could limit one's mathematical ambitions and return to help the traditional planners or, on the contrary, leave mathematical economics for verbal institutionalism mixed with radical Austrian ideas during the agony of communism.[43] In any event, in this labyrinth of research programs aiming to show the Soviet economists the way out of the realm of recurring failures, many optimal planners could think that *perhaps* the next attempt at improving the central plan would be successful.

Insider View?

Earlier I spoke about two ways in which history-writing could respond to the Austrian challenge: resentment and disregard. Those who, in principle, could have combined the virtues of the challenge and both kinds of response (while avoiding their vices) and capitalized on exclusive local knowledge were the historians of economic thought living in the communist countries that experimented with optimal planning. However, such historians were rare, many of them lacked mathematical expertise and/or stayed under surveillance. Andrei Belykh's pioneering book (2007) published in 1989 on the history of mathematical economics in the Soviet Union (which stops the narration in 1965) raised expectations that similar volumes would come to light in other communist countries, too, right after the collapse of the regime. One of the main reasons for publishing our book is that following 1989, such works[44] did not emerge *en masse*. Their lack is barely compensated for by a special genre mentioned in the Introduction: personal reminiscences by leading mathematical economists, both emigré scholars and those who did not leave the region.

A RESEARCH PROGRAM WITH A SOFT CORE

By the end of this volume, the reader has become acquainted with nine country cases that reflect nine evolutionary paths of the same research program: optimal planning and, more broadly, mathematical economics. Do the national chapters offer sufficient evidence to substantiate our comments on the state of the art and, more importantly, to surpass it in key respects? I will condense the answer to this question in the next six points.

Scholarly Identity: A Neoclassical Program of Sorts?

In my view, a large majority of optimal planners were half-hearted and technique-oriented rather than "latent" neoclassical economists. When they did not shout from the rooftops that they were Walrasian thinkers, this was not only (or mainly) due to self-censorship or lack of self-confidence. Most of them candidly believed Marx and Walras to be combinable.[45] Even if we suppose that the optimizers read the relevant neoclassical authors attentively, they were much more interested in the mathematical language these authors spoke than in the *Weltanschauung* and methodology underpinning it. They accepted without second thoughts the Pareto-Barone "equivalence thesis," that is, an interpretation of the Walras model according to which, *in principle*, the "ministry of production" of a collectivist state may not achieve worse results in finding macro-equilibrium than the free market. This also explained why they became resistant to the Austrian criticism of Lange's position in the calculation debate.

General equilibrium theory (GET) was, for the optimal planners, an *operational device* of rational resource allocation by the state (maybe with a little help from the market) instead of a logically consistent, abstract scheme that is called "general" exactly because it was built on stylized hypotheses concerning the market (perfect competition, zero uncertainty, no institutions, and so on) in accordance with the principle of methodological individualism. The suspicion among the mathematical planners about the free-market foundations of neoclassical economics was so widespread that even scholars

such as János Kornai, who by the 1970s managed to get rid of many of his state-collectivist fixations, were captivated by it. He reacted to his own failure to build a coherent theory of optimal planning by scapegoating the "unrealistic" premises and *laissez-faire* ideology of GET. This theory can be beautiful mathematically, he admitted, but it is naïve, self-centered, and unworldly, thereby mistaken and unable to serve as "real science," to cite Kornai's favorite term.[46] Such criticisms were not always grounded in scientific arguments; they also originated in the fear of being strait-jacketed by a new one-size-fits-all worldview just after ridding themselves of Stalinism and searching for a "third way."

The optimal planners were not mesmerized by the neoclassical paradigm, to say the least. Maybe at a certain point, some of them became ready to (secretly) say good-bye to key principles of Marxism, but even they mistook the principle of methodological individualism for individualism in the sense of egoism. It is difficult to explain why even the best-educated minds such as Branko Horvat, Leonid Kantorovich, János Kornai, and Oskar Lange were hesitant to jump over their own shadows even at times when political repression subsided and they achieved the privileged status of the "less vulnerable." Apparently, they were anxious about a situation in which subscribing to Paul Samuelson or Kenneth Arrow might end up in agreement with Milton Friedman and Friedrich Hayek.

The premises of GET were deliberately idealistic, but they became twice as idealistic once coupled with unrealistic hypotheses regarding the planned economy. On an abstract level, the optimal planners described planning as a system, in which all necessary pieces of information are available on time, their flow among the levels of institutional hierarchy is free, there are no vested interests in distorting information, no bargaining games, and so on, and the Central Planner is capable of revealing and concentrating inarticulate and dispersed knowledge. While many of these scholars criticized GET for assuming perfect competition in the market, they suggested, in an ideal case, a perfect lack of competition, friction, disturbance, etc., in central planning.

Further, the optimal planners could not really cope with a dual problem of direct translation. On the one hand, they used a stylized theory of market competition as a manual for operating a workable regime of central planning in the real world. On the other, they wanted to apply the model of a simple programming task (with a small number of static variables as well as with well-defined constraints and objective function) that was solvable in a factory workshop, to an extremely complex assignment of finding equilibrium in the national economy as a whole. To use Lakatosian language, the research program was shaky, incoherent, and fatally incomplete in both its hard core and protective belt. Its core should have been hard in terms of irrefutability while its changeable belt should have protected the irrefutable propositions

contained by the core. This included the underlying hypothesis of "plannability" (*planiruemost*, *Planbarkeit*)[47] that went far beyond the prediction of future conditions of the economy. It pertained to (a) the theoretical and practical preparation as well as the implementation of central plans by the party-state, and (b) the postulate of their improvement via optimization. "Perfecting (developing, coordinating) the plan" and "making the plan more scientific" were phrases invented to describe that postulate.

However, the core lacked a fundamental theory (even if a stylized one) of the micro-and macroeconomic features of the economy presumed to be planned and the economic behavior of the party-state presumed to be able to plan. That theory should have explored, simultaneously with the economy's institutional, behavioral, and informational characteristics, some of its basic driving forces as well, especially those related to changes in technology and consumer preferences in not completely isolated economic systems of great complexity. It seems that either in order to comply with the need to make unavoidable simplifications in their models or to reflect the gray reality of everyday life in economies of shortage, the optimal planners' mind was dominated by the image of a Robinson Crusoe-type planned economy with brutally limited consumer choice, sluggish innovation, autarky, and the like. They knew, for example, that even small changes in human taste would put sand in the wheels of planning but were sure that the hindrances could be overcome with the help of advanced (dynamic, non-linear, and stochastic) models.

The hard core of the research program was not only incomplete but, ironically, rather soft in clarifying crucial issues of optimization such as the definition of the objective function or the "mechanism design" of the economy. For example, the former contained a number of burning questions about who determines (and measures) the needs of society (Fehér, Heller, and Márkus 1983). As regards the protective belt, it also displayed confusion caused by often retaining the doctrine of labor value while also calculating in marginal utility; defining rationality in a sloppy fashion as a technical term; and by mixing normative and descriptive/analytical approaches. The belt was also short of an elaborate concept of supply to replace or complement that of demand in GET. Price determination (e.g., accounting versus real prices) was also a vague issue. Moreover, the optimization procedures were reduced to "naked" mathematical algorithms of input-output analysis and programming, which served, for instance, to decompose the models and ensure their convergence to the optimal solution. These were not only naked but also often empty because, by definition, one could not expect to fill them with correct real-world information. In fact, seeing such a "gappy" research program without solid conceptual pillars, one did not even know what belonged to the core and what to the belt, and whether optimal planning had had a progressive

phase at all before it began to degenerate (see below). Any clairvoyance was also disturbed by the fact that the protective belt continued to be packed with the heavy symbolism of communist planning hailing scientific foresight, the primacy of the state, and collectivist culture in general.

What do I mean by a sloppy, primarily technical definition of rationality? As mentioned in the Introduction, the optimal planners focused on instrumental (goal) rationality rather than value rationality. The latter would have provoked the censors by asking disturbing questions, for example, about trade-offs between armament programs and social welfare. It would be unfair to reprimand the optimizers for conformism in retrospect. Yet, a hint at the non-moral origins of their instrumentalist attitudes seems opportune here. The logical chain linking the Cold War, vast military research projects, cybernetics, operations research, optimization, the computer, the algorithm, and economic planning in both the East and the West was so strong and convincing that moral reservations about the crimes of communism or "only" its forced irrationality could hardly compete with it. Similarly, the admiration felt for mathematics, engineering, systemic rules of behavior, and exact methods placed *rationality above reason* even though the latter can be more humane, flexible, and—according to John Rawls—has a palpable ethical component.[48]

With time, attempts were made at inserting realistic elements (e.g., bargaining) and their related mathematical techniques (e.g., game theory)[49] in the program as well as advancing unorthodox procedures like a democratic selection of the objective function. However, slowly and unnoticeably, the program imploded *in terms of economic theory* before it could fail in the real world. It could not really go wrong in practice because most optimal plans had broken down before they were tested *in vivo*. Unnoticeably, I say, because there circled a more spectacular enemy around the research program than its scientific imperfection. It emerged from the ruling elite, without the initial support of whom optimal planning could not have entered the history of economic thought. But the same elite could cancel assistance if it suspected too much realism or iconoclasm in the optimal models or simply did not find them helpful. As a consequence, the optimizers had plenty of chances for shifting responsibility for the "marcescence" of the program to the Central Planner.

To be sure, this was not a cynical act; many of them sincerely believed in a trade-off between oppression and sound planning, hoping that democracy would cure the maladies of their theory in the future.[50] Regardless of recurrent fiascoes, they kept on building optimal macro-models for years until the political market for these dried up during the second half of the 1980s. A critical introspection could have opened the eyes of the optimal planners to see that the research program was faulty from its very inception and in that sense its failure was coded into the program's core. Scientific central

planning did not work (either on the drawing board or in the form of projects implemented by the planning office), even when it was backed or tolerated by strong groups within the *nomenklatura*.

Were the inbuilt damages of optimal planning reparable? To an extent, they surely were but with paradoxical consequences. When scholars began to improve the research program, for instance, by borrowing critical thoughts on the actual institutional setup (incentives, mechanisms, ownership forms, and so forth) of the planned economy from the market reformers, they found themselves in a vacuum because those thoughts implied that the communist economy was not reformable beyond a certain limit. Surpassing this limit would require privatization and democratization instead of regulated marketization under one-party rule. However, why would an economy of private owners need/acknowledge an overarching optimal plan that eradicates the free choice of economic actors in crucial respects? Hence, if the scientific planners did not intend to quit their research program they were interested in preserving the dominance of some sort of collective ownership. In other words, if they wanted to go on with their optimization experiments, they had better long not for capitalism but market socialism without communist dictatorship—another debatable vision by the way.

Pattern of Evolution I: Explaining Rise

Is it easier to portray the rise of an economic theory than its fall? The state of the art suggests this truism. If indulging the first act of our drama risks stealing the show from the second, one would not have to do more than identify the causes of decay to balance the story. However, the country studies by our research group convinced me that, by examining those causes, optimism about the first act may recede noticeably. It became clear that many of them had loomed large in the concept of optimal planning already in the very beginning. This encouraged me to reconsider not only the program's fall but also its rise.

Thus far, historians have not felt the urge to ask in what sense was optimal planning "better" than its predecessor. Rather than assessing the program's quality rigorously with standard tools of science studies, it was enough to cite two random sentences on planning from any of the official textbooks of political economy to attest to a vast improvement relative to them. Following a carnival of irrationality, even a pale hope for rational reasoning would shine. The intellectual strength of the new research program seemed self-evident also because its rise was extremely troublesome as far as political recognition was concerned, but the optimizers managed to overcome much of the resistance of the censors.[51] Unfortunately, defeating an intellectually weak rival can camouflage one's own deficiencies.

Be as it may, the rehabilitation of mathematics in economic research confirmed some basic methodological requirements of sound economic inquiry; consolidated key institutions of research, education, and advocacy (offering jobs to thousands of mathematical economists in the Soviet world); and promoted the inclusion of researchers in international networks. All these offered the historians motives for a story of a tiresome but triumphant breakthrough, first in the Soviet Union, then in the other communist countries, followed by repeated battles for survival and a final victory. The story would start on the day when Kantorovich first tried to sell the party ideologues the notion of shadow price as "objectively determined valuation" and would end with the award ceremony of his Nobel Prize.[52] Yet, the latter was not given to him for being one of the founding fathers of optimizing *mandatory central* planning in a *communist* economy but for much less and something different. He received the prize for his "contributions to the theory of optimum allocation of resources, [the demonstration of] how economic planning in his country could be improved, [and for showing] how the possibility of decentralizing decisions in a planned economy is dependent on the existence of a rational price system, including a uniform accounting interest rate to form a foundation for investment decision" (Nobel Prize 1975). Sharing the prize with Koopmans also suggested that it was not meant to justify optimal planning as a means of a potentially total macro-control of a non-private and non-market economy, a veritable Grand Design. Instead, it aimed to recognize the fact that the mathematical techniques simultaneously invented in the East and the West gave a chance for economists with *normative* attitudes to experiment with a large variety of "small designs" in the field of the optimum allocation of resources.

