Chapter 5

Mathematical Economics Outside the Neoclassical Paradigm?

Evolution of Planning Concepts in Hungary under Communism

Gergely Kőhegyi and János Mátyás Kovács

This chapter seeks to unravel the puzzle of the sluggish Westernization[1] of economic thought during the communist period. Why did neoclassical economics that Hungarian economists of Marxist persuasion started sampling at the end of the 1950s strike roots only after 1989? Hungary gave the world theorists such as János Harsányi, Miklós Káldor, and János Neumann, and was famous for having one of the least closed and repressed economic research communities and launching one of the most radical market reforms in the Eastern Bloc. In our country the first models describing the planned economy by means of mathematical (partly neoclassical) instruments already were built at the turn of the 1950s and 1960s, and many of those instruments were taught in regular courses at Karl Marx University of Economics in Budapest from the early 1960s. Simultaneously, a growing number of Hungarian researchers followed in the footsteps of promising young scholars such as András Bródy and János Kornai, who rapidly became renowned in the international arena of modern economics. Borrowing the label accepted in the West (Kőhegyi 2010), they named themselves mathematical economists (in contrast to the official designation of "political economists"), and established special departments not only at research institutes and universities but also in major government agencies such as the Central Statistical Office and the Planning Office.[2] Both a symbolic breakthrough and institution building were

greatly facilitated by similar achievements of mathematical economists in the Soviet Union.

After carefully gauging the political mood, Hungarian scholars with an interest in quantitative economic research published their own journal *Szigma,*[3] and engaged in a busy multilogue with their peers in both the West and the East. Transnational communication resulted in long research stays, guest professorships, joint research projects and publications, not to speak of prominent positions in international academic organizations like the Econometric Society and the International Input-Output Association.[4] Why did this segment of the research community remain a minority in Hungary for about 30 years; a minority that—no matter how strong it was in scholarly terms—proved unable (or did not want?) to orchestrate a belated but genuine "neoclassical revolution?" How could mathematical economics thrive so long while resisting the temptation to join the neoclassical mainstream in the West?

We contend that the answer to these questions is to be found not only in a competing enticement by market reforms that kept the majority of Hungarian economists within the realm of verbal ("old") institutionalism, but also in the hope of comprehending the operation of the planned economy by means of input-output (I-O) analysis and improving economic performance through optimal planning based on linear programming. Obviously, any explanation relying on that hope, though necessary, cannot be sufficient since it dwindled as years passed. The historian also must clarify why the research program of optimal planning worked as a trap, easy to enter but difficult to exit even when more than enough evidence had been gathered about the failures of the program.

As so often in communist history, it would be easy to blame (self-)censorship for ensnaring economists in the trap for decades, at the very least gluing them in place unable to reach out in a neoclassical direction, thereby delaying Westernization. Ostensibly, the rapid success[5] of neoclassical thought in the Hungarian economic research community after 1989 makes such an explanation more than plausible (cf. Kovács 2002, 2012). It might seem that a number of gifted economists already had joined the "revolutionary movement" in a clandestine manner under communism and could not wait to "come out." Undoubtedly, there were some dedicated neoclassical-minded theorists among younger researchers during the late 1980s, some of whom had an opportunity to familiarize themselves with then-mainstream economic thought at Western universities and return home with that knowledge.[6] They served as catalysts of a neoclassical awakening after 1989, while the majority of members of the older generations of mathematical economists who had put all their faith in optimal planning did not jump on the bandwagon of neoclassical triumph.

Were they scared for good by the censors in the 1950s and 1960s? One could hardly accept this assumption knowing that optimal planners experienced a weakening of political control in Hungary long before the collapse of communism; some even enjoyed special privileges as advisors to or employees of the party-state. True, one must not disregard the recurrent intimidation of mathematical economists and their partial exclusion from official political economy. Nonetheless, after a while, mathematical reasoning in economic sciences ceased to be forbidden fruit, with all the excitement of its consumption. In terms of the Kádárist trinity of cultural policy[7] pursued from the early 1960s, mathematical economics was not prohibited but tolerated and then openly supported by the authorities. To put it bluntly, Hungarian mathematical economists had a relatively easy time for decades in the trap of optimal planning. They were convinced that they had found not only a political and existential but also a scientific *modus vivendi* by tacitly abandoning Marxist-Leninist textbook political economy without joining the neoclassical mainstream. They trusted the authenticity and success of their own research program and considered it at least as valuable by scholarly standards as any similar program initiated in the West. Returning to the basic research question of this chapter, we would like to check the assumption that, following a brief phase of devotion, the majority of Hungarian mathematical economists *did not want* to turn into veritable neoclassical thinkers, and quite sincerely so, driven—as time passed—by scientific preferences rather than political fears.

The first mathematical economists in Hungary were ready to make concessions without scruples by camouflaging (a) the divergence of their models that invoked an ideally technocratic vision of communism from the real world of bureaucracy in a Soviet-type planned economy, and (b) the similarities between their theories and the neoclassical ones. As usual, self-censorship resulted in self-cheating once these scholars fell in love with their concepts of optimal planning and made a virtue from necessity. They convinced themselves that a neoclassical turn would not only backfire politically but also would be scientifically superfluous and even harmful. As if the grapes were sour, they resisted the intellectual appeal of neoclassical thought by picking and choosing some of its instruments but ignoring its underlying philosophy and methodology. Instead of recognizing neoclassical economics as a Grand Theory, they considered it as a collection of technical recipes, from which one chooses the principle of optimization without whispering a word of praise about price theory. Moreover, even a frontal attack like Kornai's *Anti-equilibrium* (1971) did not lead to irrevocable excommunication from the economic profession in the West. You safely could claim that the basic concepts of neoclassical economics (a) are unrealistic and reflect an ultra-liberal worldview replacing one extreme (state collectivism) with another (free-market individualism); (b) do not offer Eastern European economists an

opportunity to comprehend their own economies better than their homemade theories do; and (c) deter the economists from searching for a "healthy" convergence in terms of both economic systems and theories describing them. *Nota bene*, from the 1970s, neoclassical thought, particularly, the traditional interpretation of its central category, general equilibrium, began to struggle with issues of self-confidence[8] (Lucas 1976; Kydland and Prescott 1982), opening up new (among others, neo-Marxist) vistas of criticism even among mathematical economists in the West.

At any rate, can mathematical economics prosper outside (or on the edge) of the neoclassical paradigm? Considering the example of Hungary and a majority of communist countries, yes, it definitely can. At least, it could in the past, for a long time, up to a certain point, resulting in theoretical discoveries and a whole range of experimental applications in the field of optimal planning. However, while the analytical results were promising, the normative project of "plan improvement"[9] failed following a series of trials and errors. Moreover, this project trapped many of its advocates even after the fiasco. Was it the West, where the idea of general equilibrium slowly lost its popularity, that finally opened their eyes? We rather assume that Hungarian mathematical planners eventually grew disappointed with the idea of optimization, more precisely, the idea of optimization *in a Soviet-type planned economy*, which failed exactly because it was tested in vivo in that particular economic system.

In order to assess the above conjectures, we will first sketch out the ways, in which mathematics enriched planning concepts in Hungary. Then the institutional preconditions of evolution of those concepts will be discussed. Here, we will focus on the Institute of Economics, a renowned research center attached to the Hungarian Academy of Sciences, in which two leading mathematical economists of Eastern Europe, András Bródy and János Kornai, worked in close proximity for many decades.[10] They took different approaches to the theory of planning in the "triumphant" period of optimization of central plans but eventually agreed on staying outside the realm of neoclassical economics in many essential respects. The conclusion will sum up the reasons why Hungarian mathematical economists lost their trust in rationalizing planning and examine what other research programs they chose instead.

Our study had to cope with the lack of secondary literature published by historians of economic thought in Hungary and beyond.[11] We did our best to fill this gap by participant observation, numerous old and new interviews with our colleagues, memoirs, and archival sources. The reader is warned about possible biases since one of the authors (Kovács) was affiliated with the Institute of Economics for more than 30 years.[12]

Traditions, Institutions, Experts

After planting the seeds for mathematical research into central planning during the 1960s, economic sciences in Hungary seemed prepared to reap the first harvest by the 1970s. However, the first harvest also proved to be the last.[13] The idea of rationality to be found somewhere outside the spheres of textbook political economy and reform economics (market socialism) began to fade away slowly but steadily.

The 1960s were still an unmistakable success story although the previous 10 to 15 years had been anything but promising. Even if anti-Jewish legislation before the Second World War, the war itself, and—following a few years of relatively peaceful academic activity—the total Sovietization of social sciences had not nearly eradicated economic theory (and mathematical research) in Hungary through murder, emigration, imprisonment, occupational ban, and marginalization, the contingent of economists with mathematical skills would have been very small. Like other countries in Eastern Europe, two main strands of tradition dominated economic sciences in Hungary before the war: the German Historical School and—to a lesser extent—the Austrian School of Economics. Simply put, the former was open to the idea of major state intervention, and even state ownership; the latter considered the introduction of central plans and collective property as large steps along the "road to serfdom." This ideological difference notwithstanding, both schools normally excluded formal models from economic analysis. The only areas where quantitative reasoning found acceptance were in the systematization of empirical data and rudimentary economic dynamics. Between the two wars, the followers of the German Historical School in Hungary celebrated the idea of planning and advocated a *dirigiste* economic regime, a "bounded" or "managed" economy, as they called their ideal of corporative state capitalism. They formulated the planning procedures in verbal (let alone, elementary mathematical) terms, and proposed that the institutional framework of the central plan be patterned after the war economy as they knew it from the First World War.[14]

One does not know, of course, what would have happened to research into mathematical economics and its application to central planning in Hungary if scholars like János Neumann or Miklós Káldor had not left the country before the war.[15] Would they have survived and been permitted to work in the academia, particularly, in the field of planning doctrines? To take the example of game theory, could Neumann have launched his research program, teaching at a Budapest university from 1945 onward? Could Harsányi have developed the theory further during the 1960s if he had not left the country in 1948?[16] Similarly, would it have made a difference if the local forerunners of econometric research such as István Varga and Mátyás Matolcsy had not

been silenced and imprisoned, respectively, after the communist takeover?[17] To put counter-factual questions aside, what is well-known is the sad fact that Varga was the only one from an older generation of eminent scholars who made a comeback in economic research during Hungary's communist era. Varga became influential for a short period around 1956, and at that time he focused on market reforms instead of experimenting with mathematical planning.[18] Those few who kept the fire of mathematical economics warm from before 1945, such as the econometricians Ede Theiss[19] and Kálmán Kádas, were marginalized.

Communist (or social-democratic) economists, well-versed in neoclassical thought like Oskar Lange in Poland, lacking, the supply of mathematical methods in economic analysis emerged from other sources: a few Western textbooks and Soviet works, cooperation with local mathematicians (like Alfréd Rényi in the case of Bródy and Tamás Lipták in that of Kornai), and engineering education (Péter Erdős and Ferenc Jánossy). Many of the freshly-baked planning experts (such as Augusztinovics, Kornai, András Nagy and Márton Tardos) were self-made mathematicians.

Despite the unfortunate prerequisites to a solid development of mathematical economics, the seeds of the discipline slowly came to fruition. Research and education managed to profit from an ironic combination of two unrelated political factors in the second half of the 1950s: (a) the growing legitimacy of applying mathematical methods in economic research in the Soviet Union and (b) the impasse of reformist thought in Hungary due to the crushing of the 1956 revolution by the same Soviet Union. Let us now consider the domestic institutional and cultural preconditions of the turn toward the mathematics of planning.

Starting with scholarly publications, a growing number of foreign-language books and periodicals on mathematical economics became available in the libraries of the main institutes of economic research and Karl Marx University of Economics in Budapest from the late 1950s. The same applied to translated works. Edited collections of articles and book excerpts published in the West or popular guides to the new discipline such as Szakolczai (1963, 1967), Andorka, Martos, and Szakolczai (1967), Hoch (1968), and Andorka (1970) made the breakthrough. Translations of volumes written or edited by Soviet economists and mathematicians (e.g., Nemchinov 1962, 1966; Khachaturov 1966; Pontriagin et al. 1968; Petrakov 1970; Novozhilov 1971) were also helpful.[20] The books of leading Western authors followed suit. For instance, Jan Tinbergen's *Econometrics* came out in Hungarian in 1957, William Baumol's *Economic Theory and Operations Analysis* in 1968, Edmond Malinvaud's *Méthodes statistiques de l'économétrie* in 1974, and a truncated version of Paul Samuelson's *Economics* in 1976.[21] Meanwhile, also important works by Oskar Lange (1965, 1966, 1967a, 1967b), Michał Kalecki (1980, 1982),[22] and Wassily Leontief (1977, 1984) were published in translation. From the 1970s on, an avalanche of collections of papers written by other contemporary great theorists/Nobel laureates (such as Ragnar Frisch 1974, John Hicks 1978, Kenneth Arrow 1979, James Tobin 1984, Lawrence Klein 1986, Milton Friedman 1986, Gérard Debreu 1987, Miklós Káldor 1989) was launched by the *Közgazdasági és Jogi Könyvkiadó* (Economics and Law Publishing House). In some way, many prominent Hungarian mathematical economists and their disciples took part in translation and editing.

Numerous foreign authors spent some time in Budapest or met their Hungarian colleagues abroad.[23] Strong academic bonds emerged from these encounters (e.g., between Bródy and Leontief or Kornai and Arrow), not to speak of publications in excellent journals and publishing houses as well as prestigious collective volumes. Bródy's 1966 article in the *Quarterly Journal of Economics*, and Kornai's recurrent contributions to *Econometrica* (Kornai and Lipták 1962, 1965; Kornai and Martos 1973) set the bar very high.[24] The former published his books at North Holland and SAGE, the latter at North Holland and Oxford University Press.[25] Early on, they were invited to take part in edited volumes such as Bronfenbrenner (1969) in the case of Bródy; Malinvaud, and Bacharach (1967b) and Nove and Nuti (1972b) in the case of Kornai; and Bornstein (1975) in the case of Augusztinovics. The width of the stream of all these publications demonstrates not only the growing influence of Western (and, to a certain extent, Eastern[26]) scholarship on Hungarian economists but also the growing legitimacy of mathematical economics in the eyes of the authorities.