Hence, examining the research program from the perspective of "eternity," that is, of the evolution of universal economic thought, one is prompted to ask a few—somewhat ahistorical—questions of a spoilsport nature to test the "rise and fall" sequence. For example, after a while, optimal planners ceased to be contented with designing micro-and mezzo-projects (cf. Kantorovich and the Plywood Trust problem) but, thinking big, stretched their models far beyond the size of those built by Koopmans and most of his colleagues in the West.[53] Should we consider this change a sign of a rising theory? Initially, it seemed so that, with time, most deficiencies of verbal planning could be eliminated and optimization would result in perfect allocational efficiency on the macro level. However, the emerging nationwide models endangered the research program in both theory and practice and eventually contributed to its collapse. In all probability, less would have been more. Many of the serious shortcomings of optimal planning could have been avoided if its protagonists stuck to attempts at solving operations research-type problems in selected firms, industries, or regions rather than continuing the Kautskyan tradition

of imagining the national economy as a large firm to be optimized.[54] Does it make sense calling a research program progressive, which—driven sometimes by megalomaniacal goals—maneuvered itself early on into various dead-end streets such as the utopia of automated macro-control? Moreover, most of its representatives did not try to escape or reach out, at least for a Tinbergenian solution, a less determined Grand Design, by switching to indicative (non-mandatory) and decentralized planning by the government. This would have bordered on prognostics and promised modest but more reliable optimization models by also paying attention to genuine (non-simulated) market processes in the private sector. In sum, given the global postwar supply of ideas on mathematical planning, it would have been possible to emulate alternative avenues of progress.[55]

Choosing ambitious, Soviet-style optimal planning implied high scholarly "opportunity costs" in another respect, too. Obviously, one could skim the edges of central planning and the related official political economy without much mathematical finesse, with the help of the verbal research programs of market reform. These programs, too, had a number of methodological flaws[56] but promised a quicker access to a future positive theory of the planned economy, a theory absent from the core of optimal planning. While most reform economists were also collaborating with the party-state, their empirical curiosity was much stronger and normative leanings slightly weaker than those of the optimizers. They disliked the rigid hierarchy of the economy ruled by a party-state and started toying with the idea of (limited) economic liberalization much earlier. It is also true, however, that the reformers used a less accurate, and even messy scientific discourse. What if the optimal planners had not embarked upon their road to nowhere but helped the verbal reformers formalize their analytical thoughts about the communist economy? What is still regarded as the rise of optimal planning was in certain respects a persistent waste of time that could have been spent on merging the two research programs.

As a result of such a synthesis, the national research communities probably could have approached a then brand-new research program in the West earlier. Like mathematical economics in the communist countries, New Institutional Economics, and particularly Public Choice, began to bloom from the turn of the 1950s and 1960s. Knowing the institutionalist tradition of Marxist economists, the mathematical talent of some, the reformist prehistory of several mathematical economists in the Eastern Bloc, and their local knowledge of massive government failures, they could have even overtaken some of their Western colleagues in developing the neo-institutionalist program.[57] If this volume revolved around market concepts (as our next volume will), I would hasten to ask whether the market reformers, stuck in their own cul-de-sacs, were not wasting time as well. Here, it suffices to say that, owing

to the rivalry of the two groups, their research programs ran in parallel for more than thirty years without barely profiting from synergy.[58]

In principle, nothing prevented the optimal planners from asking what kind of utility the various actors of the planned economy try to maximize. They could have modeled why and how these actors bargain about the planning figures and distort data.[59] However, that would have required a critical rethinking of the "rules of the game" of central planning and a careful mapping of the actors' behavior (with a special emphasis on the *nomenklatura*). This map might have included principal-agent problems, asymmetric information, adverse selection, moral hazard issues, informality, bargaining, rent-seeking, shirking, subgoal pursuit, logrolling, pork barreling, and the like.[60] The mathematical economists were aware of many of these intricacies of the planned economy, but they lacked the scientific language to convert empirical knowledge into theory. If they had not been well-read in the rapidly growing literature in fields such as property rights, market and government failures, law and economics, and transaction costs (which in some countries would not have been their fault at all), they still could have used concepts like "ratchet effect," "hoarding," "Micawber principle," or "taut planning." After all, many of these concepts emerged in economic Sovietology and Comparative Economic Systems with their own or their reformer colleagues' assistance. However, rather than focusing on the institutional texture of the planned economy, they cast doubts on the heuristic value of the notion of *Homo Oeconomicus* by contending that in such an economy the main actors would follow irrational goals if the optimal plans did not discipline their behavior.

Today, the spread of optimal planning would appear as a less successful period in the history of communist economic ideas if we took into account the unexploited opportunities for progress. Should we blame isolation for the missed chances? I would not think so because some preconditions of exchange of ideas between East and West and East and East (see below), not to mention interaction between the various groups of the national research communities, were given from the very beginning, at least in certain countries. Also, the prospects for physical and intellectual encounters between scholars widened as the years passed by. Mutual misunderstandings aside, the optimal planners in the East and operations researchers (activity analysts) in the West spoke dialects of the same technical language. Despite the applause coming from the West and the enthusiasm of the pioneer-optimizers, the transnational multilogue also could have made them more cautious. Still, they showed a clear propensity for overstretching their research program.

Well, we returned to our basic puzzle mentioned in the Introduction: why did economic theorists in the communist countries so often become captives of what we call the "trap of collectivism?" To answer this question, one has to get rid of the widespread practice of deriving the imperfections of their concepts primarily from political repression. For brevity, let me name this the "thought police fallacy." Blaming censorship (or self-censorship) was a favorite element of the tale of woe told by mathematical economists. A brief description of the reasons for the fall of optimal planning next should explain why this may be a necessary but fairly weak account.

Pattern of Evolution II: Explaining Fall

Above, I paraphrased an old Soviet joke about Marxist philosophers who worked hard to answer a burning question of real socialism: is there life *before* death? Had optimal planning risen before it began to fall? Now, let us check the opposite: was there a fall after the rise? This is also a tricky question because in our case there was no *caesura* separating the two. The end was preprogrammed in the beginning, and the fall overlapped the rise; therefore, it is close to impossible to make a clear distinction between them. This is not to say that, taking the whole lifespan of optimal planning, there was no difference in the quality and growth of publications, stability of academic institutions, or in the enthusiasm of researchers between the start and the end. However, the gist of the research program is another matter.

So far, I have used the term "stagnation and decline" instead of fall to indicate the lack of a turning point (or points) or a peak (or a plateau) dividing rise and fall, and invoked the structure of Greek tragedies to reveal the absence of catharsis in the plot. Now, let me collect the main causes of the gradual decay, capitalizing on evidence provided by the national chapters.

Beyond Realism and Elegance

The Mises-Hayek-type reasons for the dysfunctions of rational planning came to the fore early on when researchers were confronted with the task of gathering information they wanted to feed into their models. Most of them did not know that the following questions had been asked many decades before:[61] should we measure products in physical units or in labor time in order to aggregate them? If prices are used for measurement, how reliable are they in a planned economy? Is the necessary information about quantities and prices available at all at the start of the planning process? What if they change thereafter? How can scattered information be synthetized? Are the economic actors interested in providing the optimal plan with truthful data and complying with the planning instructions in the phase of implementation? Do they know these data at all? All answers to such questions were ambiguous and insecure; in addition, they had to be translated into robust mathematical operations. Meanwhile, the models grew too large (even compared to the rapidly expanding capacity of computers) and clumsy, especially if the experts

wanted to loosen some of the simplifications such as homogeneity, closedness, linearity, staticness, and determinism, which led them far away from the real world. Yet, a more realistic model did not prove necessarily more elegant in mathematical terms and more workable in the planning practice.

However, witnessing the mushrooming of experimental models and the attraction the research program made to gifted mathematical economists all over the world, the optimizers reassured themselves that these difficulties would be overcome through scholarly invention and reasonable theoretical compromises. The models would become increasingly complex and realistic, the computers astronomically faster, and—consequently—the criticisms pointing to unsurmountable institutional/informational problems with the optimal plan outdated. In this optimistic mood the deepest wounds cut by the Austrian critique were frivolously ignored (concerning, e.g., calculation in labor time, reliance on artificial prices, or centralization of dispersed and tacit knowledge) and never healed by mathematical sophistication.

Life in the Jungle

Initially, it seemed that the ideological resistance to optimization was broken for good when the Central Planner agreed to the rehabilitation of mathematical methods. It took a long time until it became clear that political support was utterly conditional. The optimal planners were not allowed to decide on key components of their models such as the objective function and the constraints; they were deprived of essential information about certain sectors of the economy; and the rulers also reserved the right to not reveal their preferences precisely and alter or dump the complete optimal plan at their will. Obviously, the luminaries of the Austrian School could not predict these specifics of planning under one-party rule, like they could not know either how fiercely the verbal planners would resist the inflow of mathematics in the daily practice of the national planning offices ("drawing up I-O chessboard tables may be fine but please do not mess around the planning goals and instructions," they said). Mises and Hayek foresaw the detrimental effects of collective ownership (especially in the form of centralized state property) on incentives to calculate rationally, innovate, and behave as entrepreneurs instead of bureaucrats. Nonetheless, it was impossible for them even to guess the absurdity of the "ordinary business of life" in the jungle of institutions of a planned economy (cf. Lewin 1973; Harrison 2005).

If they had had the slightest idea of the complicated web of vested interests and bargaining strategies in the planning process then they probably would not have spent much time discussing such elevated theoretical issues as the controversial nature of labor value or the emergence of economic knowledge in the market. Austrian critics of collectivism simply would have drawn the

conclusion that economic rationality would be suffocated by misinformation, secrecy, ignorance, informality, political intervention, non-economic preferences, and the like.

Virtually all data providers in the planned economy were cheating without any scruples, and the only hope for rational decision-making was, to quote the writer Péter Esterházy (2004, 5), that "it is deucedly difficult to tell a lie when you don't know the truth." However, it took the majority of scientific planners decades to recognize that these were deep structural defects and could not be fixed either by mathematical tools or administrative/managerial practices such as moral persuasion, disciplinary action, and stricter monitoring. Until then, the optimizers could presume that their mission was not entirely impossible and *perhaps* the next round of experiments would succeed. They also needed time to reckon with the sad fact that it did not help much when—rather reluctantly—they borrowed ideas from the reform economists and injected a modicum of decentralization or marketization in their planning projects to raise efficiency.

Inertia and Conviction

Paradoxically, such disappointments would likely have deepened if a genuine comprehensive central plan (not just its truncated or simulated version) had ever been prepared by the optimal planners and it had enjoyed lasting support from powerful lobbies within the ruling elite. Then they would not have been able to close their eyes to its ultimate bankruptcy. The optimizers could not be sure whether or not their plans would be dispensed with any moment and they would be thrown before the lions, that is, exposed to attacks by vigilant political economists, or angry bureaucrats from the Planning Office and the party center. The researchers were dragged back and forth by the political class, and the academic institutions were incited against each other and pulled into hopeless intra- and interdepartmental fights of the ruling elite. Ironically, the optimal planners slowly lost confidence in support coming from the party-state while still firmly believing in the central role played by the same party-state in their planning models. Blaming the *apparatchiki* for the failing plans delayed facing the theoretical shortcomings of their own research program. Eventually, they put up with polishing their models, fortune-telling, assessing risks, and issuing early warnings. Some of them moved to the field of long-term planning where one could breathe more freely; many others, however, continued to take part, though more reluctantly, in what was called in Hungary "plan coordination," revealing, in the form of simple quantitative terms and causal relationships, the constraints of the unchained fantasy of the supreme decision-makers.

At a certain point, the waves of frustration and fatigue of scholars reached some groups of the *nomenklatura*, who began to switch their patronage from the optimization of central planning to marketization and even privatization of the planned economy. Not quite independently from this, the existential anxiety of mathematical economists subsided in most countries. There remained only two—strongly related—reasons for them to continue building optimal models for the Central Planner even in "softliner" communist regimes: scholarly inertia and collectivist conviction.[62] The former explained the insistence of optimal planners on staying within the discipline of mathematical economics, often submerging in econometrics, growth theory, or research on production functions, economic regulation, business cycles, disequilibrium, and so on, that is, in fields related to optimal planning, but also in forecasting and even futurology.[63] The latter was evidenced by the fact that normally these experts did not join the camp of market reformers, in particular, not their radical wing. They had second thoughts about communist and (later) post-communist liberalization and made fun of turncoat political economists who covered the distance between "Marxism and monetarism" in a few seconds.[64] Similarly, very few of them became champions of New Institutional Economics, even after 1989.[65]

To return to the image of marcescence, from the 1970s, the leaves started drying but did not fall off the tree of the research program. What explained the belief that optimal plans failing in the past *perhaps* would become successful in the near future? I have alluded to a number of reasons thus far, including myopia, self-deception, opportunism, and so on, which are not directly related to fear from the thought police. Let me elaborate on them from the perspective of the "inertia/conviction" connection. Much of the communist messianism of mathematical economists turned into social-democratic pragmatism as years elapsed and their theories opened up to adopt market socialist (initially, *khozraschet* socialist) elements. However, they did not receive powerful messages from their key reference groups for decades, which would have persuaded them to take a step further and start thinking about an exit from the research program. The recurrent attacks by political economists, on whom they looked down (calling them, for example, parrots[66]), only reinforced their beliefs. As for the market reformers, their verbal discourse and liberal pretensions did not enchant the optimizers. Moreover, the reformers could not issue warnings about the dangers of state interventionism because their projects were also contingent on cooperation with the party-state and seemed to be equally unsuccessful as those of the scientific planners in practical terms. Finally, the Western peers of the optimizers did not cease to encourage them with prestigious prizes, joint publications, conference invitations, etc., suggesting that they were producing cutting-edge knowledge. However, this support weakened after Kantorovich's Nobel Prize. General equilibrium theory began to fade in the West, concepts of disequilibrium and rational expectations appearing as strong competitors.[67] The neoconservative turn in the second half of the 1970s (Milton Friedman was awarded the Nobel Prize just a year after Kantorovich and Koopmans) started eroding two other pillars of optimal planning, its inherent statism and hope for convergence between East and West.

An overwhelming majority of the optimizers' research community developed a professional identity that relied not only on international solidarity between input-output analysts and linear programmers but also on the feelings of superiority of mathematical economists vis á vis their colleagues doing verbal research.[68] This worked as a regular demarcation criterion for the disciplinary status of optimal planners. The initial investment in the "cultural capital" of their research program was large enough to not let it go easily. Besides accumulating exclusive scientific knowledge and developing institutional and political routines in order to increase that capital, the optimal planners combined these with ideological and even emotional ingredients. For years, many of them were convinced that by finding rationality in a post-Stalinist economy they fulfilled the old dream of the left, and the marriage of optimization and humanization in the framework of a scientific program with global outreach was just around the corner. If you seriously think that you hold the stone of the wise in your hand and are imbued with a historical mission, it will be very hard for you to admit that this stone is almost worthless, at least as far as your mission of perfecting the central plan is concerned. Even if you were ready to realize this after much hesitation and self-torture, you have already fallen in love with your own ideas in the meantime—a tempting opportunity to overstretch your program, in particular if you found a comfortable place in the trap of collectivism. The market reformers were often ridiculed for "reform mongering," a sort of lucrative business pursued at the border of science and politics. Well, "plan mongering" became a similar job for optimal planners once they managed to stabilize their institutions of research and education. Nevertheless, their relationship with the Central Planner was far from being balanced: what the optimizers profited from their advisory position was a considerable (but not irrevocable) protection that manifested itself in some freedom of thought, travel, publication and the like, higher incomes, and a chance for cooptation in the *nomenklatura*. The protector's only risk was that the protected could take a look at his cards.[69]

Thus, beating a dead horse, you could build up a life work (cemented by formal academic status), and hardly anything was more depressing for you as a scholar than to admit that maybe you would not bequeath but a few model specifications or simulation algorithms to posterity. Meanwhile, the main lesson of your professional life could have been a brief warning like this:

"Think twice before you engage in central planning again! Optimization will not help."

Plans without Tests

This was a schematic view of acknowledging/denying the decline of the research program by its adherents. The causes were listed in a chronological rather than a ranking order. In some respects, the story may remind the reader of the evolution of ownership concepts described in our previous volume (Kovács ed. 2018, 325–29). From among the similarities let me choose only one. Why did the "perhaps effect" work so long? How could the optimizers continue to craft plans between two fiascoes again and again? Beyond the numerous reasons depicted above, one must not ignore a principal problem of scientific logic, testability. Why would an unrelenting experimentation end if the boundaries between success and failure are vague? Because political interference was daily business, one always could think that planning failures were brought about by it rather than by deep-seated theoretical flaws of the optimal plans. How do we know that, at a certain point in time, an optimal plan is better or worse than the other if both contain not only different mathematical structures but also different data sets and different inbuilt political compromises? Furthermore, neither of the two will be implemented and we will not be able to gauge the difference between their predictive powers.

What remains is barely more than a comparison of the two planning projects according to their mathematical abilities and beauties. By crossing the country lines, decisions on quality become even more insecure because a planning project regarded by a national research community as a conspicuous failure could be relaunched in another country without any difficulty after some years. Errors do not exclude further trials and one can always blame, not without foundation, the hard constraints of making experiments: the poor technical conditions (lack of computing capacity and skilled planning officials, red tape, permanent time pressure due to chaotic organization, and so on), the company directors and the planning bureaucrats of various state agencies who fake data, or the top policymakers who change priorities overnight and ignore the final version of the "scientific" plan, preferring the traditional methods of verbal planning.

East-West and East-East Exchanges of Ideas

As the review of the relevant literature showed, three intertwined narratives dominate when it comes to the transnational diffusion of ideas of optimal planning: (a) the research program had strong Russian/Soviet roots; (b) in contrast to the usual West-East direction, important ideas (original discoveries) of the program traveled also from the USSR to the West[70]; (c) the new knowledge exerted a decisive influence on mathematical economists in other communist countries. These narratives originate in an extraordinary interest of the authors in the Soviet history of economic thought—a plausible bias. Undoubtedly, the re-legitimization of mathematical economics in the USSR created a pattern for researchers in the Eastern Bloc to follow. The institutional stabilization of the Soviet School of Mathematical Economics also offered the optimal planners in other communist countries an excellent opportunity to justify their struggle for recognition. Nonetheless, these served as a base of reference ("if new ideas are not blacklisted in Moscow then why should they be in Prague or Sofia?") rather than triggering off an actual emulation of theories invented in the center of the empire.

The Soviet experts tried to find allies in the satellite states but the local specialists were not emissaries sent by their superiors in Moscow. To read Kantorovich or Novozhilov was not a must and not the only option either. Polish optimal planners learned the basics of the research program first from Oskar Lange (Kantorovich studied him as well), while Hungarians followed scholars like Kenneth Arrow, Robert Dorfman, Paul Samuelson, and Robert Solow.[71] For instance, in his *Anti-equilibrium* Kornai (1971, 351–55) reprimanded Katsenelinboigen for assuming the existence of a welfare function for the whole society and Kantorovich for controlling the economy via shadow prices.

Self-education prevailed in all countries for many years, and reading was promoted by the translation of cutting-edge works of a great number of prominent mathematical economists. As the Bulgarian case shows, understanding Russian was helpful not only in borrowing ideas from Soviet scholars but also in reading Western authors whose works were translated into Russian language. To give other examples of mutual and indirect impacts, Vasilii Nemchinov learned linear programming from the English-language book of a young Hungarian mathematician Béla Krekó (Leeds 2016a, 259). The writings of the East-German Georg Klaus affected many Soviet cyberneticians and optimal planners (Rindzevičiūtė 2010, 302).[72] The optimizers took over input-output analysis from Leontief who was at least as American as Russian. To show the fragility of ethnic classification in an East-West context, one may consider the case of John Neumann whose growth model made an enormous influence on the optimal planners: can he be reasonably considered a Hungarian, therefore, Eastern scholar?

U.S. activity analysts and cyberneticians exchanged key ideas with their Soviet colleagues during the Cold War, contributing to the evolution of scientific planning as well.[73] Was Koopmans the first or was Kantorovich, or their discoveries were truly parallel?[74] What about priority issues in the cases of Lange and Malinvaud versus Kornai, Dantzig and Wolfe versus Kornai,

or Volkonskii versus Kornai in various planning models?[75] Was the "West" affecting the "East" or vice versa? *À propos* Kornai, his self-criticism as an optimal planner was ground-breaking, affecting other Eastern European researchers such as Tibor Schatteles (a Hungarian in Romania), Aron Katsenelinboigen, and Viktor Polterovich. To be sure, even if these experts often did not speak each other's mother tongue, they met at various conferences,[76] visited each other on both sides of the Iron Curtain, read each other's works in translation, and published in each other's countries. Optimal planners from Eastern Europe studied at Soviet universities. Certainly, many of the new ideas were not homegrown but were not dictated by Moscow either. (True, a Soviet precedent was useful.[77]) Yet, not only Kornai but also even more cautious experts such as the Bulgarian Evgeni Mateev took the courage to diverge, for instance, from Kantorovich's theory openly.

The Soviet bias in the literature on the history of mathematical economics in the communist period was understandable but led to an optical illusion. It obscured the fact that long before the Soviet School of Mathematical Economics could begin to establish itself as a stronghold of optimal planning at the turn of the 1950s and 1960s, historic changes had taken place in economic research in the West (above all in the United States). The defeat of (old) institutionalism in the second *Methodenstreit* after the war and the victory of neoclassical synthesis, the surge in operations research/activity analysis, the triumphal march of general equilibrium theory and econometrics as well as the mathematization of economics in general were at least as decisive developments contributing to optimal planning in terms of High Theory as the simultaneous rise of computer science, systems theory, or economic cybernetics (Weintraub 2002; Backhouse and Salanti, eds. 2001). These were the times, say, between the seminal book *Linear Programming and Economic Analysis* published by Dorfman, Samuelson, and Solow in 1958 and Samuelson's (1970) self-ironical *bon mot* from 1970—"Before I won a Nobel, I felt my omniscience. Now I know it."—which reinforced the self-confidence of mathematical economists not only in the West but also in the East.

National Types?

Did this network of transnational impacts emanate from well-distinguishable national types (schools) of the research program? Was there, for example, a Polish (Lange), Hungarian (Kornai), or a Soviet (Kantorovich) school of optimal planning, which showed characteristic traits different enough to construct a fair typology? In writing the Conclusion of a volume like this, one is tempted to apply conventional distinctions between the country types of communist economies such as conservative and reformist, hardliner and

softliner, state-collectivist and self-managed. In order to diverge a little from these—often fuzzy—adjectives, our previous volume introduced another division running between "conformists" and "explorers," that is, between countries in which economists complied with the concept of social ownership and countries in which many of them searched for innovative solutions in property relations, drifting gradually toward the idea of large-scale privatization. In fact, there were countries in which no major innovation in scientific planning took place, while in others (above all in Hungary, Poland, and the Soviet Union) the specialists excelled with several original discoveries. Nonetheless, in contrast to the colorful world of ownership doctrines in which one country opted for centralized state property, another for managerial ownership, yet another for workers' self-management, following an irregular schedule, optimal planning was much more homogeneous in both space and time. With the obvious exception of Yugoslavia,[78] all countries from the GDR to China traveled along similar paths through the overlapping rise and fall of the research program.[79] These paths reflected a certain degree of ideological radicalization in the long run. Yet, a large majority of optimal planners were only able to scratch the armor of dominant state control since they remained loyal to the idea of some kind of an *imperative* central plan. Meanwhile, the ownership reformers (who belonged to the group of the most liberal-minded economists among the market reformers) challenged *nomenklatura* ownership by punching holes in that armor.

The optimizers varied in terms of timing their planning projects. Some of the countries (the Soviet Union, for sure) were early birds; some others, like China and Romania, were latecomers. In one country frustration with the program appeared at a relatively early stage (Hungary); in another the experts are still fabricating optimal planning models (China).[80] However, if we descend from the national level to that of the individual scholars we encounter a number of similar types in the different countries. These types vary not so much in the mathematical techniques they employ but in the ways in which they interpret plannability, a principal constituent of the hard core of the research program.