As will be shown, the domestic publications of Hungarian I-O scholars and optimal planners also started mushrooming in the 1960s and 1970s. The first English-language book on input-output analysis (Lukács et al. 1962)[27] was preceded or followed by a whole series of Hungarian-language works published, besides Bródy and Kornai, by Rudolf Andorka, Mária Augusztinovics, Péter Bod, Gusztáv Báger, Sándor Ganczer, Zoltán Kenessey, György Kondor, Béla Martos, Antal Máriás, András Nagy, Ferencné Nyitrai, Albert Rácz, András Simon, György Simon, András Simonovits, György Szakolczai, Márton Tardos, and others.[28] Later, when the trust in optimization diminished, scientific production did not decline but changed its face. The researchers diversified the models by including nonlinear and dynamic analysis or engaged in long-term planning. Both research strategies resulted in important English-language volumes (e.g., Martos 1975; Augusztinovics 1984). As regards scientific papers, in the beginning, the main periodical of the economic research community *Közgazdasági Szemle* (Economic Review) was reluctant to publish articles with a complex mathematical apparatus, but this attitude softened during the 1960s. With the publication of the journals *Szigma* and *Acta Oeconomica*,[29] mathematical economics slowly became

a standard discipline in Hungary by the 1970s. For example, Bródy's and Kornai's papers of mathematical relevance on intersectoral relations and optimal planning began to appear in *Közgazdasági Szemle* in the late 1950s; from then on, just about every important work by the two authors was published in both Hungarian and English.[30]

However, one genre of academic writing was forbidden to most leading research economists: the university textbook. With the exception of Bródy's (1962a, 1962b) textbooks on linear and stochastic programming and a brief chapter written by Kornai (1969) on mathematical methods of planning for a textbook published by Karl Marx University, the articles and books of eminent scholars in the field featured at most in the reading lists of certain courses (or among the informal recommendations by some teachers). Up until 1989, just two of the scholars listed previously was offered a regular professorial job at the University of Economics. In the best case, the others were allowed to hold a few lectures and smaller seminars (Bródy 1994, 328; Kornai 2007, 209–11).

Leaving the terrain of scientific publications and jumping back to the time of the communist takeover, planning theories (both verbal and mathematical) were developed in Hungary by and large under the aegis of four institutions: Karl Marx University of Economics, the National Planning Office, the Central Statistical Office, and the Institute of Economics at the Hungarian Academy of Sciences.[31]

Karl Marx University: Teaching Mathematics, Ignoring Economics

Initially, Karl Marx University of Economics (*Marx Károly Közgazdasági Egyetem*) in Budapest was the only institution of higher learning that trained economists in Hungary.[32] Over time, the textbooks of political economy incorporated thoughts about market reforms, shortages, investment cycles, and so on, but even the textbooks published during the 1980s failed to discuss mathematical concepts of planning or other quantitative models in detail.[33] Although from 1961 courses were held and textbooks written on calculus, linear algebra, probability and statistics as well as operations research, the university relegated the theory of planning to the Department of Planning the People's Economy. This unit was small and had low prestige; initially, it completely ignored modern economics and, by and large, its textbook was a summary of what was taught by the Department of Political Economy about real socialism. It hardly included any information on the functioning of real-life planning regimes.[34]

Until 1960, the role of mathematics at the university was restricted to a simple repetition of high-school level basics (Forgó and Komlósi 2015). Even György Péter, an actuarial analyst who became president of the Central Statistical Office, asserted in the 1950s that the four basic algebraic operations would be more than enough for an economist to know (Augusztinovics 2008, 1164). He served as head of the Statistics Department of the university from 1950. In contrast, Béla Krekó, a disciple of András Prékopa—"father" of operations research and probability theory in Hungary[35]—and assistant professor at the Mathematics Department, was committed to introduce the paradigm of optimization in the education of economists. He had futile discussions with the rectorate at the end of the 1950s. When he wanted to include game theory in the curriculum, one of its leading officials responded in an indignant style by saying, "Comrades, we have to preserve the university as a serious institution" (Forgó and Komlósi 2015, 3). Finally, Krekó was permitted to try out linear programming as an elective course with 20 to 30 students in 1959.

In 1961, he was allowed to invite the best 15 to 20 students in mathematics to take part in a new special program called *tervmatematika* (mathematics of planning). In this five-year program 60 percent of the courses were related to mathematics (calculus, linear algebra, cybernetics, mathematical programming, statistics, game theory, electrotechnics, and physics).[36] The program soon became popular, nurturing generations of mathematical economists. It launched a "deterministic" and a "stochastic" track. Although the program was also supervised by the Department of Planning, the planning courses were taught with hardly any mathematics. The term "neoclassical economics" popped up (if at all), followed by plain faultfinding comments, in lectures on the history of economic thought. The first textbook providing a general introduction into mathematical economics (including input-output analysis and a few neoclassical models) was not published until as late as 1989 (Zalai 1989).

Despite all efforts to the contrary, the quantitative methods courses remained theoretical because the university did not cooperate on a regular basis with either the Planning Office and the Statistical Office or the economic ministries. The courses were related neither to central planning nor to other important issues of macroeconomic research. Examples for optimization were rather taken from company life and referred to challenges such as which factors of production to purchase or how large an inventory to hold (Halpern 2020; Kőrösi 2020). The only textbook-like volume on models of long-term planning, written mostly by researchers at the Planning Office and translated into English and Russian (Augusztinovics 1979), was not taught at the university.

National Planning Office: Improving the Plan—Feeling Futile

The main institution responsible for the conceptualization and implementation of central plans was the National Planning Office (*Országos Tervhivatal*)

founded in 1947. One of its main tasks was to coordinate the planning activities of the various ministries before they started negotiating with firms in the respective branches and to aggregate the outcomes of negotiations thereafter. Central planning was dominated by a traditional (verbal) political economy approach with a minimum of mathematical modeling during the entire communist period despite the fact that many attempts were made, inside and outside the Office, to apply advanced scientific tools that outshone the so-called "material balance method" borrowed from the Soviet Union, which did not require any more skill than elementary mathematics.

"The Central Planning Office was an <oasis> in Hungarian public administration. . . . A very flexible institution, in which it was important from the very outset that employees must have something in their head," remembered Augusztinovics (2012) long after its demise in 1990. She attributed this flexibility to the fact that—although the Office was a Soviet-style establishment—it was brand-new in the 1940s, free from the legacy of Austro-Hungarian bureaucracy (Augusztinovics 2008, 1165). From 1966 onward, the "mathematics of planning" program of Karl Marx University provided the Planning Office with good-quality experts. Collaborative projects with the Institute of Economics (*Közgazdaságtudományi Intézet*), which were launched during the early 1960s, also contributed to the growth of mathematical knowledge in the Office. Its Computing Center was founded in 1968.[37]

As regards planning as a scientific discipline, the Institute of Planned Economy (*Tervgazdasági Intézet*) that had been established between 1963 and 1966 under the aegis of the Planning Office set up a department of mathematical modeling. Here, Augusztinovics was employed as a leading researcher from 1964 to 1968. Before and after, she worked on financial balances and macro-modeling in general in various leading positions at the Office. Zsuzsa Bekker, who focused on growth models, joined the Institute a little later. The majority of researchers there produced verbal studies of central planning.[38] Among them was a brilliant thinker, Ferenc Jánossy, who invented iconoclastic theories of calculating national income and modeling economic development by using old-school statistical apparatus (Jánossy 1963, 1966). He was one of few scholars who—despite mastering higher-level mathematics—refrained from using it to improve planning and did not call himself a mathematical economist.[39]

In spite of all attempts at quantification, mathematical models played a major role only in medium-term, two-level (later, multi-level) planning, an initiative of Kornai in the 1960s (see below), and later in long-term planning, Augusztinovics's favorite field of study. Both were eventually futile undertakings but enjoyed an esteemed reputation among researchers due to the involvement of the two respected scholars, the parallel research programs in the West, and the relative freedom of scientific imagination. An open-minded

scholarly approach to long-run economic processes remained exceptional in an organization whose everyday operation was based on a predominantly verbal (bookkeeping-style) planning of material balances for annual and five-year plans. In the beginning, the composition of such balances, including the final synthetic "chessboard balance" (intersectoral balance, *ÁKM* in Hungarian) describing the relationships among the main branches/sectors of the national economy, did not require advanced mathematical knowledge. However, the chessboard contained all the information necessary for embarking upon input-output analysis. Yet, despite the fact that, from the early 1960s, the chessboards were used as I-O tables and researchers in the Planning Office performed complex mathematical operations with them, the planning apparatus was bogged down in old Soviet habits of inter-and intra-departmental bargaining[40] when setting up the macro-plans and breaking them down, via various industry-level agencies (ministries, directorates, trusts, associations, and so on), to the level of individual firms. In this intricate—multi-level and multilateral—bargaining game mathematics played a subordinate role; quantitative procedures of some complexity were mostly referred to if they seemed useful for any of the actors in the game. The following is a telling story from the life of the Office:

> By the end of 1958, the *ex ante* national income . . . displayed a deficit of 13 billion Hungarian forints, an enormous amount at that time, some 10 percent of the national income . . . . (The expected price increase of material inputs was generally overestimated and the price index of outputs generally underestimated by Ministries and large firms.) The President of the Planning Office offered a prize: a bottle of French champagne for each recovered billion. Deficit-hunting went on in the Planning Office for several weeks without success.
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> As a final resort, the management reluctantly consented to the compilation and repricing of a rather large interindustry table, something that was unknown and alien to traditional planning practices. "The chessboard game" began. Cell by cell, representatives of emitting and absorbing sectors had to meet personally and negotiate. . . . Within one week, all 13 billions were found. . . . We drank the 13 bottles of champagne and many more. (Augusztinovics 1995, 272)

Yet, instead of the computing center, plans were fabricated in the shady rooms of the Office, in which clerks rather than technocrats were making deals to finalize the planning indicators.[41] In order to achieve a meaningful selection of material balances, they had to solve numerous problems of measurement, commensurability, prioritization, and so on—problems all permeated by the conflicting interests of winners and losers, be they branch ministries, regional bodies, or ordinary firms. Moreover, these conflicts were mediated by a complicated network of party and state organizations including

non-economic institutions like the army. The outcome of bargaining processes overrode any results of optimal planning models during the crafting stage of the central planning instructions that were turned into law. Provided they had not overridden them, the same would have happened in the phase of implementing the instructions, leading to an endless chain of retroactive revisions of the planning figures (and amendments to the law). True, after a while, the I-O models could be used to validate the changes made at the negotiating tables, either before the plan was approved or thereafter, much more rapidly than earlier. Originally, the clerks were running from room to room in the Planning Office with pencil and eraser in their hands in order to replace a figure in the material balance of a particular product after their boss had taken a phone call from an influential party politician or state bureaucrat.[42] Augusztinovics lamented in retrospect: the mathematical models "remained a façade all the time, they were in the best case thought-provoking but did not ever become instruments of real decision-making. The real decisions emerged from bargaining" (Augusztinovics 2000, 12–13).

Under such circumstances, one could not effectively test the applicability of the input-output and optimal planning models,[43] even if the Statistical Office delivered more accurate data as the years went by (see below) and the planners' toolbox expanded in step to include advanced mathematical methods. Whether or not these models could have proven solid instruments of planning at all was never determined. Mathematical economists did not have a choice other than refining them in the hope of being perhaps listened to by the planning officials in the foreseeable future (cf. Ganczer 1973; Simon 1970, 1973; Szepesi and Székely 1974). Since five-year planning continued until 1989, the models did not cease to emerge in the Planning Office during the 1980s, even after many mathematical economists had lost their faith in optimal planning. Quantification was, in the best case, suitable for underpinning a superficial check on the realism of plans produced by verbal techniques. While in this respect their authors exerted some disciplining influence, they were virtually powerless in affecting normative decisions.[44] Mathematical planners in the Office encountered serious difficulties, for example, in identifying the objective function, according to which the models should have been optimized. Beyond lamenting the lack of "clean" data and arbitrary changes in the plans due to petty bargaining, this could have been the point where optimal planners clashed with their principals the most vigorously.[45]

However, instead of insisting on new priorities in economic policy (*horribile dictu*, radically increasing living standards and slowing down economic growth, or cutting military spending and trade with the Soviet Union), they normally accepted most of the objectives defined by the ruling elite. Because of firm political taboos, mathematical economists did not think of resisting the will of the *nomenklatura* publicly. They put up with pointing out

inconsistencies in the balances, smuggling a few new priorities into the plans, juggling with multiple draft plans, or playing mathematical tricks, mentioned by Augusztinovics above, which could modify the outcome of plan bargaining.[46] To the luck of optimal planners, by the mid-1980s, the top leaders of the Office and their advisors hardly could be distinguished from those of the Finance Ministry,[47] a stronghold of reform-minded economic policy and a think tank of late-communist transformation. In retrospect, the Planning Office seems to have been ready to engage in indicative planning, in which mathematical economists could have found ample space for themselves to experiment with Tinbergenian solutions. However, communism collapsed and the Office was closed, leaving behind a large gap in macro-coordination.

Central Statistical Office: From Chessboard to Econometrics

Hungary's tradition of statistical work on government level and higher education programs was informed by the German Historical School that laid the foundations for statistical research. The Central Statistical Office (*Központi Statisztikai Hivatal*) established in 1867 served as its strong institutional basis even after the communist takeover. Nevertheless, the Office was reorganized by a team led by György Péter, who worked as its president from 1948 to 1968.[48] In his view, a main task of the institution was to supply the Central Planning Office with reliable economic information. In the beginning, he had despised statistics as a discipline of calculating percentages (Köves 2005, 879) but later grew familiar with input-output analysis. While dutifully Sovietizing the statistical regime of the country, Péter developed a comprehensive observation system to measure the performance of state-owned firms. The first—experimental—version of the intersectoral balance was completed by the Office in 1957. In collaboration with the Planning Office, they accomplished a proper decomposition of the productive sectors in 1957 to create the first input-output table for Hungary by 1959 (Kenessey 1959).