As for the techniques, optimal planners worked out numerous new algorithms as years passed by. Originally, the protective belt of the program included input-output analysis and linear programming. These were complemented with and refined by a large variety of mathematical instruments like game theory, non-linear, dynamic, and stochastic analysis, general equilibrium models, and so on. In this sense, the program was considered progressive with good reason.[81] In hindsight, one could create typologies comparing, for instance, those scholars in each country who experimented with non-linear programming with those who preferred to develop the theory through applying stochastic methods in order to protect the hard core.[82]

However, I am afraid that such a classification scheme would not help tackle our basic problem of whether or not optimal planning was doomed to decline because its "degeneration," to use again a term coined by Imre Lakatos, was much less related to the components of the protective belt than to those of the hard core. Owing to the refinement of mathematical methods, the belt did become more protective but not to such an extent that it could resist attacks against the core, which gained strength from the increasingly obvious lack of "plannability."[83]

In an attempt to identify real types, I suggest to examine the main varieties of reaction to the paradox annoying Oskar Lange already in the 1930s when he pondered the dangers of bureaucratization.[84] To rephrase his Leninist discourse, he wanted to know how the party-state could be strengthened and weakened at the same time by means of economic theory. Is there a way, in which the Central Planner concedes to not abusing the power it earns, profiting from the expert advice given by mathematical economists? How to ensure that the Central Planner observes the rules of the game (above all, complying with the requirement of free consumer and labor markets), does not derail the process of scientific planning, stretching from data collection to the endorsement of the plan, and accepts the optimal model's normative conclusions in the course of its implementation? In other words, how can the optimal planner convince the ruling elite about the advantages of having much less to do and, as a consequence, much less power to intervene? Will the Central Planner want to commit suicide?

To answer these questions, the optimal planners first had to get rid of Leninist illusions, according to which it was the working class and the party that would tame the Central Planner (if this would be necessary at all) and look for institutional obstacles to excessive state intervention. Like the verbal reformers, the mathematical planners started moving toward the market, sometimes echoing reformist suggestions for liberalization, but stopped at different points on their way. The ideal of a centralized regime of imperative planning did not vanish entirely from their scientific agenda. Some of them were even ready to make a U-turn and go back to "classical" Soviet planning in terms of centralization and mandatory targets, choosing its updated—automated—version. Otherwise, optimization projects with or without inbuilt elements of controlled marketization were mushrooming in many countries. These projects included key components of what analysts like to call the Soviet, Hungarian, or Polish schools but stretched beyond these in many respects.[85] They can be squeezed into four pigeonholes (ideal types):

Optimization within the Old Planning Regime

This is a prolongation of the traditional scheme of central planning, practically without any misgivings about its hierarchical nature. The first optimal plans of the post-Stalin era continued to consider the Central Planner both omniscient and omnipotent, an institution that—similar to other actors at lower levels of the planning hierarchy—has no vested interests whatsoever. It is supposed to be capable of collecting and processing correct information and sharing the job of preparing the optimal plans with mathematical economists (and computers). Its only imperfection is the exposure to the expert knowledge of scientific planners, but these must accept whatever the Central Planner wants to include in their models and quit the planning process in the phase of implementation. Selected results of the models become imperative planning tasks to be disaggregated by the center and fulfilled by intermediary organizations all the way down to enterprises.

Instead of suggesting to transform the command economy into an "advice economy," to play with words, this project retains military mobilization as the main organizational principle of planning and confines the efforts expected from the optimizers to raising the quality of commands. Hence, enterprises are not considered active "plan makers" but data providers and passive "plan takers." The entire procedure is allegedly transparent, the tasks are technical, that is, not "contaminated" by market-type decisions, and all actors serve a common cause without informational-institutional frictions. Kantorovich's original attempts at linear programming and the first models built by TSEMI researchers were among the real types of this endeavor.

Optimization in a Plan-and-Market Regime

This project admits that the Central Planner has limited powers in both acquiring correct information and implementing planning decisions. Nevertheless, it is still deemed to be unselfish and worth being assisted by "the science" in controlling some self-interested lower-level state institutions including enterprises. These need to be incentivized to reveal information and comply with the center's will. The optimal plan is presumed to deliver the proper incentives to channel the energy of informal bargaining into plan making. Here the principle of tit for tat is regretfully acknowledged. Once the optimal plan is completed, these institutions turn into passive plan takers. Similar to the previous project, the Central Planner is entitled to govern the entire planning process and dominate the optimal planners by disrespecting the rules of optimization any time. Still, it has to acknowledge the virtues of some decentralization and indirect control as well as to create a few quasi-market institutions like *khozraschet* in order to oil the planning machinery. Planning thus

becomes an interactive and iterative venture with multiple rounds of negotiation between the center and its inferiors, in which the last word belongs to the former and nothing is enshrined in contract. At the end of the final round, the Central Planner is assumed to become omniscient and omnipotent again. In the phase of implementation no bargaining is permitted.

This ideal type derives from a great number of real types and their blends[86] that differ in the degree of doubt about the "innocence" of the main actors. Initially, for instance, in the Lange models, not only the higher echelons of the economic hierarchy had been presumed to lack vested interests but also the lower ones. As mentioned, red tape was considered a risk but the need for negotiation was explained rather by the fact that the task of macro-planning was too complex and the enterprises were better informed about their own situation than the planners. Later, the suspicion toward all participants of central planning grew and the optimal planners had to face the hard task of designing models that reduce the flow of distorted data from below, arbitrary interference from above, and both from between the two levels. The real types embodied many dozen attempts at executing that task. They range from one- to two- to multi-level planning models with or without games. They also differ in the structure and size of information required from the actors and of instructions or normatives resulting from the model calculations as well as in the space left by parametric planning for the actors to maneuver. In these models the iterations of the draft plans between the various levels of hierarchy may start from below and from above; they may apply input-output schemes of diverse depth and width and use or produce different kinds of prices (including shadow prices), or no prices at all; the calculations may or may not result in profits and rents as planning normatives—one might list the differences *ad nauseam* without leaving the core of the research program.[87]

In the last analysis, it was the Central Planner who remained the plan maker and decided on how much of its power might be sacrificed and how to compensate for the loss. These projects did touch on some main taboos of the planning concept canonized by official political economy, but they continued to bestow so much power on the party-state that its intrusive character could not compare to that of a detached Walrasian auctioneer. The latter was an idol for many optimal planners—a mediator who processes data but does not coerce and punish the real actors. As for the planning normatives, they were not immune to being transformed by the authorities into mandatory instructions at will. Even in the best case they were artificial (accounting-style) indicators generated by the planning model instead of produced by flesh and blood agents of the market. To return to the military analogy, the "captains of industry" were obliged to inform their superiors about the combat force of their units and allowed to complain about the quality of food or the quantity of ammunition but were strictly prohibited to resist the commands of the general.

Democratizing the Planning Regime

Relative to the previous two ideal types, this one aims at depriving the center of the exclusive right of defining the *telos* of the planned economy when formulating the constraints and objective function of the programming model. If the citizens were allowed to vote, for example, on the desirable patterns of consumption, the rate of economic growth, or the share of military investments, then the party-state could have much less chance to abuse power.[88] Heretic thoughts like "consultative" rather than "directive" (Birman 1968) or "compositional" rather than "decompositional" (Petrakov in Sutela 1984, 187–88) planning were put forward only by a small minority of mathematical economists even in Yugoslavia where they could have made use of the self-management rhetoric of the ruling elite in certain periods of communist history.[89] Be as it may, a discussion whether a democratically defined social utility function exists at all (cf. the Arrow paradox) did not even begin among researchers.

Some of the optimal planners saw clearly that democratic participation in planning needs legal guarantees to defend the weaker party in the negotiations, be it an enterprise or the whole society. In order to prevent the Central Planner from ignoring or amending a popular vote or any of its promises made to enterprises, they advocated for the introduction of contractual relations (e.g., *khozdogovor*) among the various actors or, for example, of formalized procedures for bidding for resources. In this way, the contractual partners might establish transparent market relations. These initiatives, as so many others, remained on paper, possibly saving their authors from new frustrations.

Automatic Planning

This type of planning project steps out of the plan-and market paradigm to return, with a cybernetic twist, to the realm of the end-of-nineteenth-century collectivist visions of a world governed by benevolent manmade machines. The idea of total automation of central planning, an extremist version of what was called "computopia" in the 1960s, replaces the Central Planner with a centrally managed network of computers that have no interests, preferences, or biases whatsoever. Still, their omniscience and omnipotence are beyond question. Thus, any constraint on state planning would be superfluous and even harmful. Unlike most of the previous projects, some elements of this were tested in the Soviet Union during the 1970s and 1980s. Originally, the size of the project was thought to be comparable with the Soviet nuclear

and space programs. In other countries (e.g., in the GDR and Bulgaria) the automatization program was aborted at an even earlier stage.[90]

Although at first sight, automatization seems to be a plainly hyper-centralist apotheosis of state-based planning, some of its followers wanted to exclude not only the market (and even money) from improving the plan but—boldly—also the Central Planner. The mathematical algorithm was supposed to be the plan maker while all institutions in the vanishing economic hierarchy were thought to become simple plan takers. It was hoped optimistically that the so-called "automated management and planning systems" (ASU, ASPR, OGAS) were decentralized and impersonal enough to resist the interventions of the party-state. Unsurprisingly, however, it turned out that these systems were designed to be "*centrally* decentralized," to use an oxymoron, and not neutral at all. They were exposed to those politicians who decided on power distribution encrypted in the software to be installed in the computers and on the data. They were also presumed to determine the constraints and objective functions of the optimal planning models. In these models the problem of rational calculation was overshadowed, in a cybernetic daze, by that of optimal control. The "Austrian suspicion" about institutional/informational frictions was ignored, which explained much of the failure of the entire project.[91]

At the same time, automatization of planning had its own enemies within the ruling elite. Suggesting in a dictatorship that the dictator should obey the instructions of an automatic machine was a hopeless initiative. How can the "leading role of the party" be defined in an optimal model, asked the official ideologues. What if the optimal solution determined by the machine does not match the "interests of the working class"? What if it harms industries, firms or regions that the party wants to favor? An optimal plan is by definition rigid: if it promises the best solution how could we bend it to attain our own goals went the argument. Hence, disappointment with automatic optimization was preprogrammed in the genes of large lobbies within the *nomenklatura*, first of all in those of potential losers. Therefore, if they disliked optimization then they disliked its automatic variants even more. The optimal planners recognized rather late that it was not by chance that—as ironic as it may have been—the official political economy of communism had not developed its own theory of planning in the course of so many decades. Today, it is already a commonplace conclusion that it did not need such a theory because a rigorous (mathematical) doctrine would have grossly limited the liberty of the ruling elite in taking macroeconomic decisions.

To avoid misunderstanding, neither the ideal nor the real types outlined above were arranged in a chronological order. Many of them appeared in the research program simultaneously, especially if all countries in our sample are considered. This is another reason for the claim made earlier that the rise and

the fall of optimal planning overlapped and the final decline was preceded by a longer period of stagnation.

Status and Role within the Research Community: "Optecons," "Polecons," and "Refecons"

Sociology and politics, and more broadly put, the non-economic external drivers of change in economic sciences of the communist era, will be the subject of our fourth volume in the series. There we will discuss standard themes ranging from the institutions of research and education, through the socio-cultural features of the epistemic communities, to the political control over scholars. Here I will only gather from the chapters of the present volume a few elements missing or hiding in the literature, which pertain to conflict and cooperation between the optimal planners and either the political economists or the market reformers. To simplify my account, I will call them "optecons," "polecons," and "refecons."[92]

In the previous sections it has become clear that the community of optecons was layered in many ways. It included empiricist I-O analysts just like linear programmers with normative aspirations; those among them who focused on mathematical techniques and those who also advocated institutional changes like the refecons or opposed such changes like the intransigent polecons; and those who cherished close contacts with the ruling elite and those who were forced to emigrate. Obviously, intermediary types abounded. In any event, the best way to demarcate optimal planning from the other two economic subdisciplines was the language its representatives spoke, although there were also a few refecons who were well-versed in mathematics. Above, I used the word "rivalry" repeatedly to describe not only conflict but also cooperation between the three groups. As mentioned, the state of the art is rather uncertain about their interactions. For example, Ellman portrayed the optecons as refecons, even if inconsistent ones, whereas Sutela and Feygin regarded also part of the polecons (the *tovarniki* in the Soviet Union) as refecons, while Bockman believed that both the optecons and the refecons were proto-neoliberal thinkers. I am afraid that by remaining on this level of generalization, one cannot understand why the research program of optimal planning "degenerated," was often left alone by its potential ally, the theory of market reform, got locked in its own inertia, was trapped by collectivist traditions, and found an emergency exit to mainstream mathematical economics only during the last hours of communism.