In 1963, a special department was established within the Statistical Office to develop the economic applications of mathematical-statistical methods. Two years later, an econometric laboratory and a larger information processing laboratory (later, *Infelor*) was also set up.[49] While *Infelor* slowly became a quasi-independent company (Lampl 1971), the Econometric Laboratory remained within the Statistical Office. The members of the Laboratory (such as László Halabuk, Katalin Hulyák, László Hunyadi, Zoltán Kenessey, Judit Neményi, János Paizs, and György Szakolczai), were well-trained researchers in mathematics and statistics who started teaching one another modern econometric methods. They were driven by the urge to understand time series as well as linear and nonlinear regression analysis and other

contemporary econometric techniques.[50] The early econometricians of the Office had to overcome the resistance of traditional German-style descriptive statistics reinforced by its Soviet version. In the 1950s and 1960s, official political economy rejected any stochastic approach to central planning, assuming "objective" certainty instead of probability in portraying economic processes. Unsurprisingly, the most educated—more importantly, neoclassical-minded—expert of econometrics in Hungary, Ede Theiss, had only an advisory affiliation with the Statistical Office.[51] Nonetheless, he was instrumental in launching the first experimental econometric macro-model of the Hungarian economy, M-1 (Theiss 1965; Halabuk, Kenessey, and Theiss 1965). The multidirectional causalities among the sectors had been captured with the help of a simultaneous system of stochastic equations. This method was in vogue in the West at the time, and the project including the estimations, forecasts, and simulations was successful enough. The next model, M-2, exerted influence on models in other communist countries; M-3 was a joint Czechoslovak-Hungarian initiative; and the authors of M-4 made an attempt at integrating econometrics and input-output analysis by incorporating an interrelated, deterministic, and stochastic input-output block in the model and representing the effects of non-material production closer to the SNA technique[52] than earlier (Halabuk 1971, 1976; Hulyák 1972; Hunyadi 2012). In 1982, some members of the Laboratory moved to the Institute of Economics. Here, they did not initiate collaborative projects with those researchers of the Institute who had already begun to run econometric programs themselves (Halpern 2020).

While economic theorists always complained that the Statistical Office delivered neither sufficient nor accurate information, the level of precision of the data increased remarkably in the communist era. Obviously, political biases, ranging from military secrets to artificial prices, continued to deform statistical information, and the lowest-level economic actors were astute enough to start plan bargaining already during the data provision phase. The planning bureaucrats would have magnified these errors and falsifications to their extreme if I-O analysts, optimal planners, and econometricians had not succeeded in confining distortion through their models time and again.

Institute of Economics: Making Mathematics Legitimate in Political Economy

The fourth institution that made a lasting contribution to developing planning concepts and methods in Hungary was the Institute of Economics at the Hungarian Academy of Sciences. In terms of original discoveries that might match similar results in mathematical economics in the West and the East, it proved the most productive in input-output analysis and optimal

planning. Scientific innovation stood in strong correlation with the privileges the Institute's researchers enjoyed in accessing literature, choosing projects, fostering international relations, and publishing.

In the wake of Imre Nagy's "New Course," the Institute was established in 1954 with the aim of "laying the scientific foundations of economic policy." It published *Közgazdasági Szemle*, the main scientific monthly of the discipline to the present day in Hungary.[53] Founding director István Friss was appointed by the conservative faction of the Central Committee to counterbalance Nagy's reform program. However, a majority of affiliated researchers identified themselves with that program since they had been selected by Friss according to their scholarly talent rather than political loyalty.[54]

Even those among them who had some prior knowledge of mathematics refrained from applying quantitative research techniques at the very beginning.[55] They put faith in the possibility of restarting market reforms after the 1956 revolution, at least until the so-called Varga Commission that had suggested a further liberalization of planning was disbanded by the government in 1957. It was only during the later years of the first—militant—phase of Kádárist "consolidation" that several members of the younger generation, many of whom burned their fingers in 1956, felt persuaded to withdraw to a safer space within academia and use mathematics as a jargon of dissent.[56]

Amidst the post-revolutionary hangover, a number of frustrated market reformers were looking for a refuge where they could tide over hard times and from where they could emerge well-equipped with sound techniques of economic measurement, analysis, and prediction. They felt uneducated and inaccurate, and decided to overcome forced parochialism. Eagerly catching up with then-mainstream theories in the West, they wanted neither to fully renounce their Marxist convictions nor to exclude the possibility of rejoining reform programs at a future point. They hoped that—provided they could reassure their main adversaries about the political innocence of mathematical methods—the scientific language might protect them for the simple reason that it was impenetrable to the censors.[57] They did not anticipate, however, that such a discursive refuge could turn into a trap in the long run.

This strategy of self-camouflage did not prove entirely successful. Although Bródy's proud Marxist/collectivist stance as well as Kornai's sharp attack on general equilibrium theory may have demonstrated a fair degree of ideological obedience, suspicion toward mathematical economics burst out repeatedly. It was fueled by some leading scholars of the Institute, including deputy director Tamás Nagy, an influential reform economist and dedicated Marxist, even as late as the end of the 1970s.[58] Nevertheless, in the shadow of the Institute's persistent commitment to market reforms, mathematical-economic research programs continued to remain a tolerated (or provisionally supported) albeit secondary feature of the place. Prior to the introduction of

the New Economic Mechanism (NEM) in 1968, the Institute of Economics served as a major pool of ideas on market reform and—under the directorship of the father of NEM, Rezső Nyers, from 1974 onward—became an academic stronghold levelling criticism at the counter-reform measures taken by the party-state after 1972/73. Mathematical knowledge did not count for much in this rearguard battle.

At the turn of the 1960s and 1970s, Bródy and Kornai were permitted to organize small research groups that attracted gifted young economists and mathematicians to the Institute. As mentioned, neither of them nor their close associates were allowed to teach regularly at Karl Marx University. Thus, they were not urged to build up a systematic body of knowledge in mathematical economics (Simonovits 2019). Yet, they affected many students of the university's "mathematics of planning" program through their works and numerous formal and informal discussions held at the Institute and even at the university.[59] The bulk of research into mathematical methods of planning in Hungary revolved around the Institute in concentric circles. For example, from the early 1960s onward, the Institute worked together with the Central Statistical Office and the computing center of the Planning Office (and later with its research institute) with hardly any friction. To an extent, cooperation was based on personal relationships[60] without aggressive political control. Astonishingly, the breakthrough of mathematical economics during the 1960s proved irreversible. In 1964, István Friss solemnly stressed that "if one could dispute the application of mathematics in economic science for a long time, there is no room for such doubts after the [positive] experiences during the past years" (Augusztinovics 1964, 65).[61] Apparently, this declaration was not just caused by internal lobbying by mathematical economists in the Institute but also by the influence of their Soviet colleagues, which resulted in mutual research visits and the publication of Nemchinov's path-breaking edited volume in Hungarian in 1962.[62] The process of legitimization seemed to end with an invitation, sent to Kornai who—accused of revisionism—had been fired in 1958, to rejoin the Institute in 1967. (The decision was made by Friss in both cases.)

As the previous sections suggest, there was a fairly cohesive group of dozens of scholars cultivating mathematical techniques of economic research in the partner institutions of the Institute of Economics.[63] Within the latter, two generations combined forces before 1989.[64] This was a small and stable research community, with two international stars surrounded by their associates who were barely threatened by external professional competition and enjoyed considerable freedom of thought within their research groups. However, ultimately they had to adjust to the mix of family atmosphere and quasi-feudal hierarchy prevailing in the Institute.[65]

During the 1960s and early 1970s, the majority of older researchers in mathematical economics focused on the theory of central planning in some sense. Professional solidarity among them was relatively strong for many reasons, ranging from the scientific vernacular they spoke to being occasional victims of harassment. The same applies to Bródy and Kornai who—irrespective of a growing divergence between their research programs and political attitudes—did not air their dirty linen in public.[66] The early research projects of the Institute in mathematical economics focused on input-output models (Ausch, Bródy) and optimal planning/programming (Kornai and Martos, András Nagy). Kondor and Simon studied both fields. According to Virág (1973), Simonovits (1996), and Csató (2019), the principal research fields covered by both generations in the Institute at the turn of the 1960s and 1970s were as follows: closed and open, static and dynamic input-output models, and the Neumann model (Bródy, Halpern), "searchlight programming" (Simon) as a decomposition procedure, nonlinear programming (Martos), equilibrium theory (Kornai), team theory (Simonovits) "vegetative" (non-price) control (Kornai, Martos, Simonovits, and Virág), queueing theory (Simonovits), planners' behavior (Lackó), decision theory (Tényi), growth models (Virág, Horváth, and Rimler), planning labor market and vocational training (Bondár, Horváth, and Tényi), consumption theory (Hoch, Ilona Kovács, Ördög, and Radnóti), and macroeconomic modeling (Kondor, Simon, and Gábor).

Interestingly, the most powerful academic initiative to rationalize medium-term central planning based on the idea of two-level planning came from outside the Institute of Economics in the course of the 1960s. Its pillars rested on a nearly decade-long cooperation of multiple state agencies and research institutes and embraced dozens of researchers under the guidance of Kornai, then formally still an outcast (Kornai 1965).

Ironically, mathematical economics became largely uncontested within the Institute only *after* Kornai's (1965) and Bródy's (1970) seminal works on optimal planning and input-output analysis, respectively, had been completed and the attraction of these research programs started petering out. At first sight, this cries for a political explanation, for it might seem as if mathematical methods were tolerated or even promoted once a growing number of researchers had abandoned applying them as means for intervening in the "high politics" of central planning. Accordingly, from that time on, they were free to build quantitative models of shortages, the labor market, shadow economy, and economic fluctuations, or even to indulge in the intricacies of economic control, just to name a few successful research projects, provided they did not challenge the institutional and ideological core of the five-year plans. Moreover, the model builders were permitted to use any mathematical techniques they thought opportune. Yet, in terms of methodology, some of the new models were more rigorously neoclassical than those of optimal

planning, and the results of many of them were more explosive politically (see Postscript).

Undoubtedly, these models grew less normative and more descriptive and analytical in nature. However, with normativity their "meliorist" attitudes (cf. perfecting the planned economy) faded away and slowly were replaced by a cool-headed, impartial approach colored by a kind of "inverse normativity" pointing toward capitalism. Quite a few economists at the Institute were equipped to transition to neoclassical scholarly culture by the mid-1980s, at least as far as their mathematical expertise was concerned, and this had little to do with self-restraint in matters of high-ranking party and state affairs. Just the opposite happened: by then official political economy and its guardians in the higher echelons of the party-state became too weak to resist the proliferation of critical economic thought underpinned by an ever deeper mathematical knowledge. Nevertheless, this deepening never would have taken place without the groundbreaking contribution of the first cohort of input-output specialists and linear programmers.

At the same time, the members of the older generation—while pulling their disciples into mathematical economics as well as nurturing and safeguarding them—did not push them out from the "refuge," prompting them to convert to neoclassical economics. What is more, during the 1970s, they continued to refine I-O analysis and planning models, in harmony with close colleagues outside the Institute (e.g., Augusztinovics 1979).[67] True, their attention switched from five-year plans to planning economic processes in the long run (see below). It was only Márton Tardos (who joined the Institute in 1980) and András Nagy (who rejoined it in 1973) among the older scholars who acquainted some of the younger researchers with standard neoclassical thought—ironically, through its critique offered by new institutional economics.

This schematic story of the evolution of quantitative methods in economic research cultivated in the Institute of Economics would not stand the test of reality if, next to the textbook political economists and the mathematical economists, a third group of actors, the reform economists, were ignored. For example, the weakening of the party-state's resilience to criticism mentioned above was due, to a large extent, to the radicalization of reformist thought. Moderate or radical, the market reformers were similar to the textbook political economists (a rare species among the members of the Institute by the way[68]) in doing predominantly verbal research while reminding the observer of the mathematical economists when rejecting the sub-scientific discourse of the official textbooks. The reformers raised serious doubts upon state planning and contributed to its ideological disenchantment, which was received by many mathematical economists with mixed feelings. The latter also disapproved of the bureaucratization of planning and plan bargaining, namely,

the distortion of scientific planning procedures by lobbies within the *nomenklatura*. However, they were afraid that the devaluation of central planning would eventually result in an overvaluation of the market and a decline in the quality of macro-management. Despite such disagreements, both groups shared the ideal of independent thinking, disliked parochialism,[69] cherished the memory of the 1956 revolution, and so on, that is, common attitudes sustaining solid bonds between their members. Furthermore, over the years, it was increasingly difficult to find a mathematical economist in the Institute who did not agree with the reformers on a considerable degree of marketization or even join verbal institutional research programs on that issue. To be sure, it was much easier for them to do so than for reform economists trying to learn how to build formal models.[70]

TWO PIONEERS IN ONE HOUSE: COMMON START, PEACEFUL RIVALRY, BIFURCATION

In terms of methodology András Bródy (1924–2010) and János Kornai (1928–2021) had chosen different points of departure for doing economic research on the planned economy. In the mid-1950s, the former opted for quantitative modeling while the latter chose verbal, quasi-sociological research. Later they took parallel roads leading to then-mainstream economics in the West. If space allowed we could write pages on the similarity of their social roots as well as political and cultural motivations—rich families, *Bildungsbürgertum*, cosmopolitan attitudes, Holocaust survival, joining the communist party and fascination with Marxism, the trauma of 1956, respect for scientific knowledge, a spirit of rebellion, and so forth—that would explain why the two young, self-educated intellectuals turned to Western economic theories. As mentioned, they helped (but also competed with) each other on their unfinished trip to neoclassical theory until they drifted apart. The causes of bifurcation of their research programs also would require a space dedicated to major differences in scholarly styles, attraction to other social/natural sciences, mathematical skills, demand for their works in scientific markets, political attitudes, and so on.

Bródy had introduced Kornai to input-output analysis whereas Kornai became more erudite in optimal planning than his friend and colleague. As Kornai (2018, 6) remembered, "in terms of methodology, Bródy (and many more Marxists, for example, Mária Augusztinovics) and I, who was not a Marxist but a fan of neoclassical theory in this phase of my life, were allies. . . . We wanted to use mathematical methods, which forged a sort of alliance between us, I would say, complicity in the sense of understanding each other." Kornai imported Western-type research techniques, broke with Robinson

Crusoe-like routines of scientific organization and set up research teams whose members were assigned special tasks including literature reviews, case studies, model building, and testing, with particular attention to publication. While he benefited from a set of managerial skills, in addition to an ability to reinterpret and systematize ideas, Bródy was a lonely rider and a daring dreamer. "A majority of researchers in the Institute profited from or simply worked on projects developed from his flashes of inspiration" (Molnár 2019). Kornai carefully nourished many of his discoveries in comparison to Bródy who was not keen to flesh out his original insights in detail.[71] The role of the *enfant terrible* was always closer to his heart than that of the well-disciplined, widely respected researcher. Their younger colleagues had a chance to choose from these two scholarly attitudes or combine them freely.

The two charismatic scholars held sway over the research programs of the Institute of Economics in mathematical economics for a long period. In the beginning, Bródy's preoccupation with I-O models and Kornai's concentration on optimization complemented each other. Ironically, in working together on various projects, Bródy the Marxist grew less skeptical about neoclassical virtues than Kornai who had initially underpinned his studies of mathematical planning with neoclassical principles. Later, Bródy moved to the study of dynamic processes with a special interest in economic cycles and their mathematical complexities whereas Kornai, following a desperate struggle with general equilibrium theory, immersed himself in the scrutiny of disequilibrium with a renewed curiosity in institutional analysis. Meanwhile, problems of economic control, particularly whether it can lead to balanced growth, intrigued both of them immensely. The concept of equilibrium did not lose its appeal to them entirely even if they revisited it with growing suspicion. Bródy's (1994, 317) following words underline why their programs nonetheless diverged:

> Equilibrium is a very nice concept, without it one cannot do disequilibrium economics either. However, one also cannot create a theory that would guarantee, either via the market or the plan, that the equilibrium materializes. Moreover, and this applies to Kornai's works after *Anti-equilibrium*, my objection was that he wants to control the economy to adjust to an equilibrium that is again determined from outside.

András Bródy: From the End-of-Month Rush to the Kondratiev Cycles

Bródy's first inspiration to study economics came from Marxian political economy as he wanted to find an adequate mathematical structure for the reproduction schemes in *Capital*.[72] In the mid-1950s, he and his co-author Alfréd Rényi were unfamiliar with both Leontief's and Neumann's writings (Bródy 1994, 298). In examining centrally-managed price adjustment, they contended that prices with a given rate of profit are generated in an iterative process of circular adjustment where current prices emerge from the distortion of the unit cost in the previous period. Bródy and Rényi (1956) specified the conditions of convergence of this process. Later Bródy recognized that they accidentally had rediscovered the infinite series solution of the Leontief model—remarkably on the dual side.

In his early works Bródy also investigated the fluctuation of production in state-owned firms. Analyzing statistics of energy consumption by elementary tools of mathematics, he discovered that labor intensity sharply increased at the end of each month (*hóvégi hajrá*) (Bródy 1956). According to the key finding of this article, the cyclical characteristics of the production process were due to the periodic accounting of the fulfillment of planning targets, which was required by the branch ministries.

He also showed interest in the intersectoral foundations and computational methods of economic planning. Bródy's publications (1957, 1958, 1960a) were expository papers on input-output analysis, in which he demonstrated that the margin of error in the results of the I-O models is smaller than in the original data. Besides the ability of those models to display circular flows and cumulative effects in the economy as a whole, this was his main argument for their application, claiming that they provide robust conclusions concerning production structures, prices, and growth rates.[73] In addition to theoretical research, he participated in the computation of the first Hungarian SAM in the Central Statistical Office. At that time, Bródy (1960b, 954–55) protected his own model-building activity from excommunication by describing mathematical economics as "vulgar political economy," and accusing econometricians in the West (and the Hungarian Kálmán Kádas) of relying on the notion of "bourgeois rationality." He claimed that models cannot be borrowed from the West unless "one eradicates the last germs of bourgeois economics from them" and was embarrassed to read that Leontief's work had been said to be of "negative social value" in the United States because it helped manage a "totalitarian state." In retrospect, he portrayed his anti-Western attitudes as a blend of faith and opportunism (Bródy 1994, 348).

In 1961, Bródy defended his doctoral dissertation that summed up his knowledge of input-output analysis at the time, and this was his first attempt to clarify the Marxian background of I-O schemes. He proved the unicity of production prices and the rate of profit (Bródy 1962c). Later, he said that he recognized that this evidence was only a special case of Neumann's proof of the existence of general equilibrium (Bródy 1994, 314) although Neumann dealt with existence instead of unicity. In 1964, he continued working on the

application of input-output models in Leontief's research group at Harvard where he cooperated with Anne Carter. In 1969, Bródy published a book with the title *Érték és újratermelés* (Value and Reproduction) that grew to be popular off the mainstream in the West. He regarded it as his *magnum opus* and had it translated into English under the title *Proportion, Prices and Planning: A Mathematical Restatement of the Labor Theory of Value* (Bródy 1970). The book departed from a closed, static, and deterministic model that drew from Lange, Leontief, and Neumann, and reinterpreted the turnpike theorem of equilibrium growth.[74] According to the author, the model reflected the duality of the Marxian concepts of use value and exchange value, could be directly applied to data, was computable, and was suitable for building consistent economic plans. Nevertheless, in his view, consistency was not tantamount to optimality:

> The model does not take decisions according to a given criterion of optimality, it does not automatize planning. It only makes for us possible to assess and compare relatively fast and simply some of the important consequences of decisions reflecting different economic policy considerations. (Bródy 1969, 12)[75]

> I did not believe in the Good Plan, but definitely trusted that the plan and the economy can be improved through model calculations. (Bródy 1994, 316)

Without attacking the theorists of optimal planning (including Kornai) head-on, Bródy cast doubts on the theory of optimal processes by pointing to (a) the vast number of constraints and control variables to be included in the I-O model if dynamized, which lead to difficulties in obtaining precise data and finding correct mathematical formulations, and (b) the possibility of sacrificing longer-term equilibrium for shorter-term optimization. More importantly, he alluded to the fact that the optimal planner is, in fact, not familiar with two things "only": the system to be controlled and the objectives, according to which it ought to be controlled.[76] However, rather than challenging directly the right of the party-state to determine the economic policy priorities (objective functions) of the central plan in Hungary (Bródy 1970, 147–53), he nailed down his own priorities, including a radical slowdown of economic growth, development of human capital, and avoiding overinvestment in fixed assets—suggestions identical to those of his friend Ferenc Jánossy.[77] Both of them thought that the market reformers of 1968 attributed too much importance to institutional change instead of calling for a balanced economic policy.[78] "If one wants to maximize something very much, what is one of the troubles with the planned economy, one will succeed in the beginning but fail in the end, even in fields where maximization was sought the most" (Bródy 1994, 325).

While continuing to refine his I-O models for decades (e.g., Bródy and Carter 1970a, 1970b; Bródy 1978, 1981, 1995, 2004a),[79] the problems of economic growth and development began to dominate Bródy's mind. A formative experience in studying economic dynamics was his encounter with Evsey Domar at MIT and Richard Stone in Cambridge in the mid-1960s. Upon return to Budapest, he aimed to clarify both the statics and the dynamics of economic systems, the latter with and without technological change (Bródy 1994, 315). This research endeavor gave an impetus for writing three books, including *Proportion, Prices and Planning*. Back in 1965, he had invented a simple model for economic growth. Departing from a closed dynamic Leontief model, he pointed out that the crucial factor restricting growth is human capital, in the Marxian sense of "production of workers" (Bródy 1966, 137). In Bródy's life the 1970s were devoted to resolute attempts to comprehend economic cycles. In 1980, he published a book entitled *Ciklus és szabályozás* (Cycles and Control) with the purpose of building a mathematical model of markets and cycles as suggested by classical authors such as Smith, Ricardo, Walras, and—obviously—Marx. He intended to derive the dynamic process of price formation from their texts (Bródy 1980, 44) and came to the conclusion that prices do not converge toward equilibrium but show a cyclical variation around it, which is analogous to the motion of a pendulum or a planet. In his model, product prices and quantities regulate each other (he calls this cross-control).[80]

Bródy (1980, 139) asserted that, according to the standard Marxist view of economic cycles, they were caused by the capitalist market even though cycles had existed before capitalism (see, e.g., the parable of seven years of great plenty and seven years of famine) and emerged also thereafter, in the planned economies. He searched for short and long cycles not only in economic and demographic time series like Kondratiev (Bródy 1997a, 1997b, 1999a, 1999b) but also in biological ones such as the pig cycle (Bródy 1994, 340). Bródy was interested in Goodwin's predator-prey model as well (Bródy and Farkas 1987). The theory of cycles served as a foundation for his explanation of economic crises in the world and errors in Hungarian economic policy. He wrote many articles in newspapers about this topic to a wider audience and published a popular book *Lassuló idő* (*Slowdown*) in 1985, which anticipated a global stagnation and many other economic maladies.[81]

As suggested above, Bródy (1994, 318) did not cease to believe in the labor theory of value but lost his faith in planning early on.[82] Instead of central planning, he envisioned a kind of economic self-regulation similar to that of physical and biological systems (cf. Kornai's concept of "vegetative control"). Thus, he was not really affected by the arguments of any of the conflicting parties in the Socialist Calculation Debate.[83] Indeed, he tried to integrate, to use his terminology, the "deterministic-causal" models of labor

theory of value with the "teleological-optimizing" models of marginalism in the same mathematical framework and argued that these models bring identical results if the same data are fed into them (Bródy 1970, 50, 165). Seen as a follower of Wassily Leontief and Oskar Lange, heir of János Neumann, a mathematical interpreter of Karl Marx's theories, an adherent to Piero Sraffa and the Ricardian legacy, and one of the rediscoverers, with Michio Morishima, of the turnpike theorem, Bródy has been labeled a radical (heterodox) political economist of the 1968 generation until today, a neo-Marxist thinker who did not shy away from a critical dialogue with the neoclassical paradigm (Simonovits and Steenge 1997).[84] As Leontief put it politely,

> András Bródy's scientific contributions are marked by a creative, to some extent, dialectical combination of Eastern and Western streams of economic thought. On the one hand, it is rooted in the honorable tradition of classical economics interpreted by Karl Marx but carried forward by a sophisticated use of the analytical tools forged by modern neoclassical, mathematical economics. (Leontief 1997, VII)[85]

János Kornai: From Overcentralization to Shortage

Besides the impossibility of running reform-oriented empirical research projects in Hungary after 1956, Kornai's motivation to use mathematical methods stemmed from a real-world problem of central planning discussed in *Overcentralization,*[86] namely, the disincentives of firms to fulfill the plan. Following his dismissal from the Institute in 1958, he continued to examine the planning process in industry and began to tackle the issue of incentives by means of optimization.[87] He started sympathizing with neoclassical ideas of the time,[88] and in order to catch up with the state of the art, he relied on the support of the mathematical genius Tamás Lipták.[89] Their incentive-compatible optimization model generated complicated nonlinear programming problems whose solvability was not trivial. Although Lipták was arrested in 1957, Kornai managed to publish their research results (Kornai and Lipták 1959) with the support of the Ministry of Light Industry. When Lipták was released from prison, they summarized their findings in an English-language paper and submitted it to *Econometrica* for publication (Kornai and Lipták 1962). Its co-editor Edmond Malinvaud[90] proposed to accept the paper in an unchanged form. While the programming model dealt with the delicate issue of profit distribution, its authors exercised significant self-restraint. According to them, it is the state that performs the tasks of optimization on both upper and lower levels; the model does not tell whether the sum or the ratio of profit is to be maximized (if at all); it indicates the

impact of choosing between these options on price policy but refrains from suggesting any solution.

Parallel to theoretical research, Kornai initiated an applied project of linear programming in industrial planning. First, he organized a large group of experts to model choices among different technologies in the cotton industry. They investigated the effects of major exogenous variables such as interest and exchange rates as well as export and import prices on the outcomes of the model. The project resulted in a competition within the group between linear programmers and input-output analysts. The latter, led by Bródy and later by Augusztinovics, already had collected experience in this field, but Kornai (2007, 140–42) insisted on assuming the endogeneity of technological change and the flexibility of the volume and structure of output, that is, properties excluded by I-O models with fixed technological coefficients and predetermined final consumption.

He extended this approach to the whole economy by decomposing the principal planning problem into linear programming subproblems and introducing an authentic algorithm to find and connect their optimal solutions. Yet, the habitual practice of the Planning Office was fundamentally different. True, the Office also planned macro-indices and decomposed them first into sectoral/branch indices, then into firm-level ones. However, as mentioned earlier, many (sometimes most) of these figures did not emerge from mathematical models but from a foggy web of pressure group interests and were modified in several rounds of multilateral bargaining, both horizontally and vertically. In reallocating resources, the Planning Office did not follow fixed rules of the game and mixed the principles of economic and political rationality.