Undoubtedly, optimal planning was a prime terrain for middle-of-the-road solutions. It offered an excellent chance for scholars to (a) distance themselves from the theory of central planning as glorified by official political economy *without* demanding sweeping market reforms; (b) work together with the

reformers *without* becoming liberal thinkers; (c) borrow certain instruments of neoclassical economics *without* accepting its original philosophy; and (d) break with the parochialism and Byzantine atmosphere of the communist academia and join the international community of modern (data-based, formalized, computerized, and so forth) scientific research driven by competition *without* hurting the rules of censorship. To put it bluntly, they could open up to the West without having to turn their back on the East. This stunt was, of course, contingent on observing the taboos of the communist regime and collaborating with it as expert advisors or planning officials at various levels of the party-state. As an optecon you could be a fellow of a research institute today, a head of department in the Planning Office tomorrow, and a member of the Central Committee the day after tomorrow, or just the other way round. To cite Albert Hirschman, exit and voice were rare; instead, loyalty based on a mix of conviction, inertia, and survival instinct prevailed. The optimal planners rarely became dissidents; they were dwarfed by market reformers in this respect.

While the optecons had much in common with both of their rivals, they did not foster equidistant relations with them. The recurrent ideological attacks launched by the polecons scared the optecons,[93] whereas the competition with the refecons was more peaceful. For a mathematical planner to forge an alliance with the latter was almost a natural move, but with the former it was rather a tactical compromise. At a certain point, an optecon could not team up with a polecon who believed in the "dialectics" of economic laws including the freedom of the Central Planner to change them. Both the optecons and the refecons were dissatisfied with the performance of central planning and wanted to improve it through evolutionary change. Imbued with the optimism of social engineering, both promised Rationalization (writ large) in their scientific programs. However, the market reformers pledged to make the planned economy rational by changing the behavior of economic actors through new institutions rather than training them, like the mathematical economists proposed, how to conduct themselves "more scientifically" in the framework of the old ones. The optecons did their best to reveal the inexactitude and sterility of official political economy, but they also criticized the methodological sloppiness of market reformers.[94] Nonetheless, they more easily could agree together on the values of scientific quality, transparency, innovation, East-West exchange of ideas, and so on, more than any of them with the polecons. It happened time and again that reformers became optimal planners and *vice versa*, or these two egos coexisted in the soul of the same scholar for a while.[95]

Then why did cooperation between the optecons and the refecons not prove to be a long-term solution leading to the integration of their research

programs? Was the hubris of the former the main reason for their isolation? Or did the majority of optecons count as excessively interventionist and, therefore, opportunist[96] in the eyes of the refecons? Or, on the contrary, were some optecons irritated by those refecons who—as young Stalinists—had denounced "bourgeois" (mathematical) economics in the early 1950s?[97] Or was it the optimal planners' preference for formal analysis to verbal-institutional study that alienated the reformers from them? Most likely, all these reasons contributed to the sharpening of the demarcation lines around the optecons' research program, which stiffened their professional status and roles. In addition, adhering to the principle of *divide et impera*, the ideological supervisors of economic sciences were always keen on inciting conflicts between the two groups, threatening both with excommunication for heresy. As a result, the marriage between neoclassical knowledge and institutional experience did not take place and in the declining phase of the optimization program the scientific planners had to console themselves with other research fields within mathematical economics.[98]

IS OPTIMAL PLANNING *PASSÉ*?

The readers may put down our volume in a rather sad mood. They have been presented a research program that, moving back and forth, ended up as a typical Eastern European project of innovation in technology or business life. Ingenious ideas, comparable to those in the West, struggled for recognition in a demotivating social environment. They seemed successful at the outset, were overblown with the fervor of neophytes and instrumentalized by politics, failed in practice but did not vanish. The program moved ahead producing ambitious models on this side of the Iron Curtain at a time when it already began to retreat on the other. The inventors tried their best to save the original ideas of the program by fine-tuning its technical components in order to make it work outside the laboratory. Meanwhile, optimal planning cracked under the burden of its own ambiguities and fallacies, and the fiasco could not be primarily attributed to censorship and other machinations of the thought police. Experimentation was stopped by an abrupt change in the real world, the collapse of communism, that made the optimization efforts as a whole questionable in retrospect. During the implementation of the program, many of its followers got too close to the ruling elite and narrowed the opportunities for alternative inventions. As one of my interview partners in Hungary put it, optimal planning, just like alchemy performed in royal courts centuries ago, will certainly be exhibited in the virtual museum of human thought, but we will not know if the stone of the wise produced by it accelerated or slowed down the progress of science.

This is the seamy side of our story. Admittedly, our comparative research program on the evolution of economic ideas under communism was (and is) a little schizophrenic. Besides reminding the reader of epic failures, we also would like to show the sunny side of that evolution without, of course, persuading anyone to repeat the communist adventure. The Introduction could not conceal that we launched this book project with rather gloomy working hypotheses. Today, we see the intricacies of planning concepts more clearly and have revised some of our assumptions concerning, among other things, the two stages of evolution, the meaning of rationality, and the typology of optimal planning accordingly. As a result, the overall appraisal of the research program has not got significantly brighter. However, it became clear that in scholarly terms optimal planning proved to be the most creative and influential research program in economic science of the communist era. Indisputably, it enriched universal economics in many crucial fields such as input-output modeling, linear programming, general equilibrium theory, welfare economics, mechanism design theory, control theory, and—indirectly—concepts of disequilibrium. Yet, it was probably the greatest merit of these scientific discoveries that they revived the Socialist Calculation Debate,[99] in most cases eclipsing the work of market reformers, not to speak of textbook political economists, in terms of scholarly quality. In the Eastern Bloc as a whole, the optimizers did much for the rehabilitation of mathematical culture (and, more broadly, of the ideal of exactitude, quantification, and formalization) in economic thought in general and for the takeover of key concepts of neoclassical economics in particular. For example, no matter if leading mathematical economists had contended tactfully or hoped sincerely that the Marxian labor theory of value would not suffer from the conceptual apparatus of optimization, it did suffer immensely. More than thirty years after the collapse of communism, hardly anyone among serious economic theorists tried to resuscitate this theory in the ex-communist countries.

In a wider context, taking back the notion of economic rationality and starting to "decollectivize" it inflicted vast damage on the once celebrated concept of central planning. Maybe, to nuance the title of this chapter, not all important aspects of rationality were found by the optimal planners but some of the aspects they found did not get lost again. The requirement of coupling the concept of rationality with individual (and later with institutional) choice as cornerstones of standard economic inquiry survived and was carved in stone in the course of the neoclassical upswing under post-communism. Last but not least, descending from High Theory to earthly matters, optimal planners were rightly proud of generating indispensable empirical knowledge and its rational assessment by means of input-output analysis. To sum up, these accomplishments helped the economists climb out from the hole in which they sank at the end of the Soviet twenties, but their dream about the

reintegration of Eastern and Western economic thought, capitalizing on their own theoretical discoveries and local empirical knowledge did not come true. The most exciting and rewarding opportunity, namely, to attain rapprochement via New Institutional Economics remained largely unexploited in communist times.[100]

And so, our volume could not be finished with anything close to a happy ending, not even in the sense of what Jürgen Habermas called *nachholende Revolution* at a societal level. Arguably, catching up with standard neoclassical thought gained momentum *after* the communist system had collapsed. Prior to 1989, reintegration was severely inhibited by the fact that none of the leading theorists of optimal planning admitted the failure of their mission clearly, and such an admission (not an apology, of course) is still due.[101]

Their sending of a "Never again!" message might have moderated expectations today about reigniting the Socialist Calculation Debate and challenging the impossibility thesis with the magic bullets of our age, artificial intelligence, including machine learning,[102] which offer behavioral intent prediction, datafied knowledge production, algorithmic governance, and so forth. Like it or not, economists of a collectivist persuasion who are familiar with these novel disciplines and methods have begun to claim that real-time insights in production and exchange as well as in changes in technologies and consumer preferences are possible. Moreover, they add, there is also decent chance to collect and centralize near-perfect information by eliminating the distortion of data by fallible humans.[103]

Certainly, Big Data and AI oblige economists to rethink the century-long debate, and it is very likely that some of the Austrian arguments will need to be amended or abandoned. Owing to the fact that during the past two decades the very notion of data has expanded rapidly (including non-verbal information *en masse*), their quality has improved immensely, their collection and processing have become far more accurate and faster than ever before, and short-term market prediction can rely on real-time information managed by self-correcting models operating on online platforms (cf. "anticipatory shipping"). Today, any of the big tech companies uses more data (and more efficiently) than the national planning office of a large country in the communist epoch. Nevertheless, crucial elements of the Mises-Hayek position, notably, those related to tacit knowledge and distorted information, seem to remain valid even in an imagined non-hierarchical collectivist economy. Also, it is doubtful whether the AI models are capable of sustaining longer-term planning and can release themselves from the prison of the past and the present, say, in deciding on technology and consumer taste in the future. The old question of "How to craft plans based on knowledge we do not have?" still waits for an answer. Finally, are the extremely complex new models really computable, or—returning to the beginning of the calculation debate—will

the would-be planners have to face an "impossibility (of computation) thesis" again?

In any event, rationality seems to be back again, allegedly taking the wind out of the Austrians' sails. "Why wouldn't we try to optimize the economy again?" ask some new-collectivist thinkers—but at this point without one-party dictatorship and imperative planning, yet with dominant collective ownership, workers' self-management, decentralized planning, and regulated markets?[104] Optimizers in the previous century experimented in the framework of vertical collectivism. *Perhaps* under the rule of horizontal collectivism and with the help of machine learning, the program of optimal planning will work. *Perhaps* . . . , and the trap of collectivism may close again.

The world has just begun to fear the use of artificial intelligence by dictatorial regimes. Thus far, these have focused on surveilling and brainwashing their citizens.[105] But what will happen if the Big Brother decides to switch to the control of the national economy as a whole, trusting in a conversion from "platform capitalism" to a sort of "platform collectivism?" Hopefully, and very likely, this will not work or at least will not work efficiently. Nevertheless, knowing the disastrous consequences of an earlier failed experiment with macroeconomic control starting with the First Five-Year Plan at the end of the 1920s, one does not look forward to witnessing another six-decade-long bankruptcy.

NOTES

1. In the pre-1989 period, these scholars were among the most credible analysts of the rise of mathematical economics in the English-speaking world: Edward Ames, Abram Bergson, Morris Bornstein, Robert Campbell, Martin Cave, Maurice Dobb, Robert Dorfman, David Dyker, Michael Ellman, Alexander Erlich, George Feiwel, Philip Hanson, John Hardt, Paul Hare, Richard Judy, Michael Kaser, Carl Landauer, Don Lavoie, Herbert Levine, Moshe Lewin, John Michael Montias, Egon Neuberger, Alec Nove, Mario Nuti, Jan Prybyla, Peter Rutland, Leon Smolinski, Nicolas Spulber, Pekka Sutela, Vladimir Treml, Benjamin Ward, Peter Wiles, Eugene Zaleski, and Alfred Zauberman.

2. Even Aron Katsenelinboigen (1980, 30) who emigrated from the USSR in 1973 and had a strong opinion about many of his Russian colleagues showed understanding, for example, for the leaders of the mathematical economics movement: ". . . one could view Nemchinov as a collaborator with the Stalinist regime. The refusal of a creative person to collaborate with a totalitarian regime is a moral act of selfless asceticism, difficult for most people. Activity, with its possibility for creation, is too important. Moreover, a young person once fallen into the rut of collaboration finds it difficult to leave. Such is the subjective side of the behavior of many scholars in totalitarian regimes. However, this activity has some positive aspects. Since the regime is already formed, the presence of decent people with power can, in changing conditions, result in a renewed moral atmosphere and the creation of new directions in science."

The market reformers faced the same dilemma. Recently, János Kornai (2019) who, following the 1956 revolution, had already been confronted with this ethical predicament and opted for (half-hearted) collaboration, likened himself to Frankenstein for advising Chinese communist leaders to liberalize their economy in the 1980s and thereby contributing to the rise of a new authoritarian empire. See also note 80 and 105.

3. There were important reasons for the Western specialists to express cautious opinions about the research programs of their Eastern Bloc colleagues. They felt compassion for their peers exposed to repression; at the same time, they wanted to do field research—a forgivable motive for sure. For the story of how an American scholar's articles caused difficult moments in the life of Kantorovich, see the chapter on the Soviet Union in this volume, Campbell (1960; 1961) and Boldyrev and Düppe (2020, 271).

4. They can look back on the noble tradition of Russian mathematics from before the 1917 revolution and the world-famous economists of the 1920s like Aleksandr Chayanov, Grigorii Feldman, Vladimir Groman, and Nikolai Kondratiev who spoke the language of mathematics fluently. True, this fame had not been shining bright until historians like Alexander Erlich (1960) and Nicolas Spulber (1964) rediscovered these scholars in the early 1960s. Interestingly, Evgeny Slutsky and Boris Brutskus were not among them at the time. For many years, the Vladimir Dmitriev—Aleksandr Bogdanov—Pavel Popov—Wassily Leontief—Leonid Kantorovich lineage was more acceptable in the USSR, especially after Leontief was permitted to re-enter his fatherland. For Leontief's symbolic blessing to this history of ancestry, see Leontief (1960).