A mathematical model for iterations like these, called by Kornai "two-level planning," again was built by Lipták.[91] He portrayed the bargaining segment of the linear programming problem in a game-theoretical framework as a polyhedral game. This was a surprisingly innovative idea because the game paradigm was hardly ever used by mathematical economists in the West in the early 1960s. The paper was first published in Hungarian in 1962, then in *Econometrica* in 1965. It became one of Kornai's most influential (and perhaps "most neoclassical") works. A reason for the success was the similarity of this two-level model with Lange's dual scheme of market socialism as reformulated by Malinvaud (1967).[92] Although the Kornai and Lipták paper did not refer to the debate between Hayek and Lange, not even to Lange's contributions to that debate, it also revolved around the question whether or not the central planner has perfect information. As is well known, the omniscience of the planning authority remained an axiom even in post-Stalinist official political economy for a long time. The authors touched on this taboo by postulating a so-called "overall central information problem" to be solved by the programmer. Another insult to the ruling ideology was the description

of the planning process as a game (albeit, not a bargaining game), in which the center and the sectors have different strategies (i.e., different interests) that have to be coordinated. To reduce political risk, the paper reassured the reader that two-level planning only mirrored the actual dialogue between the center and the sectors: "the method proposed here is an attempt to *aid* this process of planning and counter-planning by means of *objective* criteria" (Kornai and Lipták 1965, 143, our emphasis). The authors stressed that the results of the two-level procedures could be useful in checking the consistency of the plan but abstained from interfering with the economic policy of the state through specifying the objective function of the model. In retrospect, Kornai (2007, 145–46, 181–83) claimed that they had managed to build an abstract (though unfeasible) model of perfect planning.

In 1963, Kornai got a job at the Computing Center of the Hungarian Academy of Sciences where the first mainframe computer had been installed in the country. There he launched the implementation of their planning concept. In order to avoid confrontation, Kornai did not question the legitimacy of the original targets of the five-year plan for 1966–1970 that he had promised to improve. Instead, he treated them as constraints of the model and experimented with various objective functions such as increasing the balance of current account in convertible currency or the value of private consumption (Kornai 2007, 148–49). Although he was unfamiliar with Arrow's impossibility theorem at the time, he instinctively resisted accepting a one-and-only welfare function defined by the communist ruling elite. He was firm in promising to not design an optimal plan and only to propose a better plan than that offered by verbal planners.

Yet, the original two-level algorithm in such a large model[93] was too complicated and had to be radically simplified. Thus, the results became much less precise and less true-to-life, while the computation process proved too slow to support the planners. Communication between the center and the sectors (not to speak of the firms) was clumsy and unpunctual, and the center proved intolerant to run enough iterations, which jeopardized the model's operation. Moreover, the input data were unreliable, intentionally distorted by the bargaining partners while the objectives and even the constraints were seldom defined by the policymakers clearly, often contradicted one another, and changed, following a chain of improvisations during the planning process. As a rule, the verbal planners were reluctant to reveal the sources of information they used in crafting the plans (e.g., when estimating the model coefficients), and—like their bosses—took the mathematical results seriously only if those supported their preconceptions. As a consequence, despite its scientific elegance, the Kornai and Lipták model could not be set up, computed, and implemented in planning unless one made a series of humiliating scholarly, political, or mundane technical concessions (Kornai 2007, 145–46, 155–56).

In other words, the "Faustian bargain" did not really work. The optimal planners offered the state their expertise in rationalizing planning (cf. the algorithm of "plan improvement") without making the communist rule questionable (they even helped prolong its existence), but the state did not provide in exchange proper data, sufficient computing infrastructure, or unambiguous economic policy goals and constraints, which were all necessary to run the planning model. Kornai's memoir testifies that in the early 2000s he still had a bad conscience because he had collaborated with the Kádár regime during its "consolidation" after 1956, for which he had excused himself earlier in the hope of increasing the welfare of Hungarians a little through optimizing the planning procedures. With the wisdom of hindsight, he did not find any other major advantage of this failed undertaking than its contribution to augmenting the mathematical knowledge of economists and releasing some of them from the ideological cage of textbook political economy (Kornai 2007, 147–57).

This is how Kornai remembered the reasons why the enthusiasm of his research team ebbed following five years of hard work to improve the central plan during the 1960s. His narrative borders on the Mises and Hayek impossibility thesis. However, some years after he had quit the terrain of optimal planning, he put his frustrations more diplomatically. Had he really managed to quit the project of ameliorating the planning system? Kornai related his model experiment in a book entitled *Mathematical Planning of Structural Decisions* (*A gazdasági szerkezet matematikai tervezése*) in 1965, published it in English in 1967. A slightly revised second edition came out in Hungarian in 1973 and in English in 1975. While outlining the difficulties of mathematical planning at length, none of these works alluded to the fact of impossibility. Instead, Kornai repeatedly comforted the reader, occasionally in a hopeful tone, about the need of central planning and its optimization *against all odds*. Although one "threw stones into the coffee mill," to use his phrase, that is, processed crude and unreliable data by sophisticated quantitative models, these models displayed the logical structure of planning decisions as well as revealed the inconsistencies of traditional plans and made these plans sounder. In sum, mathematical planning has a "pedagogical function": "it schools in rationality," it offers a "modest extension of rationality" (Kornai 1975, 426, 428, 523–25).

Even in the 1973 edition of the book he praised the procedure of plan improvement, the extension of two-level to multi-level planning, and a future construction of a pyramid of planning models and computing centers with the Planning Office on the top. In his view, the implementation of this vision—that "may rightly seem to be a utopia at a stage like the present"—basically will depend on the pace of development of computing capacity and expertise. Of course, planning does not have to be all-encompassing: it

has to focus on "fundamental" economic processes (like capital investment) while the less fundamental ones can be left to the market (Kornai 1975, 377, 380). Incidentally, the years from 1972 to 1973 were the start of what was called "recentralization" or "counter-reform" in Hungary when the New Economic Mechanism suffered a serious backlash. In order to dull the edge of an anti-market interpretation of his reasoning, Kornai (381–84, 524) distanced himself from any kind of "computopia"[94] and claimed that his model does simulate market processes since the center actually distributes resources like an auctioneer. Nevertheless, he failed to explain why then a Lange or Malinvaud planning regime that imitates auction to a larger extent would be less realistic than his two-level planning scheme that was at least as dependent on ideal assumptions on the economic behavior of the main actors of the game.[95]

Meanwhile, Kornai's *Anti-equilibrium* came out in 1971, which passionately called at least two basic principles of optimal planning (equilibrium approach and optimization) into question. Two years after, the reader was surprised to see, as an explanation for the glaring contrast, Friedrich Dürrenmatt's cynical *bon mot* in the introduction to the second edition of the volume on planning: "He who never contradicts himself will never be read again" (Kornai 1975, XIII). Why criticize the very core of the neoclassical research program and republish shortly thereafter a volume on optimal planning, not to mention a few other articles on similar subjects (e.g., "plan sounding," see below) and participation in discussions on long-term planning in Hungary during the 1970s?[96] Undoubtedly, it was easier to satisfy the censors by contending that the Western mainstream was fatally flawed than by admitting that the rationalization of central planning proved to be an illusion. At the same time, if one goes beyond this simplistic political/moral explanation, it seems also likely that Kornai hesitated to decide which path of Westernization to take until he became absorbed in preparations for his subsequent book, *Economics of Shortage* (*Hiány*), that came out in 1980. Arriving at a crossroads, he could have insisted on the path he had chosen at the end of the 1950s, which led to neoclassical economics *and* made him an illustrious member of the international research community of planning theorists.[97] However, he also may have hoped that, by abandoning neoclassical theory (or correcting its allegedly fundamental mistakes), he would not have to give up his work on planning but could perhaps opt for modeling its indicative rather than directive (decentralized rather than centralized) varieties in the framework of a new—universal—systems theory suggested by *Anti-equilibrium*. This also would be a basically Western product but contain a larger-than-ever Eastern contribution. Simply put, he yearned to have his cake and eat it, too.

Kornai embarked on the second path without knowing the refusal he would provoke by challenging neoclassical economics so fiercely. The fact that

he jumped from recognizing the failure of optimal planning into a blanket disapproval of general equilibrium theory and—more broadly—neoclassical economics was difficult for the representatives of the latter to digest. They believed, not without foundation, that Kornai threw the baby out with the bath water. Initially, *Anti-equilibrium* was received with deafening silence—except for soft applause from some "old-institutionalist" experts in the West and textbook political economists in the communist countries, both feeling justified in their contempt for the mainstream.[98] From a bird's-eye view, the decision to turn against neoclassicism while retaining the instruments of mathematical economics was a bold venture, even if Kornai could not know at the time that in some years Leonid Kantorovich would receive the Nobel Prize for his findings in a field very close to Kornai's research on optimal planning. Kornai was unaware of his "objective" boldness probably due to an optical illusion. Neither Tjalling Koopmans, who shared the prize with Kantorovich, nor Kenneth Arrow had dissuaded him from challenging general equilibrium theory when he visited them in the United States at the end of the 1960s. Reading the manuscript of *Anti-equilibrium*, these two eminent protagonists of the theory even helped strengthen the arguments of their Hungarian colleague,[99] but this could not prevent a third eminent protagonist Frank Hahn (1973) from publishing a devastating review of the book under the frightening title "The Winter of Our Discontent." It revealed Kornai's methodological naïveté reflected in his failure to make a distinction between the internal consistency of an abstract theory and its realism/applicability. Who said that we wanted to develop an empirically accurate "real science" that you require from us, asked an embarrassed Hahn.[100] Kornai was also reprimanded for (a) rejecting a workable and coherent scientific paradigm from the platform of a "vague and misdirected" research program, (b) using "his most vehement language to criticize what he has not properly understood," and (c) introducing dozens of new terms from his still non-existent theory, most of which were "empty boxes" (325–29).

Although the review was patronizing, it only mirrored Kornai's militant discourse and quasi-neophytic zeal against orthodoxy. To use Lakatosian language, besides a constructive criticism of the assumptions within the "protective belt" of the neoclassical research program, Kornai also called into question major axioms of its "hard core." His targets of criticism ranged from the principle of rationality based on optimization (this is what he considered the "original sin" of neoclassical theorists) and the maximization of profits and consumer utility, through using a normative concept of equilibrium, idealizing perfect competition, as well as disregarding increasing returns, non-price signals, and changing preferences, to the static and institution-free nature of the theory and its inattention to uncertainty. He accused the general equilibrium (GE) school of focusing on nothing else but these facets

of economic systems, and hence, analyzing just one set of key economic features of the real world. In his opinion, its members actually dealt with partial rather than general equilibrium. Thus, they moved backward from the position of Léon Walras and have "become a brake on the development of economic thought" including "most of the work which is attributed to the <neo-classical school>. . . . The GE school makes the description of economic systems entirely too dull; it over-schematizes and impoverishes it" (Kornai 1971, 27–8, 30).

Part of the criticism could have been reasonable if neoclassical economists had not wanted to offer a complex but coherent *ideal* scheme instead of an empirically relevant "comparative systems theory" envisaged by Kornai. Similarly, his anti-equilibrium drive originating, to a large extent, in the dismal fate of optimal planning in communist countries certainly would have encouraged his potential allies to think twice about the pros and cons of the neoclassical paradigm if he had been able to substantiate that the project of plan improvement derailed exactly because of its optimization philosophy and not because of the fact that this philosophy was tested in planned economies. Moreover, *Anti-equilibrium* was permeated by strong doubts about market coordination and weaker ones about planning, which did not increase its popularity, even in non-libertarian circles of economic thought.

Later, Kornai saw, with a peculiar mix of regret and self-justification, his attack on neoclassical economics as rather unfortunate. He admitted to have made

> serious errors in the theoretical starting points of my [his] critique, within the philosophy of science. . . . Modelers can be accused of many mistakes, but not of abstracting from reality. . . . The market economy that actually operates under capitalism is far from the Walrasian ideal, but the ideal makes a useful gauge of how far reality lies from it. . . . I should have attacked not the purity of the theory (the abstract, unreal nature of its assumptions), but the wrong use of it in mainstream economics. The real addressee of the critique should have been mainstream teaching practices and research programs. (Laki 2006, 28–30; Kornai 2007, 183–85; 2018, 7–9)[101]

Following this confession, Kornai repeated some of the main points of the "indictment," submitted in his *Anti-equilibrium* more than 30 years before, concerning the notions of rationality, optimization, equilibrium, and so on. He called *Anti-equilibrium* a "semi-failure" and was proud to "grope in the right direction," adding that he might have employed a less offensive language, delayed the attack until his counter-theory matured, and trusted in the ability of the mainstream to progress (Kornai 2007, 185–90, 192–95). At the same time, he ignored an alternative road leading out of the impasse into which he had led his research program. Yet, given his never-ending interest in institutions, he might have joined the emerging stream of new institutional economics during the 1970s, combining neoclassical methodology with realism, that is, orthodoxy with his favorite heterodoxy.[102] Surely, as Kornai had hoped, he managed to contribute to a spread of mathematical culture among economists behind the Iron Curtain. Nevertheless, prior to 1989, he was probably just as successful in persuading them *not* to fall on their knees before the neoclassical school.[103]

All in all, Kornai did not accept Hahn's criticism[104] but took it as an act of exclusion, almost excommunication, decided not to burn his fingers again, and withdrew to his own safe territory, the study of planned economies. He wanted to prove that he had not been wrong when dismissing general equilibrium theory and embarked upon a kind of disequilibrium analysis, using some neoclassical instruments but also inventing a series of verbal means to study a new field, the economics of shortage. He assumed that this research program would offer him, as he said later, a "one foot in, and one foot out of the mainstream" position (Kornai 2007, 195) that was sufficiently Western in terms of methodological rigor but did not sacrifice the imagined realism of his own *Sonderweg* proclaimed in *Anti-equilibrium*.

Meanwhile, Kornai stopped bashing the neoclassical paradigm openly but did not forget his bitterness regarding the concept of equilibrium.[105] His research program became less universalistic: it did not aim at founding an overarching systems theory any longer. Following the excursion to occupy a place on the peak of economic sciences in the world, he tried to carve out a large niche for himself a little lower and slowly returned to his former role played as an "area studies" scholar. Here, he put up with examining disequilibrium in planned economies but never ceased to call himself a mathematical economist who combined formal models with verbal research of institutions that he had abandoned in the late 1950s.