5. The reading list of the most important journal articles on the evolution of optimal planning would be incredibly long if one also took into account, beyond the authors listed in note 1, scholars publishing in French, German, and other languages.

6. In his foreword to Zauberman (1975, VII–VIII) Gregory Grossman also used this word but elegantly distanced himself from the author' enthusiasm. Prior to this book, Zauberman was a co-editor of a pathbreaking work on *Planometrics* in 1967. In 1976, he published a voluminous book on *Mathematical Theory in Soviet Planning*, which provided a rich background material to the book discussed here.

7. This is how Aron Katsenelinboigen (2009) remembered one of his conversations with Kantorovich: "He said that <if the government supports me all economists will think like me in five to seven years. And a new era will begin in the economy of our country>."

8. In an earlier article Zauberman (1969, 2) examined the "rapprochement between East and West in mathematical economic thought." He drew a very optimistic picture of mutual help in developing new mathematical techniques but remarked that it was not sure that the "reconciliation of historical materialism and econometric formalism" would be successful.

9. Similar to Oskar Lange in the Socialist Calculation Debate, Zauberman (1975, 52) was contented with a vague complaint about socialist bureaucracy, particularly, the "inertia of the planning and controlling apparatus."

10. Zauberman knew that, besides the prices, the dual side of the models could deliver the optimal size of capital investment, profit, and interest. However, he did not realize that while the rehabilitation of these categories helped the market reformers, it also stole the show from them because the optimal size was specified by the computer instead of emerging in the market process.

11. This book was a sequel to Ellman (1971).

12. Surprisingly, a few years later, he published a sharp-tongued article against Tinbergen's convergence theorem. See Ellman (1980).

13. Some years before, he settled the issue of economic rationality for himself with these words: "What Barone and Mises did not realize is that it is perfectly possible for an economy to function, and in many respects perform exceedingly well, even if the plans are inconsistent and micro irrationalities abound" (Ellman 1968, 27). Ten years later, he amended his position a little, though remained far from promulgating the Austrian "impossibility thesis": "the theory of decision making implicit in the Marxist-Leninist theory of planning is inadequate because it ignores the fundamental factors of partial ignorance, inadequate techniques for data processing, and complexity" (Ellman 1978, 249). "Subordinates may transmit inaccurate information, the process of transmitting information may destroy some of it, and the addressees of information may not receive it" (251). "In this respect the Marxist-Leninist theory of planning suffers from the same weaknesses as neo-classical price theory" (255). These remarks did not go much beyond Lange's or Zauberman's criticism of "bureaucratization."

14. Cf. the chapter on the Soviet Union in this volume.

15. The picture of self-centered marketeers did not differ much from the one painted by textbook political economists and hardliner politicians in the communist countries as well as by certain theorists on the New Left. See also Ellman (1968) published in the *Socialist Register*.

16. Meanwhile, both scholars lost interest in studying optimal planning: Zauberman published on the history of game theory in the Soviet Union and Comecon trade while Ellman focused on planning and market reforms in a comparative perspective. Ellman's 1979 volume on *Socialist Planning* (republished in 1989 and 2014) discussed mathematical methods less and less.

17. In 1966, the SOFE guru Nikolai Fedorenko put this less mildly when he spoke about "descriptive" versus "constructive" political economy to distinguish old-school textbooks from optimal model building. His outspoken older colleague Aleksandr Lur'e added: official political economy was not descriptive but destructive (Ellman 1973, 9).

18. He argued that not only the older generation of Leonid Kantorovich, Vasilii Nemchinov, and Viktor Novozhilov but also their younger colleagues such as Nikolai Fedorenko and Stanislav Shatalin were sincere devotees of central planning in some collectivist (not necessarily administrative-hierarchical) framework. Their affirmative attitude could not be explained solely by self-censorship (Sutela 1984, 92–97, 198–99).

19. Sutela (1991, 40) already took a larger distance to the optimal planners and their illusions: the planning bureaucracy was "regarded as unselfish servants of the system with no power aspirations or interests of their own. All the <petty tutelage> was simply seen as a consequence of a badly designed hierarchical division of labor, not as a natural way of exercising ownership rights in a situation where the planners and ministries were responsible for the performance of <their> empires. Since the late sixties, however, the bureaucrats have often been accused of sabotaging the reform of 1965. During the seventies planners generally supported the mechanization of plan calculations but fiercely opposed any reform that would lessen their concrete power over resource allocation."

20. Cf. the chapter on Yugoslavia in this volume.

21. Some years later, Sutela (1991, 45) reassessed optimal planning in an even more pessimistic mood. He discovered Mises and Hayek but did not reject Lange and subscribe to the impossibility thesis. Witnessing how during *perestroika* the idea of market reform replaced that of improving the plan in the hearts and minds of a number of Soviet mathematical economists, he gave up any hope about a "workable new course." SOFE, wrote Sutela "really has no place for money as a liquid asset, credit, foreign trade or the conversion of military production. Questions of competition, ownership, the legal framework and entrepreneurship are all absent. This was the technocratic and romantic phase of Soviet economic reformism."

22. The same applies to David Prychitko (2002) who offered a powerful critique of the decentralized projects of communist planning (particularly in Yugoslavia), complementing the writings of his close colleagues on central planning. Peter Boettke's (2000a) pioneering series of volumes republishing most of the important contributions to the consecutive waves of the Socialist Calculation Debate contained only some of the relevant essays of Eastern European scholars. Lavoie (1986) was supported by a rich review of the literature but his Eastern European sources were dwarfed by references to Western star economists.

23. Both Mises in the various editions of *Human Action* ([1949] 1966, 694–711) and Hayek in *The Fatal Conceit* (1988, 85–88) confined themselves to a general summary of their thoughts on socialist calculation. Also, they retained their suspicion about formal analysis. As Mises ([1949] 1966, 698) says, "the mathematical economist, blinded by the prepossession that economics must be constructed according to the pattern of Newtonian mechanics and is open to treatment by mathematical methods, misconstrues entirely the subject matter of his investigations. He no longer deals with human action but with a soulless mechanism." Hayek (98) talks about macroeconomics that "seeks casual connections between hypothetically measurable entities" and "may sometimes . . . indicate some *vague* probabilities" as well as about mathematics, "which must always impress politicians" and "is really the nearest thing to the practice of magic." Although the new generations of Austrian economists made friends with mathematics, the reservations of their predecessors about mathematical methods poisoned the climate of the ongoing debate on rational calculation. Also,

they gave an advantage to the neoclassical experts who felt reinforced to regard the Austrian discourse as imprecise, ideological talk.

24. On differences between Austrian and neoclassical theory in interpreting the concept of rationality, see Lavoie (1986, 10–14). On the limitations of neoclassical analysis, see Lavoie (1985, 100–113) and Boettke (2000b, 8–22).

25. Apparently, they accepted Mises' ([1949] 1966, 703) paternalistic words in *Human Action*: the socialist reformers "want people to play market as children play war, railroad, or school. They do not comprehend how such childish play differs from the real thing it tries to imitate." Boettke (1990; 1993) examined the reform economists with more compassion but showed little interest in them in the long period between the NEP and *perestroika*.

26. Rothbard (1991, 72) warned the optimal planners about the danger of building "garbage in, garbage out" models.

27. As Lavoie (1986, 9) puts it, ". . . the essence of the <knowledge problem> argument is not simply that plant managers know things that the Central Planning Board does not or the communication of this knowledge from the former to the latter would . . . entail the cost of losing some data or accuracy. The problem is rather that the relevant knowledge is inarticulate. The producers know more than they can explicitly communicate to others. While the market marshals this dispersed knowledge without requiring its articulation all these market-socialist models necessarily require the full articulation of localized knowledge to the Central Planning Board during the <dialogue.>"

Boettke (1990, 36) enumerated the main difficulties of socialist calculation and planning as seen by the Austrian School: "(1) property rights and incentive problems, (2) problems of informational complexity, (3) epistemological (tacit knowledge) problems, (4) the totalitarian problem." The last point pertained to the underlying hypothesis, according to which central planning *logically* presupposes some kind of dictatorship. Boettke (2001, 41) summed up the Austrian message succinctly: ". . . socialism is *impossible* precisely because the institutional configuration of socialism precludes economic calculation by eliminating the emergence of the very *economic* knowledge that is required for these calculations to be made by economic actors."

28. Here, Lange ([1964] 1967, 158) proudly declared: "my answer to Hayek and Robbins would be: so what's the trouble? Let us put the simultaneous equations on an electronic computer and we shall obtain the solution in less than a second." See also Rothbard (1991).

29. On the contrary, they tended to demonize the Austrian School as a refuge for free-market fanatics (while borrowing some of their arguments about evolutionary institutional analysis).

30. "International and domestic political elites created a package of neoliberal ideas to take advantage of the changing political situation around 1989. These elites, as well as right-wing economists and activists, co-opted critical, transnational socialist discussions and presented them, along with a narrow version of neoclassical economics, as calls for private property, hierarchy, and markets within capitalism. In doing so, they distorted the neoclassical economic discussion of socialism and markets into neoliberal ideology." ". . . Around 1989, these elites began to implement neoliberalism . . . " (Bockman 2011, 12, 217).

31. This expectation was supported by prominent economists such as Pranab Bardhan and John Roemer (1992) who, attributed the failure of market socialism to the lack of democracy (instead of the lack of market and private property) and trusted in some sort of rational macro-planning. See their sharp dispute with Andrei Shleifer and Robert Vishny (1994).

32. This is how, for instance, Feygin (2017, VIII) starts his dissertation: "I challenge the prevailing historiographical narrative that so-called Soviet <liberals> <learned from the West> and instead show that reform-minded economists became equal partners in trans-European intellectual communities."

33. Feygin (2017, 4) talks about "cold-war science" to refer to a critical impact of geopolitical drivers on mathematical economics in the USSR. Vincent Barnett (2009) and Joachim Zweynert (2006; 2018) examine the evolution of Soviet economic thought in a much longer perspective and are more sensitive to methodological nuances. See also Barnett and Zweynert (2008).

34. Citing Bert Hamminga, Hands (2016, 3) employs the term of a "set of elementary plausibility convictions."

35. Cf. Dorfman (1976).

36. See also Leeds (2016a, 274, 351). Boldyrev and Düppe (2020, 272) note that, surprisingly, Kantorovich was "never seriously interested in general equilibrium theory or game theory."

37. Feygin and Leeds are clear exceptions. However, perhaps due to the fuzzy designations used in the USSR at the time, they regard both the *tovarniki* who were part of the official political economy (but advocated the broadening of the "commodity-money relations") and the *khozraschet*-prone optimal planners as market reformers/socialists. Feygin's (2019) *tovarnik* hero is Yakov Kronrod who, to say the least, did not maintain a friendly relationship with the optimal planners.

38. Boldyrev and Kirtchik (2014) and Boldyrev and Düppe (2020) mention the socialist leanings of Polterovich and Kantorovich several times. Leeds (2016a, 295–96), too, speaks of Novozhilov's Marxist beliefs, but he is also unsure to what extent these experts were turncoats defending their "true" positions against the censors. It is only Feygin (2017, 9) who says explicitly that many Soviet mathematical economists were "dedicated Soviet patriots and Communists who were trying to deal with problems of the modern territorial state that thinkers west of the Iron Curtain were grappling with at the exact same time." As an exception within this group, Zweynert (2006, 189–92) stresses the devotion of Soviet economists, verbal or mathematical, to social engineering.

39. On Kantorovich, see also Bockman and Bernstein (2008).

40. Referring to Hayden White, Rindzevičiūtė (2010, 290) dislikes evolutionary schemes based on a simple "rise and fall" dichotomy. Leeds and Feygin are uninterested in the logic of the decline of optimal planning: Leeds (2016a, 343) applies the term "accomodation" for decline while Feygin (2017, 6, 156–262) sees the Brezhnev years not as a period of stagnation and decay but that of "conservative reform," in which "a gradual improvement of technical elements of Soviet planning practice"

took place. True, Feygin also talks about "the closing of the soviet economic mind" during these decades (242). Finally, Boldyrev and Düppe (2020, 278) refuse to think about the evolution of Kantorovich's work "in terms of success or failure."

41. Kantorovich, for example, quits the field of macro-optimization during the 1970s.

42. Leeds (2016a 289–90) maintains that with time, "input-output models changed from description and prescription to prediction. This was perhaps the greatest effect of input-output modeling." On the opportunities to switch to econometric research, see Feygin (2017, 81, 268–81).