Interestingly enough, this turn was preceded, like in the case of Bródy, by (a) further work on planning models (Kornai et al. 1971; Kornai, Dániel, and Rimler 1972; Martos and Kornai 1973; Kornai 1973), (b) a short digression to alternative approaches to economic development (Kornai 1972a), a polemic verbal study in favor of balanced growth,[106] and (c) a reexamination of the concept of economic control (Kornai and Martos 1973; Martos and Kornai 1981; Kornai and Simonovits 1977. Although the latter initiative might have evolved into a general theory again, Kornai dropped his anchor at studying "non-price control" (including "vegetative control"). These were favorite notions already in *Anti-equilibrium*, which have much more to do with planned than market economies and led him directly to studying the economics of shortage. This agenda reinforced his position as an esteemed expert of the economics of really-existing socialism but further alienated him from the

research community of neoclassical economists while not bringing him into the fold of their heterodox critics.[107]

After a while, this kind of expertise concerned economic control rather than planning, and the normative attitude of plan improvement was replaced by the research objective of describing and explaining how planned economies are regulated with a special emphasis on non-price signals.[108] Simultaneously, the principle of optimization vanished from Kornai's research agenda, and with *Shortage* the share of mathematical reasoning also diminished in his work. He did not miss an opportunity to package the principal notions of his theory in mathematical formulae but failed to construct a synthetic model of shortage with their help.[109] Although in terms of verbal research, his book contained a great number of original approaches to concepts such as soft budget constraint, vegetative control, resource-constrained system, shortage versus slack, friction, queuing, and forced substitution among others, it applied formal models to illustrate rather than to profoundly analyze the planned economy as well as to measure its functioning. Thus, it could not catch up with the level of mathematical erudition of the disequilibrium school emerging at the time.[110] This is how Kornai remembered his debate, for instance, with Richard Portes and associates: they "had one huge advantage over me in these debates. They gleaned data from the statistics available to them. They were then able to make mathematical-statistical calculations, which undoubtedly impressed everyone. I could do little else than appeal to intuition or common sense; I could not oppose the quantified Portes models with likewise quantified Kornai models" (Portes and Winter 1980; Kornai, 2007, 249). He admitted that he realized too late that, despite the fact that the *Economics of Shortage* that he regarded as his *magnum opus* had a deeper insight than its rivals in the imbalances of communist economies, it would be outcompeted in the scholarly market. Indeed, the book's illustrative models construed to comprehend cause-and-effect relationships lacked the necessary explanatory force and econometric sophistication; furthermore, they were not tested on a critical mass of data.[111] His efforts to fill this gap eventually stumbled upon the collapse of communism: the time series data needed to substantiate his own interpretation of shortage could not be gathered any longer.[112]

An even greater disappointment for Kornai derived from his inability to identify a comprehensive and skillful mathematical portrayal of what he considered the main discovery of the book, the "soft budget constraint syndrome."[113] Such models were developed later by a number of scholars,[114] most famously by Mathias Dewatripont and Eric Maskin (1995). They employed game theory to capture the strategic interaction between firms and supporting organizations that bail them out. In their model interpreting the syndrome as a dynamic commitment problem, the actors maximize utility (payoff) and arrive at bargaining equilibria—the latter two concepts had been used by

Kornai back in the 1960s. Moreover, the primary reason for the soft constraint, paternalism as presented in *Shortage*, became a secondary issue that in the authors' view was neither a necessary nor a sufficient condition for comprehending the syndrome (Kornai, Maskin, and Roland 2003, 1111). In other words, the notion of paternalism was, in fact, abandoned, and thus the validity of the theory could be extended beyond the borders of the planned economy, fulfilling an old desire of Kornai.[115]

At the same time, it remained unclear whether paternalism could have been formalized and measured at all (cf. Kornai and Matits 1987a, 1987b), and whether—if one nevertheless sticks to murky explanations—this was really the best way to grasp the deeper political/ideological causes of the softness of budget constraints, or Kornai ought to have named directly at least a few powerful institutional factors such as state ownership, one-party rule, and Soviet occupation. In his memoir, he justified this choice with the need of self-censorship (Kornai 2007, 242–44, 253–55). Again, without passing any moral judgement on his decision, it had a heavy price in terms of scientific quality. If Kornai had not degraded his relationship with neoclassical economics dramatically, he might have gained inspiration from its new-institutional extensions and refined the notion of paternalism with the help of property rights, rent seeking, or principal-agent models, thereby not only showing political courage but also playing a pioneering role in universal economic research again, like he did at the time of inventing two-level planning. While many of his colleagues in the West[116] borrowed from new institutional economics among other subdisciplines in order to "consolidate" his concept of softness, he contented himself by saying "I did not use the term <institution> in every second paragraph as it recently has become fashionable to do, but I think I understood what a system means, and what the difference is between socialism and capitalism" (Kornai 2000, 654). If he had not thrown the concept of optimization overboard several decades before but assumed *some kind* of rationality in the behavior of the party-state, that is, if he had accepted that it can even maximize utility in a strictly economic sense of the word, then he might have arrived at the conclusion of his neoclassical-minded colleagues much earlier. They claimed that a bail-out of a state-owned enterprise can be in the best interest of the communist authorities not only because of purely political, ideological, reputational, and other considerations (which are also seldom immune to some economic motivation) but also of ordinary calculations of costs and benefits (Dewatripont and Maskin 1995).

During the 1980s, Kornai had almost everything at his disposal to crown his scientific career: the discovery of the importance of soft budget constraint, first-hand knowledge of the intricacies of the planned economy (and, as he says, "intuition and common sense"), mathematical skills, *Sitzfleisch*, embeddedness in Western academic culture,[117] and so on. At that time, one might

think that nothing could prevent him from receiving the Nobel Prize virtually any time. Allegedly, he has been nominated quite a few times among the frontrunners since then. We suspect that Kornai's bad luck with the prize was rooted in his decision to launch a frontal attack against neoclassical economics, which, following a brief period of self-Westernization in the 1960s, led him back to the realm of area studies and left him without a reliable methodology. Capitalizing on the results of his authentic research programs accomplished in the "communist laboratory," he could have returned even more successfully than he did to the world of universal economic sciences via less self-censorship (if he had trusted more in the decay of the Kádár regime) and through borrowing from new institutional economics (if he had not continued to reject its neoclassical foundations). In both cases he chose a language the leading epistemic community of economists in the world did not want to speak. It seems that *sic* non *itur ad astra* . . . [118]

OUT OF THE TRAP? TENTATIVE CONCLUSIONS

To return to our working hypotheses, in the previous sections we witnessed how difficult it was for the adherents of optimal planning to leave this research program behind and release themselves from the trap that prevented them from becoming "regular" neoclassical theorists prior to 1989. In fact, they could not help facing[119] a long chain of serious shortcomings. They were shocked to realize that—despite improving the mathematical quality of their models and raising the capacity of computers to run them—their optimization efforts repeatedly stumbled upon the institutional/informational regime of the planned economy.

The optimal planners may have expected that, with the advent of the New Economic Mechanism in 1968, the termination of annual plans, and a shift from mandatory instructions to "indirect regulators," the "controlled market" would enhance transparency and accuracy by disciplining the actors through competition while some political taboos might disperse. Instead, they saw an even more chaotic system of planning arise, in which plan bargaining was replaced or complemented by "regulatory bargaining," to use the contemporary phrase. Apparently, capturing such a complexity of bargaining games by means of numerous small models of optimization instead of constructing a single Big Optimal Plan did not prove an attractive (or viable) scientific venture for mathematical economists in Hungary.[120] Yet, here again, an exchange of ideas with new institutional economists in the West probably could have been beneficial for both sides and paved the way for the Hungarian experts to reconcile themselves with neoclassical ideas without having to fear from ignoring real-world problems.

Unfortunately, the empirically grounded insights in the imperfections of optimization were not condensed in elegant scholarly theses. Instead, they sank into the tacit knowledge of mathematical economists. The research community of optimal planning in Hungary did not rethink the Socialist Calculation Debate in the light of the dismal experience of mathematizing central plans and challenge the axiom of rational economic calculation under communism.[121] Many of its members continued to refine the methodology of planning and moderate the worst outcomes of the bargaining games. They relaxed the initial—often prohibitively strict—assumptions, eliminated some of the simplifications of their models requiring homogeneity, linearity, closedness, determinism, staticness, and so on, and fine-tuned the estimation of data. The remedies also included disaggregation and "monetization" of the models, incorporating human capital and foreign trade and decentralizing the planning procedures (Augusztinovics 1981; Réti et al. 1981; Augusztinovics 1984, 43–85; Augusztinovics and Bod 1985; Ámon and Ligeti 1987; Sivák 1987). At the same time, the mathematical economists did not suggest any substantial change to the planning regimes. They, including Bródy and Kornai, demanded neither an irrevocable transition from imperative to indicative planning nor at least the dismantling of the central planning of capital investments, a major obstacle to marketization under the NEM.[122]

Those experts who were not locked up in the treadmill of the daily fabrication of plans turned to long-term planning,[123] which was much less exposed to the interplay of lobby interests than five-year plans. True, it was with diminishing hope that they were waiting for the arrival of an enlightened technocratic elite, to which they could have handed over a Great Plan of modernizing the Hungarian economy during the 1970s and 1980s. While planning became a less popular scientific undertaking, input-output models were prepared even in the 1990s (e.g., Halpern and Molnár 1997), and the perfection of I-O theory was not terminated for good. Besides Bródy, one of his followers, Ernő Zalai (1997, 2014), kept on publishing in this field during the 2000s. As to Augusztinovics, she closed the story of the research program by saying that "the heyday of Input-Output as a simple, transparent, deterministic, static linear model is . . . certainly over." She added though that its "subject matter has not been lost, . . . it has merely been transformed, incorporated into more complex structures. The subject matter . . . is the dual and circular nature of the economy in general" (Augusztinovics 1995, 275).

What about the two pioneers? Did the bifurcation of their research programs result in differences in their assessment of neoclassical theory? As suggested above, Bródy chose another way out of the trap. He lost faith in educating the communist decision-makers through planning models early on, and did not trust in market reforms either since he had second thoughts about both the efficiency of market coordination and the altruism of communist bureaucracy

that was supposed to manage marketization—something that probably would jeopardize its own integrity.[124] Therefore, he elevated his research program to a higher level of abstraction and made efforts to identify organic links between the Marxian theory of labor value and input-output analysis (later even claiming that neoclassical theory is a special case of them)—not quite the best *rite de passage* to become a neoclassical economist. Remaining in the realm of mathematical economics, Bródy strived to prove that *all* economic systems suffer from cycles, any convergence toward market equilibrium is actually a cyclical oscillation around that, and economic dynamics can best be explained through a combination of classical (including Marxian) theory of labor value and marginalism—a contention again that did not really match standard neoclassical principles. As for his self-image, Bródy (1994, 325) liked to characterize himself as an heir of the classical tradition.

Kornai's was perhaps a more complicated case. It was neither an attraction to Marxism nor a high-level mathematical understanding of economic dynamics that prevented him from subscribing to the neoclassical paradigm. Unlike Bródy, he was not animated by abstract concepts of economic development ranging from the Neumann model to chaos theory, and distanced himself from both Marx (tacitly, quietly) and the neoclassical school (openly, loudly). Rather than finding the institutional architecture of the communist economy responsible for the failure of optimal planning, he blamed—with a dose of self-criticism—the "neoclassical illusions" blinding mathematical economists like himself. In passing, he alluded to the Socialist Calculation Debate and—while Bródy did not defy the legacy of Lange—Kornai disliked the Lange tradition as an unfortunate mix of Marxist and neoclassical thought and dropped skeptical remarks on Lange's "naiveté" in postulating a fruitful cooperation between the state plan and the regulated market. Here, he made no distinction between Hayek's classical liberalism and the neoclassical view of the market: both of them were rejected as *laissez faire* doctrines. After having left optimal planning behind, he continued to define himself as a mathematical economist but insisted on many of his former doubts about neoclassical thought. [125] Being "one foot out," however, prevented him from building new mathematical models as powerful as earlier.

Arguably, the failure of optimal planning did not prompt the two pioneers to critically examine the deep layers of the institutional world of the planned economy, no matter how knowledgeable they were about not only the economic sociology but also the social anthropology and psychology of central planning's main actors.[126] Refraining from thorough institutional studies could be justified by (self-)censorship and—until the mid-1960s—by the hopelessness of far-reaching economic reforms. Nevertheless, with the New Economic Mechanism appearing on the political agenda, ideological cautiousness did not require persistent skepticism toward the efficiency of

market control, particularly not a frontal attack on neoclassical theory. As presumed in the first pages of our chapter, such attitudes and actions can hardly be explained if the historian solely focuses on political fears and ignores scientific preferences.[127]

It is our hope that the story we have told about the evolution of planning concepts in Hungary shed light on a whole series of sources of those preferences: Marxist indoctrination, misinterpretation of neoclassical theory as a bundle of abstract (unrealistic) ultra-liberal ideas, seeking a *modus vivendi* between communist and capitalist fundamentalisms, pride felt for authenticity and equality with the West in terms of scholarly discoveries, inertia of a large and initially promising research program, self-deception promoted by Western peers, and so on. Let us leave aside the questions of how justified and coherent these motives were and which author was inspired by which of them the most. Rudimentary answers to them were scattered in the notes attached to this chapter. Be as it may, it was the same motives (fixations?) that helped the former adherents of optimal planning avoid entering other dead-end streets, favored much too long in a number of communist countries, such as the decentralization of planning (e.g., on the basis of workers' self-management or on that of mega-enterprises) or, on the contrary, the organization of vast—centralized and automated—planning systems spirited by a sort of "computopia."

The I-O analysts and the optimal planners in Hungary had to accept the inevitable: what they once thought would become a hegemonic discourse and planning technique remained a negligible, auxiliary tool in the hands of the top *apparatchiki* of the party-state. Over time, hegemony was attained by another group of economists, the market reformers, by far the largest segment of the research community in Hungary. Witnesses to the failure of rationalizing the plan, they were comforted in their conviction that the agenda of marketization of the planned economy had no real alternative: depending on the boldness of their project, they claimed that central planning must be tamed or dismantled—but not optimized. The failure of optimal planners strengthened the pre-existing suspicion of many institutional reformers toward mathematical analysis as such, which in turn blocked their road leading to neoclassical economics.[128]

POSTSCRIPT ON ECONOMETRICS

The examples of Bródy and Kornai as well as their disciples demonstrate another comparative advantage *vis-à-vis* their colleagues in many communist countries. During the 1970s and 1980s, a growing number of mathematical economists in Hungary turned their backs on the normative strategy of

improving central plans.[129] Although this turn was unspectacular, the program of producing a sound analysis of the planned economy (not just planning as such) with the help of mathematical instruments eventually replaced the intention of enlightening the *nomenklatura* and supporting the communist regime through "science-based" plans. While it was not always clear where optimal planning ended and where econometrics started,[130] many of the younger experts refused to construct overarching planning models and shape countrywide economic policies any longer. They indulged in econometric research and—after experiencing the imperfections of their own simultaneous macro-models—contented themselves with smaller-scale research projects that were to comprehend the real world of certain segments of the planned economy. The econometricians' scholarly choices stemmed neither from a deep sociological/political critique of planning under communist rule nor from a devotion to neoclassical principles. Nevertheless, they *nolens volens* kept a larger door open for Westernizing their research programs than I-O experts and optimal planners earlier (Kőrösi 1996).

Their role was controversial in other respects as well. In the field of quantitative economic research in general, empirical analysis was neglected for a long time. In retrospect, this may be surprising because in Hungary empirical studies served as a bridge connecting pre-and post-communist economic sciences. Subdisciplines such as labor and educational economics, health economics, financial economics, and empirical industrial organization, which applied econometric methods extensively, progressed more rapidly during the past decades. The roots of neglect stretch back to the early period of communism.

Back in the 1950s and early 1960s, empirical works containing statistical arguments were sporadic in Hungarian economic research. As mentioned, the foundation of the Econometric Laboratory at the Central Statistical Office brought some fresh wind into quantitative studies although its econometric investigations were not concatenated with models of mathematical economics in a way suggested, for instance, by the Cowles Commission's slogan of "theory and measurement."[131] Mathematical economists mostly avoided confronting their models with empirical data; therefore, the stage of verification was absent in their research agendas. This cannot be explained simply by the lack or bad quality of empirical information. Data served as a source of inspiration to generate a model (cf. Bródy's theory of cycles and Kornai's concept of overcentralization) or to support economic policy arguments (cf. the Laboratory's model series) rather than to precisely corroborate or falsify scientific hypotheses.[132]

During the 1970s, the situation changed slightly. The I-O models started including stochastic blocks (e.g., Hulyák 1972). The parameter estimations of production functions and the regression analyses of macro data began to

infiltrate, quite unsystematically, the theoretical arguments instead of only helping solve practical problems and fill the input-output tables and SAM matrices. The authors were mostly self-made econometricians who chose their topics and methods often accidentally.[133] In the late 1970s and the 1980s, macro-econometric models still used the already outdated method of simultaneous equations (Kőrösi 1996, 359). The critique of simultaneous macro-econometric models (Lucas 1976; Sims 1980) did not affect this attitude for a while.[134]

A turn to more professional econometric research came only in the second half of the 1980s and in the 1990s. Younger researchers left I-O analysis and optimal planning for fields that were less macro-oriented and required robust evidence-based reasoning. To use the example of the Institute of Economics again, Halpern and Molnár started studying household statistics and corporate data that led them to so diverse research projects as the analysis of subjective welfare and industrial organization. Labor economists such as István Gábor, Károly Fazekas, János Köllő, and Gábor Kertesi explored employment and educational data and drew conclusions also with regard to gender and race economics.[135] A detailed case study made in the textile industry introduced some of them to both empirically and theoretically grounded procedures of neoclassical research.

The recognition of the problematic aspects of old-fashioned macro-econometric analysis discouraged some of the experts and micro-data methods became more popular. However, during the 1980s, there was no institution of higher education in Hungary to teach economists applied econometrics. Econometrics at the universities was regarded as part of the "high theory" of mathematical statistics. Symptomatically, the first generation of new-school econometricians like Gábor Kőrösi and László Mátyás learnt cutting-edge methods while teaching abroad (in Australia) in the 1990s, and returned to Hungary to cooperate with economists whose interest in micro-data analysis was greater than their knowledge of econometric methodology.

However, prior to 1989, this process of catching up with the West was not yet accompanied by a large-scale takeover of neoclassical principles and by a profound reconsideration of former assumptions and axioms of mathematical modeling, although a few young scholars (e.g., Imre Csekő, Júlia Király, János Vincze) decided to build their scientific careers on cultivating mainstream micro-and macroeconomics and finance.[136] A good example for the inertia of economic thinking was the way in which the computable general equilibrium (CGE) approach was received in macro-level modeling (Zalai 1983). Due to the flexibility of this approach, it could be used in input-output tables and SAM matrices without subscribing to the underlying philosophy of general equilibrium theory.[137] Similarly, the real business cycle (RBC) model proved too neoclassical to be adopted by Hungarian economists before 1989.

In sum, Hungarian econometricians built new pillars to support the bridge that input-output analysts and optimal planners had begun to erect in the late 1950s but only a few of them proved able to reach neoclassical economics situated on the opposite side of the abyss separating them. No matter how robust and sophisticated the new quantitative models became in comparison to those formulated by optimal planners, econometrics in communist Hungary (a) did not excel with significant original discoveries, and (b) failed to evolve into a compact discipline in close cooperation with micro-and macroeconomic theories. Unlike their peers beyond the Iron Curtain, Hungarian econometricians indulged in applied rather than basic research, the applications were scattered over a random variety of topics and were not underpinned by a tightly woven net of neoclassical concepts. In a sense, they moved ahead too quickly in the 1980s: they had to wait for the breakthrough of the other two core disciplines of neoclassical thought, micro-and macroeconomics, to progress further.

NOTES

1. This term will be used in our chapter in descriptive sense except when a scholar under scrutiny attaches a normative meaning to it.

2. The department of mathematical economics of the Hungarian Economic Association, the only professional organization of economists in the communist era, was established in 1962.

3. It was first issued by the Hungarian Economic Association in 1968. Béla Martos served as its editor-in-chief until 1990.

4. Bródy was one of the founders of the Association and Kornai was elected president of the Society in 1976.

5. While one can have second thoughts about the quality of the neoclassical breakthrough, its quantitative indicators, ranging from journal articles through university curricula to East-West research projects, show a sweeping victory of mainstream economics imported primarily from the West (see Kaase and Sparschuh 2002).

6. For example, László Csontos, Gábor Kertesi, Péter Pete, Balázs Váradi and others played a crucial role in setting up new neoclassical-style departments of economics at Budapest universities in the 1990s. Of course, these experts also felt happy about the fall of censorship, despite the fact that they were already lucky to not face a cruel "thought police" in the 1980s.

7. This was the infamous "3T" principle (in Hungarian: *tiltás*, *tűrés*, *támogatás*) distinguishing between prohibition, toleration, and support.

8. In fact, general equilibrium theory received quite a few punches that seemed devastating, but it managed to survive and prosper, resulting in a series of new models of computable and dynamic-stochastic general equilibrium (Kovács 2009).

9. "Improving," "developing," or "perfecting" the plan were terms used above all by the official rhetoric. The pejorative alternative was *tervkovácsolás* (hammering the plan) originating in the German word *Planschmied.*

10. As the reader will see, there is also a third hero in the story, Mária Augusztinovics, who would deserve a separate study. She was involved at each and every stage of research made by the two "pioneers" who probably would not have been able to reach the Parnassus of Hungarian economic thought without her help. A typical fate of an extremely talented female scholar, she was stuck willy-nilly on a lower level of scientific abstraction for a long time, striving to build optimal planning models in the Planning Office, even as late as the early 1980s. She could step out of the shadows of the two men only thereafter, when she switched to modeling life-cycles and pension systems. For a while, Augusztinovics was married to Bródy and was closer to him than to Kornai in terms of loyalty to Marxism.

11. For example, Szamuely and Csaba (1998), thus far, the most detailed overview published on the history of Hungarian economic thinking in the communist period, devoted less than a page to mathematical economists. The literature is dominated by works, in which the main representatives of the discipline and their associates share their memories with the reader or offer a snapshot of a certain stage of evolution in their scholarly field. Typically, these are brief texts, including published interviews and obituaries. Important (and refreshing) exceptions are Bródy's long biographical interview from 1994 and Kornai's voluminous memoir from 2007 (2005). A 1996 conference on "legacy, emulation, invention" in economics, in which numerous scholars, old and young, who conducted research in mathematical economics in any phase of their lives, made presentations also proved a very informative source, see Csekő (1996), Csontos (1996), Kőrösi (1996), Nagy (1996), Pete (1996), Simonovits (1996), Vincze (1996). Although normally Péteri (1993, 1997, 2002, 2017, 2019) do not focus on the nexus between mathematical economics and planning theory per se, they give valuable insights, based on careful archival research, in the political and sociological environment of their development. For the state of the art in writing the history of planning concepts in communist countries in general, see the Introduction and Conclusion of this volume.

The citations in this chapter were translated by us if no English-language translation was published.

12. For more on his personal attitudes to mathematical economics as experienced in the Institute, see Kovács (2016).

13. In preparing this section, we received useful research assistance from our students Dániel Baglyos, Barnabás Benyák, Zalán Cseresznyés, András Heténi, Balázs Mayer, Tamás Sáfár, and Dániel Tordai.

14. Admittedly, that pattern fell short of the organization of central planning in the Soviet Union in both width and depth. However, the leading economists of the time such as Károly Balás, Frigyes Fellner, Farkas Heller, Mátyás Matolcsy, Ákos Navratil, Tivadar Surányi-Unger, and István Varga (even those who preferred Austrian economics) did not regard planning as a derogatory term. For those among them who flirted with the national socialists or later with the communists this was a natural ideological gesture. However, cautious liberals like Heller or liberal socialists like Károly Polányi did not reject some kind of state planning categorically, not to mention Károly Mannheim with his eulogy of planning in general.

15. Actually, Káldor had visited Hungary for some months at the turn of 1946/47 in order to advise the social-democrats but did not return there for about two decades. Then he paid only short family visits and gave lectures.

16. Béla Balassa, who emigrated in 1956, wrote his first book in the United States exactly on the planning system of Hungary. Would he have become "another Kornai" if he had decided to stay?

17. They were leading researchers in the *Magyar Gazdaságkutató Intézet* (Hungarian Institute for Economic Research) founded by Varga in 1927. On combining German-style institutional research with econometrics, see Varga (1947) and Theiss (1947).

18. For a brief period during and following the 1956 revolution, Varga replaced Friss as director of the Institute of Economics.

19. On his contribution to mathematical economics in Hungary, see note 51.

20. At the time, many Hungarian economists understood Russian. They could read not only the works of Leonid Kantorovich or Viktor Novozhilov in their original but also, for example, the Russian translation of Leontief (1953, 1958) on the U.S. economy. Kantorovich's (1965) seminal book on the best use of economic resources has never been translated into Hungarian (cf. Simon and Kondor 1962, 1963). In the 1960s and 1970s, the similarity between the 1968 economic reform in Hungary and the NEP aroused interest among Hungarian scholars about the ideas of Soviet mathematical economists such as Grigorii Feldman, Nikolai Kondratiev, and others.

21. The uncensored version of the book was released only in 1988.

22. The translation of Zbigniew Pawłowski's *Ekonometria* in 1970 also demonstrates the remarkable influence Polish scholars exerted on their Hungarian colleagues. The same applies to the translation of the 1977 book by the Czech theorist Josef Goldmann on macroeconomic analysis.

23. Kornai cherished the memory of his debut in Western high theory when in 1963, he met Maurice Allais, Sukhamoy Chakrawarty, Frank Hahn, Leo Hurwicz, Tjalling Koopmans, Lionel MacKenzie, Edmond Malinvaud, Roy Radner, and Richard Stone at a conference in Cambridge (Kornai 1996, 268).

24. Since then, just a few Hungarian economists have succeeded in publishing in these journals (see Medvegyev 1984; Simonovits 1975, 1978).

25. Martos (1975, 1990) were also published by North Holland that agreed on a joint publication project with the Budapest publishing house *Akadémiai Kiadó*.

26. This was the only phase of communist history in which Hungarian scholars maintained strong links to leading Soviet and other Eastern European mathematical economists and insisted on publishing in Russian as well.

27. For the first English-language review of the evolution of the new research program in Hungary, see Horvath (1963).

28. Although these experts contributed to each other's edited volumes, joint articles were rare among them. For example, Augusztinovics, Bródy, and Kornai did not publish scholarly papers together despite the fact that they were good friends for a long time. For collective volumes, see, for example, Bod et al. (1962), Lukács et al. (1962), and Juhász and Morva (1982).

29. This English-language journal was edited in collaboration with the Institute of Economics from 1966 onward.

30. Nevertheless, in the beginning, they also had to publish in marginal bulletins run by industrial organizations or in "official *samizdat*" like the working papers of limited circulation, which were produced by various research institutes.

31. For a fifth institution, see note 63.

32. It was founded as Hungarian University of Economics in 1948 to offer a full-time degree program in economics. The Sovietization of the university during the late 1940s was crowned by renaming it Karl Marx University of Economic Sciences in 1953. In fact, until the late 1980s, it taught political economy instead of economics despite a surge of programs in mathematical economics.

33. See, for example, Hámori (1986). On the eve of the collapse of communism, low-quality experimental textbooks on micro-and macroeconomics were written by members of the Department of Political Economy (Váradi 2007).