43. Cf. Leeds' (2016a, 379–422) case study of the "Gaidar Boys" and the concept of the "administrative market." In his view "the optimal planners were normative theorists. They did not systematically study the institutions of the Soviet Union. They created an ideal mathematical structure, and then dreamed up institutions that might realize it. . . . In contrast, the young economists were empirical theorists. They began not from the math but from the institutions as they actually existed" (418).

44. Most of them were cited in the national chapters such as Caldwell (2003), Doležalová (2018), Kaase, Sparschuh, and Wenninger (2002), Krause (1998), Mau (2017), Mlčoch (2010), Szamuely and Csaba (1998), Wagener (1993; 1998); see also Mau (1990; 1995), Shukhov and Freidlin (1996), and Zhang et al. (2016). Many "insiders" published brief chapters in the Palgrave collection on the planned economy (Eatwell et al. ed, 1990). This volume represented the last (and surprisingly soft) word on planning by the international research community before the 1989 revolutions.

45. Cf. the section "Revisiting the Soviet Case" above, especially Hands (2016) and Boldyrev and Kirtchik (2017). For a long time, Western observers did not attribute as much attention to the conceptual differences between neoclassical and optimal planning models as to the linguistic tricks with which mathematical economists in the communist countries tried to camouflage the similarities by inventing special terms for optimality, utility, or the shadow price and prove that Marx, Engels, and Lenin were forefathers of mathematical modeling as a guarantee for scientific accuracy.

46. Here Kornai returns to the "German" position in the first *Methodenstreit*. For more details, see the chapter on Hungary in this volume.

47. The quasi-axiom of plannability ("intrinsic governability," to use Roumen Avramov's phrase) had had a long history before it became associated with imperative central planning at the turn of the nineteenth and twentieth century. The concept did not come out of the blue. Without recapitulating the evolution of planning doctrines prior to the October Revolution, one can safely claim that the birth of the idea of War Communism, that is, the first (failed) attempt at some kind of mandatory macroeconomic control under Soviet rule, was contingent on a whole series of synergetic effects. They included Marxism and its interpretation by German social democrats, the end-of century utopias in Europe and beyond and their influence on Bolshevik thought, the idea of *Naturalwirtschaft*, the theory of the German war economy, as well as their common philosophical background of a collectivist variety of evolutionary optimism backed by a positivist approach to social sciences. Nevertheless, the spell of plannability could not have survived safely without the three alleged success stories of the interwar period: Stalinist and national-socialist planning and the New Deal.

The approval of planning to be performed by central government agencies could be articulated in cautious understatements like those of John Maynard Keynes in his 1926 essay on "The End of Laissez-Faire" but also in crude nazi or fascist slogans swarming in their party programs. It could be expressed in the Hegelian language of Marx detesting spontaneity and saluting the class consciousness of the proletariat, in technocratic terms applied by Otto Neurath or Walther Rathenau to praise in-kind regulations in a war economy, and also in the romantic style of utopian novels such as William Morris's *News from Nowhere* (1890) and Edward Bellamy's *Looking Backward* (1887). You could be a parliamentary democrat like the protagonists of the New Deal, a fan of *Räterepublik* like Neurath, or an economic advisor and politician serving a dictatorial regime like Hjalmar Schacht, Nikolai Voznesenskii, or later Oskar Lange.

Approaching our research field, the economist subscribing to the idea of plannability after 1945 could be of social-democratic and communist persuasion, a heir of "military Keynesianism," to use Michał Kalecki's phrase describing the economics of national socialism, like the Hungarian Béla Csikós-Nagy and Mátyás Matolcsy, a fan of Henry de Man's doctrine of *planisme*, maybe in Romania, a "bourgeois" economist like the Czech Karel Engliš combining Keynes' program with the teachings of the Austrian School of Economics in his theory of the "regulated economy," or a steadfast Marxist who like Lange applied neoclassical instruments to prove the rationality of a centrally administered economy. (The term "wartime capitalistic socialism" coined by the Bulgarian liberal Assen Christophoroff resignedly reflects such hybrid doctrines well.) Even the attitudes of many scholars in the interwar to private property were bad predictors for being an enthusiast of central planning. One finds among its devotees of German national socialism who, while resisting large-scale nationalization, endorsed strict governmental planning as well as various socialists and social-liberals ranging from the old Karl Kautsky to the young Karl Polányi who also disliked all-encompassing and hierarchical state ownership but favored some kind of—democratically designed—central planning. Moreover, a number of Russian agrarian (*neo-narodnik*) economists like Aleksandr Chayanov may be mentioned in this regard who insisted on the freedom of small-and medium-sized peasant property (private or communal) but also acknowledged central planning based on a certain degree of state coercion. (See also the chapter on the GDR in this volume.)

48. See Rawls (2005, 49) and the excellent books by Gerovitch (2002) and Erickson et al. (2013). The latter called my attention to Rawl's opinion.

49. One of the most exciting problems of the evolution of mathematical economics under communism is why game theory did not succeed to conquer the discipline in spite of the early discovery of its usefulness by eminent scholars like Viktor Volkonskii and Yurii Gavrilets in the Soviet Union, Tiberiu Schatteles in Romania, or Kornai and Lipták in Hungary even if they replaced the term "bargaining" with those of "dialogue" and "negotiation." Also, no mathematical incompatibility was to be expected since a linear programming task can be described in a game-theoretic form. In order to model the interplay of main economic actors in the planned economy, the optimizers should have defined the strategies of these actors, including that of the Central Planner—a risky venture for sure. For instance, they should have asked

"what the Soviet rulers maximized" just like Kontorovich and Wein (2009) did many years later. Below, we will see that approaching New Institutional Economics could have helped them raise such questions. In any event, it seemed much easier (and more elegant) to construct a single Big Optimal Plan than to find the optimum in thousands of smaller but important games and aggregate their outcome on the macro level. For Lubomir Mlčoch's concept of institutional games, see the chapter on Czechoslovakia.

50. For more on faith and opportunism/cynicism as well as on their covert and overt variants, see note 2 and 18. Koopmans described Kantorovich after their meeting in 1965 as a person of "self-imposed political cautiousness . . . beyond the call of duty and necessity" (Boldyrev and Düppe 2020, 274). Katsenelinboigen (1980, 43–44) recalls that "I did not succeed in understanding whether it was out of tactical considerations or from conviction that he wanted to reconcile shadow prices with labor value." The chapter on Hungary in this volume brings many examples of this dilemma by comparing the approaches of Bródy and Kornai to censorship. The following words of Schatteles (1970, 196) also demonstrate ambiguity between expressing loyalty to communist principles and accepting part of "capitalist criticism": Mises's "rationality postulate is essentially a capitalist one from which he tries to prove the impossibility of socialism. But the problem of computation in socialism is—and must be—beyond the question thus put. For the economist, the social system is a <fact of the world,> his task being the study of this fact and to compute the computable in the field of planning practice defined by this very <world>." Multiple examples for the durability of Marxist views of prominent mathematical planners such as Mária Augusztinovics, Aleksander Bajt, András Bródy, Emilian Dobrescu, Josef Goldmann, Branko Horvat, Evgeni Mateev, or Miroslav Toms can be found in the national chapters.

51. See note 45. Andrei Belykh quotes a critic of optimization from 1943: "Kantorovich suggests the optimum, and who else suggests the optimum? The fascist Pareto, Mussolini's favorite" (see the chapter on the Soviet Union). The censors and their allies among the official political economists had a hard time when they accused the mathematical economists of formalism, subjectivism, revisionism, anti-Marxist deviation, or being the Trojan horse of bourgeois economics but did not really understand the jargon these spoke. Accordingly, it was not the excellence of optimal planning theory that convinced them of relaxing the grip on the experts but its expected utility in running the economy and strategic importance in military affairs. At any rate, the process of recognition was very slow if one considers the fact that Kantorovich, Novozhilov, and Nemchinov had worked out the basic principles of optimal planning in 1939, 1943, and 1946, respectively (cf. the chapter on the Soviet Union).

52. Between the two, in 1965, Novozhilov, Kantorovich, and Nemchinov (posthumously) were awarded the Lenin Prize: of similar importance was the fact that step by step they succeeded in occupying strategic positions in economic research and education as well as within *Gosplan* and other key institutions of the party-state. An important milestone in the international recognition of optimal planning was Leontief's Nobel Prize in 1973.

53. The Walras model of general equilibrium pertains to the economy as a whole but does not contain an overarching objective function. At the same time,

the Dorfman-Samuelson-Solow model of linear programming does not aim at macro-optimization. Cf. Koopmans (1957) and Dorfman, Samuelson, and Solow ([1958] 1987).

54. While trying to craft big optimization models with moderate success, many optimal planners put up with smaller ones. Mathematical experts in China enjoyed the advantages of latecomers, skipped the overambitious phase of optimal planning, and have continued to work on smaller-scale projects until today. See the chapter on China in this volume. See also note 80.

55. I have no room in this chapter (nor enough knowledge) to discuss the troubled fate of indicative planning in market economies.

56. Cf. Kovács (1990; 1992).

57. For reasons why this East-West cultural encounter did not take place, see Aligică and Terpe (2012), Avramov (2012), Franičević (2012), Kochanowicz (2012), and Kovács (2012).

58. This came in handy for the textbook political economists (see below).

59. See note 49. The chapter on Hungary includes the example of verbal reform economists Tamás Bauer and Attila K. Soós, revealing the sad fact that their deep knowledge of planning regimes in many communist countries were hardly processed into mathematical models to increase its accuracy and testability.

60. János Kornai (1959; 1980) had started examining some of these phenomena in *Overcentralization* in the 1950s, that is, before he began to work on optimal planning, and returned to them in *Shortage* during the 1980s. See the chapter on Hungary.

61. In fact, some of these questions already had been asked by Boris Brutskus (1935) in Soviet Russia simultaneously with Mises in the early 1920s.

62. On conviction, see note 2, 18, and 50. Of course, institutional inertia also mattered, especially in the case of model builders who could not do armchair research on their own but were exposed to cooperation with fellow scientists and assistants, not to speak of the availability of computer centers. See also the term "plan mongering" below.

63. The scientific career of Józef Pajestka is a good example for how one gets from the estimation of production functions in Poland to sketching up megatrends of civilization.

64. See the chapter on Yugoslavia.

65. A remarkable exception is a group of Russian scholars, including Sergei Guriev, Konstantin Sonin, and Ekaterina Zhuravskaia. For more on their scientific and political attitudes, see Leeds (2016a, 431–40). A little earlier, the concept of the "administrative market" seemed to provide a promising opportunity for reform-minded and mathematically literate economists (like Piotr Aven, Anatolii Chubais, Vitalii Naishul, and Viacheslav Shironin, some of whom became members of the Gaidar team later) to join forces under the auspices of a similar research program (Leeds 2016a, 361–419).

66. See the chapter on Czechoslovakia.

67. A number of mathematical economists in our countries (e.g., Eduard Braverman, Viktor Polterovich, Wojciech Charemza) turned to disequilibrium analysis but with much less commitment against GET than János Kornai in his *Anti-equilibrium*.

Cf. Boldyrev and Kirtchik (2014), Kirtchik (2019), and the chapters on Hungary and Poland.

68. This feeling originated not only in the dramatically poor record of textbook political economy but also in the traditional prestige of mathematical sciences, especially in the Soviet Union. Leeds (2016a, 261) cites a founding member of the first economic-mathematical laboratory in Moscow Vladimir Kossov: "We felt like people defending ourselves with weapons against the savages. We could read, formulate the task, propose calculations. It gave us a sense of enormous moral superiority." Let me add that frequently pride was also due to a simplistic engineering view of planning: "I am right because my calculations were correct and my <machine> seems to work in the real world." (See note 17 on constructivism versus destructivism.)

Mathematical planners ridiculed the verbal specialists as bookkeepers preoccupied with their simplistic balances. Yet, the scorn often pertained neither to the bureaucratic attitudes of the "accountants" nor the roughness of their calculations but rather to the fact that this method of planning was considered to be heavily exposed to arbitrary political intervention, far more so than the complicated quantitative operations suggested by the optimizers.

69. For more on this "Faustian bargain," see the chapter on Hungary.

70. Optimal planning is probably one of those few fields in economic sciences, in which the *ex oriente lux* thesis is not without any foundation (see below). Eminent economic theorists (including Nobel Prize winners such as Arrow, Frisch, Hayek, Hurwicz, Koopmans, Leontief, Ostrom, Samuelson, Sen, Solow, Tinbergen, and Williamson) in the West have profited from outstanding scientific discoveries made in the communist world or—indirectly—from challenges stemming from not-so-outstanding scholarly products fabricated there, or—even more indirectly—from the reality of the planned economy.

71. Philip Hanson (2003, 97) remembers visiting a laboratory in TSEMI in 1964 where he saw "the excellent economist Viktor Volkonskii and a group of young women, all math graduates, armed with copies of Samuelson's *Foundations of Economic Analysis* and English-Russian dictionaries."

72. See the chapter on the GDR.

73. Cf. Gerovitch (2002, 264–88), Rindzevičiūtė (2010), Erickson et al. (2013, 1–21), Düppe (2016), Leeds (2016b), and Feygin (2017, 260–323).

74. Cf. Bockman and Bernstein (2008), Düppe (2016), Boldyrev and Düppe (2020).

75. See the Hungarian chapter.

76. For example, Schatteles (1970) was originally presented at a conference in Novosibirsk. Soviet mathematical economists took part in regular meetings in Warsaw, Prague, Budapest, and Berlin but visited Yugoslavia as well (Katsenelinboigen 2009). In these cities they could also meet top scholars from the West (Boldyrev and Düppe 2020, 267) but some of them, like Koopmans and Leontief, traveled to Moscow. In the framework of his LINK project, Lawrence Klein visited many countries of Eastern Europe and China. IIASA in Laxenburg, Austria was also a crucial place of East-West encounters. Łódź provided home for an annual workshop of econometricians. Even in Bucharest there were regular symposia with Soviet and French experts, respectively, in the brief period of opening at the turn of the 1960s and 1970s. Experts from the planning offices met regularly under the aegis of the Comecon but also by crossing the Iron Curtain (cf. Guarné 2018). For more details, see the national chapters.

77. For Vasilii Nemchinov's role in establishing the Economico-Mathematical Laboratory in Prague and Sofia, see the chapters on Czechoslovakia and Bulgaria.

78. Here mathematical economists were much less enchanted by optimal planning and accepted the basics of neoclassical economics and standard econometrics (cf. the "Klein connection") earlier than in most communist countries. As mentioned, leading scholars like Aleksander Bajt and Branko Horvat did not give up their Marxist views entirely but supported not only macro-but also microeconomic analysis of planning once imperative central planning was replaced, first by indicative, then by so-called "social" planning. Simulating market socialism by means of optimal models was not popular among local experts since the Yugoslav economy had fragile but real markets. Another difference was that economic theorists working in and on the country discovered early on that these markets were exposed to heavy government intervention (a kind of informal planning) and tried to develop the existing neoclassical models of Yugoslav self-management with the help of new-institutional techniques. See the chapter on Yugoslavia.

79. The chapter on the GDR demonstrates that the rise could be interrupted (cf. "the revolution that wasn't"). Cybernetics became a philosophical discipline, input-output research did not develop into optimal planning, and in 1971 (!) cybernetics and systems theory was condemned by the supreme party leader as pseudo-sciences.

80. Understandably, in our comparison China is always the (instructive) outlier. The first chessboard table of its national economy was completed during the Cultural Revolution in 1974. When Chinese scholars could have started to work out optimal plans, the country embarked upon a long journey of reforms that made imperative macro-planning questionable step by step: first through deregulation, then through privatization. Indirect macro-control became the rule, which relied on standard (Western) macroeconomic models and was implemented by means of monetary and fiscal incentives rather than mandatory planning targets even in the state sector that was shrinking anyway. Large-scale administrative decentralization (e.g., fiscal federalism) also required indicative methods of planning instead of imperative ones.

Ironically, unlike other communist countries before, in China the establishment of "centrally planned commodity economy" and later of "socialist market economy," to use the official designations, did not result in an upsurge of optimal planning. On the macro level, optimization was rather used in forecasting and checking the consistency of the annual and five-year plans that have not ceased to exist until today (However, since 2006, the "ministry of ministries," the National Commission for Development and Reform, does not carry the term "planning" in its name.). Of course, targeted interventions by the party-state in economic life abound, but these are not arranged in formal mandatory instructions. Nevertheless, informal recentralization can turn into a formal one. What is today outside the plan can get inside it tomorrow, and optimal planners may face an increasing demand for their services. Currently, the Chinese Society of Optimization, Overall Planning and Economic Mathematics has about 17,000 members (Chow 2005; Chen, Guo, and Yang 2005; Lin, Liu, and Tao 2013; Zhang 2016).

81. Undoubtedly, progress was contingent on the cultural baggage of mathematical knowledge economists brought along from the pre-communist era. Here the Soviet scholars had no strong competitors. Moreover, as the national chapters show, the ascent of mathematical economics in Belgrade, Berlin, Prague, and Sofia also was promoted by Russian émigré scholars (e.g., Oskar Anderson and Aleksandr Bilimovich) between the two wars. Nonetheless, in searching for the sources of quantitative methods in economics under communism, the chapter authors found in these countries a whole series of indigenous economic theorists, both communists and non-communists, who studied mathematics prior to 1945.

82. One can observe an interesting difference among the countries in the attitudes of mathematical economists to econometrics. While in most countries it served the transition from optimal planning to standard neoclassical research during the agony of communism, in Poland and Yugoslavia it evolved parallelly to optimization or even replaced it.

83. According to the joke spreading all over Eastern Europe before 1989, if the central plans had been correctly implemented the communist system would have crumbled much earlier.

84. Lange (1936, 70) regarded this a decisive threat to the survival of his model of market socialism in a really-existing planned economy: "By demonstrating the economic consistency and workability of a socialist economy with free choice neither in consumption nor in occupation, but directed by a preference scale imposed by the bureaucrats in the Central Planning Board, we do not mean, of course, to recommend such a system. . . . Such a system would scarcely be tolerated by any civilized people."

85. The chapters of this volume help preserve the memory of eminent scholars of their time, input-output analysts and linear programmers, who have not been given enough light in the shadow of the Lange-Kantorovich-Kornai triumvirate. Here is a very short list of them: Mária Augusztinovics, András Bródy, Xikang Chen, Emilian Dobrescu, Josef Goldmann, Jaroslav Habr, Branko Horvat, Evgeni Mateev, Krzysztof Porwit, Tiberiu Schatteles, Mijo Sekulić, Ivan Stefanov, Miroslav Toms, Aleksy Wakar, and Zhang Shouyi. No matter what role some of them played in communist politics or scientific management at certain stages of their lives, their work is part of the (more and more) hidden treasures of economic thought in their countries. Obviously, Soviet scholars, ranging from Nemchinov and Novozhilov to Katsenelinboigen and Volkonskii, have received much more attention in the history of economic ideas.

86. With time, Kantorovich and the TSEMI experts approached this type (Nemchinov called it "flexible planning") whereas Kornai, Schatteles, and Wakar started from here. While Wakar's theory of "direct account" was based on the paradigm of general equilibrium, it also yielded insights into problems of incentive incompatibility in a planned economy, thereby anticipating neo-institutional conclusions. See the chapter on Poland and the Soviet Union.

87. In the cavalcade of planning models it was enough for the Central Planner to change somewhat the definition of "strategic industries/products" that need intensive state control, or insert new constraints or a modified objective function in the model, and—as a consequence—the initially market-friendly versions of optimal plans returned to the traditional planning schemes.

88. Cf. Volkonskii (1967; 1973), Katsenelinboigen, Lakhman, and Ovsienko (1969), Petrakov (1971a, b), in which forbidden themes like the pluralism of interests and social goals as well as market feedbacks were discussed. Earlier, János Kornai also refused to calculate with a single objective function but did not demand to establish a democratic procedure for coordinating interests. See the Soviet and Hungarian chapters.

89. Josef Goldmann drew a similar conclusion with regard to popular discussion on planning goals. See the chapter on Czechoslovakia.

90. The attraction of automatization was so great that in the beginning even a pragmatic like Kornai could not resist it entirely (see the chapter on Hungary).

91. See the chapter on the Soviet Union. Cf. Gerovitch (2002, 279–88), Peters (2016, 107–90), Katsenelinboigen (1980, 147–56), Ericson (2019, 162–71), Leeds (2016b, 663–66), Feygin (2017, 255–58). The Soviet experiments with automated planning systems were not unique at the time: see Stafford Beer's *Cybersin* (Synco) project supported by Salvador Allende's government in Chile (Medina 2006; Morozov 2014).

92. Cf. Leijonhufvud's (1973) "econological" parody about the Math-econs, Micros, Macros, and Devlops. Compare with another typology of economists (mathematical versus verbal and reformist versus conservative) in the chapter on the Soviet Union.

93. The best documented stories have been told about the political humiliation of the Soviet optimal planners at TSEMI and other research institutes and university departments, particularly during the 1970s. For example, in the course of ideological cleansing following the occupation of Czechoslovakia in 1968, it was not only market socialism that featured among the accusations levelled against TSEMI but also the "too high" proportion of Jewish researchers in the institute (Birman 2001, 241–76; Katsenelinboigen 1980, 78–80; 2009; Leeds 2016a, 237, 340, 395; Sutela 1991, 83–94). In Prague the Institute of Economic Sciences at the Law Faculty of Charles University, which was regarded as a "nest of revisionists" was closed in the period of "normalization." Mathematical economists were not safe from recurrent attacks in more permissive communist regimes either. See the chapters on the Soviet Union and Hungary.

94. While mathematical economics and the theory of market reform did not merge, optimal planning did contribute to the development of the theory of marketization in some way. It prompted reformers to say goodbye to some of the fuzzy notions of the official discourse (e.g., commodity production, the interest of the people's economy, material incentives) and think in terms of well-defined economic actors who want to maximize some kind of utility but planning instructions and other state regulations force them to join the informal economy.

95. The refecons who normally came from the realm of official political economy and retained some loyalty to Marxism seldom returned there. On the contrary, owing to the successive radicalization of refecons, the road to teaching at universities was

blocked by the polecons for many of them until communism started imploding in the 1980s. A conversion between the optecons and the polecons was virtually impossible.

96. In private conversations the liberal-minded optimal planners used to combine the following self-justifications (which initially were similar to those of the market reformers): first, the regime will only change (if at all) in the long run; second, what we are doing can be seen as a gradual and peaceful destruction of the planned economy by injecting the poison of rationality in its body and eliminating the *raison d'être* of a large part of the planning apparatus; third, provided our suggestions are accepted by the rulers, the life of our fellow citizens will improve.

97. See the chapters on Czechoslovakia, Hungary, and Poland.

98. "Consoling" is meant here with a grain of salt. A key change, namely, the turn to econometric studies, presented in most chapters of this volume, could not really comfort those scholars who had been used to normative research with a direct impact on the economy, an academic position of high prestige embedded in the planning regime at its higher echelons, and a distinguished status in the international scientific community.

99. It is perplexing to see how many times the participants of the debate thought that it ended with their victory. Austrian theorists of different generations, Lange, even Koopmans belong to them. Koopmans (1951, 7), for example, praised George Dantzig, saying that his model "is an abstract allocation model that does not depend on the concept of a market" and as such it disproves Mises's impossibility thesis.

100. See note 57.

101. Among the eminent optimizers, it was Kornai who proved to be the most self-critical. For the limitations of his "repentance," see the chapter on Hungary.

102. I learned a lot in conversation with Péter Bodó about the role these disciplines can play in economic planning.

103. For anticipating some of these developments by Emmanuil Braverman, see Kirtchik (2019, 200). For a selection of the rapidly growing literature on whether AI can guarantee rational calculation under collectivism, see Cockshott and Cottrell (1989; 1993a,b), Laibman (2002), Jablonowski (2011), Morozov (2014; 2019), Phillips and Rozworski (2019), Feygin (2019), Van Den Hauwe (2019), Nieto and Mateo (2020), and Daum and Nuss (2021). On the possibility of bringing the labor theory of value back in economic calculation, see Cockhshott and Cottrell (1989). For a most recent critique of "cyber-communist" projects, see Wang, Espinosa, and Peña-Ramos (2021).

104. As mentioned, Bardhan and Roemer (1992; 1993) started groping in this direction right after the collapse of communism by resuscitating the doctrine of market socialism. Since then, the ideas of industrial democracy, cooperativism, participatory economics, and so on have continued to appear in different forms on the Left. (See, e.g., the project of "investment and consumer councils" in Nieto and Mateo (2020).) For earlier and later comments on concepts of decentralized socialism, including the murky experiment with "social planning" in Yugoslavia, see Prychitko (1988; 2002). Meanwhile, modern macroeconomics, with its varying families of models (ranging from "computable general equilibrium" through the "real business cycle" to "dynamic stochastic general equilibrium"), does not seem to attract the would-be planners like the Walras model did almost a century ago.

105. The system of grading them by means of a "social credit" system in China is a case in point. For an ambitious program of a democratic architecture of a "plan-oriented market economy system" controlled by artificial intelligence, see Wang and Li (2017).

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