34. In 1972, students of planning theory (both verbal and mathematical) organized a strike against the course syllabus offered by the Department, and demanded to change the list of mandatory readings by replacing the official textbook with works of András Bródy, Ferenc Jánossy, János Kornai, Włodzimierz Brus, Jan Tinbergen, and selected authors from the Soviet 1920s and the Socialist Calculation Debate. The new textbook (Stark 1981) made a few insecure steps in this direction. On the development of research on mathematical economics at the Department, see Móczár (1980).

35. Prékopa was a student of Alfréd Rényi (mentor to and friend of András Bródy, see below) who taught operations research to mathematicians from 1958. His main research area was stochastic programming. The research groups and departments in operations research headed by him at two Budapest universities and the Academy of Sciences became strongholds of education and background studies of optimal planning (Prékopa 2018).

36. The curriculum was reorganized many times. Besides Béla Krekó and Jenő Szép who held mostly the calculus, linear algebra and operations research courses, Margit Ziermann, a student and co-author of Prékopa taught stochastic processes, and György Meszéna mathematical statistics. Later Géza Denkinger and István Dancs also entered the Department and taught core mathematics courses, Ferenc Forgó joined Szép in teaching game theory and János Paizs econometrics. In order to strengthen the ties to economic applications, Krekó published textbooks for each and every course, which were linked by pivoting techniques that allowed the solution of economic problems through computer programming (see, e.g., Krekó 1972).

37. In the early 1970s, it owned the highest-performance computer in Hungary (*ICL-4/70*). The first staff of about 40 operators were trained in London. The research affiliates included Bródy and Kornai. The main task of the Center was to prepare sectoral and central plans with the help of input-output analysis and later linear programming. The Kornai-Lipták model of two-level planning (see below) was also run here.

38. Nonetheless, the Institute and the Office raised a large group of quantitative experts including Gusztáv Báger, Zsuzsa Dániel, Éva Ehrlich, Sándor Ganczer, László Hunyadi, Tamás Morva, János Réti, Béla Székely, and György Szepesi. See also note 46.

39. "There are fans of <verbal> and <mathematical> approach among economists. I do not belong to either of them. Moreover, I consider the contrasting of the two methods a wrong alternative. If you please I am the enemy of verbal method if it is based on . . . empty abstractions. However, I am equally an enemy of . . . mathematization for its own sake" (Jánossy 1969).

40. The structure of departments within the Office matched that of the sectors and branches of the economy.

41. Augusztinovics remembers: the mathematical models "did not become influential, decisive instruments in planning . . . . Our first results were not to the liking of supreme economic policy leaders because one could not squeeze out of the models a larger than 3 percent growth on average . . . or force them to support that billions and billions would be poured into agriculture. Then, we had to be silent for a while. Of course, sooner or later one learns how to constrain everything in a model in a way that we get what we wanted to . . . " (Augusztinovics 2000, 45; see also Medvegyev 2015). For the advantages of I-O models in planning, see Augusztinovics (1995).

42. "In the practice of planning, future coefficients . . . are usually derived from various sources of information, experience and speculation. These are amalgamated, by intuition, conscious weighing, simple or more complex arithmetic, and pondering, into the most probable guess. This domain of planning must draw on technical expertise and knowledge, general economic know-how and political common sense" (Bródy 1970, 120). Augusztinovics (1984, 45) put it more bluntly: "The decision process is hierarchical and decentralized, even if it looks fully centralized. . . . The processes of elaboration and acceptance are intermingled: this dual process is called plan coordination. . . . The battle of figures, arguments, and interests takes place on the same battlefield."

43. For the remarkably small number of ministry-level models in the early 1970s, see Farkas (1973).

44. Ganczer (1973) reports this failure using the example of the Fourth Five-Year Plan (1971–1975). A large group of experts in the Planning Office was commissioned to elaborate a mathematical model for the plan too late, in March 1969. They wanted to go for sure and decided to work out a linear programming model that was much simpler than Kornai's inoperational two-level planning scheme (see below). While making the calculations, the plan was approved by the government in December 1970, based on data that were largely different from the ones the researchers applied to set up their model. The real plan and the model became incomparable; therefore, the former could not be checked by the latter, even retrospectively.

45. It is symptomatic that Augusztinovics (1995, 273) could not imagine that the suggestion made about popular voting on societal preferences by Ragnar Frisch in the early 1970s could ever become viable.

46. With time, a group of younger able experts crystallized around Augusztinovics, including, e.g., Tivadar Faur, Katalin Haraszti, Júlia Király, János Réti, Béla Székely, and György Szepesi, who were ideologically less committed to central planning and put forward economic policy goals compatible with the radical programs of market reform. Small wonder that they were disliked by officials coordinating the five-year

plans, a large majority of the Office's employees, whose work was managed by another department.

47. István Hetényi, a student of Farkas Heller at the pre-communist University of Economics, later professor of public finance, is probably the best example for continuity. He had supervised long-term planning in the Planning Office until 1980 when he left for the Finance Ministry to lead, as minister, the preparations for the last reforms of the planned economy. Hetényi was not the only reform-minded leader of the Planning Office in the communist era. He worked together with communist technocrats such as Miklós Ajtai, József Drecin, István Huszár, Ottó Gadó, Miklós Pulai, and Péter Vályi.

48. Péter frequently attended economic debates on market reforms in the 1950s and 1960s, criticizing overindustralization, emphasizing the role of profit incentives and marketization in general. He became one of the first reform economists in Hungary although he and the chief economist of the Office, Júlia Zala, seldom took part in open political battles. Following György Péter's mysterious death in 1969, his deputy István Huszár was appointed the new president of the Office. He had initiated in 1968 that János Paizs, a self-made econometrician, starts teaching econometrics at Karl Marx University (Hulyák 2014, 72).

49. The former was headed by László Halabuk and György Szakolczai, the latter by Ferenc Rabár.

50. They made parameter estimations of CES production functions for specific industries and input-output calculations for the Planning Office. The Laboratory also built forecasting models and took part in the calculation of price indexes (Szakolczai 1972; Halabuk 1971; Havass 2011).

51. Theiss advanced his knowledge of neoclassical economics at leading U.S. universities (Chicago, Columbia, and Stanford), worked with Ragnar Frisch and Henry Schultz, and published in *Econometrica* and the *Journal of Political Economy*. Instead of emigrating after 1945 or 1956, he exposed himself to humiliation, being deprived of organizing a Hungarian school of econometrics. From 1948, Theiss served as head of the Statistics Department at the University of Economics. In 1950, he was accused of "mathematical formalism" and dismissed. He was permitted to teach again (but only law students) in 1959. Instead of becoming a celebrated pathbreaker of Western economics in Hungary, Theiss died as an isolated scholar. As so often in Eastern Europe, the subsequent generations had to reinvent what he had already known (Kádas 1980; Huszár 2008; Hunyadi 2012).

52. The M-4 model fitted into the pattern of Klein's LINK project that connected the trade accounts of several countries by uniform specifications to better understand trade flows.

53. The journal that had had various predecessors from 1874 on was founded in 1895. Between 1949 and 1954, it was called the *Hungarian-Soviet Economic Review* (Magyar-Szovjet Közgazdasági Szemle).

54. On Friss's professional and political ambiguities, see Péteri (1997, 2002, 2019). Among the recruited scholars Erdős and T. Nagy were prominent reform economists of the time while their younger colleagues (e.g., Bródy, Kornai, A. Nagy, and

the maverick Tibor Liska) joined them in their struggle with the textbook political economists.

55. Bródy was a conspicuous exception (see below).

56. It did not help them that a number of the first mathematical economists in the communist era, such as Andorka, Szakolczai, Theiss and Varga, were stigmatized as "agents" of the previous regime.

57. "We did have to pour Marxist holy water on mathematical economics in order to be allowed to deal with it. When physicists realized that, by frankly admitting what they thought, they—like Giordano Bruno—committed themselves to the flames, invented mathematical physics that the clergy did not understand" (Bródy 1994, 294). "Mathematical language was incomprehensible to commissars, party officials, and all who kept watch on institutes, publishers, and journals. Having seen a few equations in a manuscript, they put it down with a shiver" (Kornai 2007, 152). Erdős chose a different strategy of survival. After 1956, instead of relying on his profound mathematical knowledge, he left the reform battles for research on capitalist economies and became a critic of Keynes.

58. He used to make condescending remarks about mathematical economists, which prompted Kornai (1981) to publish a bizarre article, full of self-critical comments on mistakes these economists made, in defense of the discipline. This is how Nagy invited Bródy to join his research group in the 1960s: "Andriska, come over to us, you are a smart researcher, but the precondition of your transfer is that you will not deal with mathematics because I do not understand it" (Bródy 1994, 300–301).

59. Meanwhile, Tamás Nagy taught political economy at the university, without any special reference to mathematical economics.

60. Not only Bródy and Augusztinovics were married. Kornai and Zsuzsa Dániel who also worked on mathematical planning were husband and wife as well. Bródy and Jánossy (who was the stepson of the Marxist philosopher György Lukács) were good friends and most of them maintained friendly relations with Martos, A. Nagy, and Tardos. The latter was son-in-law of Péter. As years passed, many of their younger colleagues joined this network.

61. In 1964–1965, a number of important Hungarian works in mathematical economics were published: for example, Bródy 1964, Kornai 1965, Simon and Kondor 1965, Theiss 1965.

62. The volume included a chapter written by Kantorovich on optimal planning.

63. A smaller research unit, the Institute of Market Research (*Konjunktúra-és Piackutató Intézet*) where, among others, János Gács, Kamilla Lányi, András Nagy, Gábor Oblath, Péter Pete, András Simon, and Márton Tardos worked for a long time also needs to be mentioned in this regard. In the 1960s, they were building optimal models for planning foreign trade and rationalizing the New Economic Mechanism, and later engaged in econometric research in various fields of macroeconomics. With time, Gács, Nagy, Pete, and Tardos moved to the Institute of Economics.

64. The older one included, besides Bródy and Kornai, Sándor Ausch, Anna Gelei, Róbert Hoch, György Kondor, Béla Martos, Éva Radnóti, and György Simon while the younger one consisted of Péter Bodó, Éva Bondár, Judit Barta, Győző Gábor, László Halpern, József Horváth, Zsuzsa Kapitány, Gábor Kertesi, Ilona Kovács,

János Köllő, Gábor Kőrösi, Mária Lackó, György Molnár, Miklós Ördög, Judit Rimler, András Simonovits, Judit Szabó, Tamás Tarján, György Tényi, and Ildikó Virág. Many of them focused on I-O analysis and/or optimal planning (and all of them applied some sort of formal models) at a certain point in their careers. While frequently leaving the country for conferences, longer research stays, or teaching, with the exception of Bodó and Kondor, none of them emigrated.

65. Normally, the younger researchers came from Karl Marx University or the Faculty of Mathematics of Loránd Eötvös University, and were recruited by the heads of the research groups who protected them from political intervention "from above" both inside and outside the Institute. With the gradual decline of political control, the young generation of researchers became dependent mainly on their group leaders, basically the same persons for decades. Fluctuation between the groups was weak, and loyalty overrode voice and exit.

66. Of course, their tongues were much sharper among themselves. For instance, Bródy (1994, 316) liked to call Kornai "the last advocate of Stalinist planning" and made fun of the alleged imperfections of his mathematical skills while Kornai ridiculed Bródy's Marxist nostalgia and superficial reading of literature. Otherwise, they respected each other and wrote cordial reviews about each other's books with only a few exceptions (cf. Kornai and Simonovits 1981), organized conferences together, and assisted each other abroad.

67. She left the Planning Office for the Institute in 1984.

68. In the Institute even the dedicated Marxists (such as Bródy, Erdős, Friss, Hoch and T. Nagy) distanced themselves from textbook political economists. The latter were called *polgazdos* ("polecon" may be the translation) with some contempt.

69. To be sure, all research on mathematical planning presented in this chapter was dwarfed by a great diversity of verbal approaches of mixed quality, thriving outside the Institute, to the problematic of planning. These approaches, which unfortunately we cannot cover here, equally embraced (1) the confirmation of traditional (Stalinist) principles of central planning and a large variety of (2) diluting or (3) denying them. To give examples, Kálmán Szabó (1960) represented the first, Ákos Balassa (1979) the second, and Tibor Liska (1988) the third approach. Sometimes, even those experts stuck to traditional principles (e.g., directive planning) who otherwise worked on optimization (cf. Morva 1965, 1966). As for research programs unfolding within the Institute, there were excellent verbal studies providing historical comparisons of planning regimes and policies in the Eastern Bloc from a reformist perspective and offering the mathematical economists original variables to model. See, for instance, Bauer (1981) and Soós (1986) on investment cycles.

70. The interest of younger mathematical economists in market reforms was facilitated by the fact that, in contrast to how their older colleagues felt in the early 1960s, they already were not enchanted by the idea of improving planning (see below).

71. György Molnár (2019) recalls that, as a young mathematician, he tried to correct one of Bródy's proofs. "It was full of mistakes and I was convinced and eager to show that his theorem was false. After having fixed the proof, I realized that the theorem was true. Bródy saw the truth somehow through the algebraic structure

of the input-output model but was not interested in puttering around the technical details at all."

72. His attraction to Marx cannot be explained if one disregards his intimate relationship with many members of the Budapest School of the "renaissance of Marxism," including his brother Ferenc, a philosopher as well as Lukács and his family (Bródy 1994, 292–96).

73. That is why Bródy was so skeptical about isolated calculations of investment efficiency, which were fashionable at the time and which actually contributed to the breakthrough of mathematization in official economic thought. He was not enticed by the econometric studies of the 1960s either since he deemed their results less robust than those of the I-O models (Bródy 1960b, 954; 1994, 313).

74. Bródy (1969, 43) was convinced that in terms of both the mathematical formulation of Marx's theory of reproduction and its combination with the turnpike theorem, he preceded Michio Morishima's discoveries.

75. These sentences were omitted from the English translation of the book.

76. Ironically, despite such reservations about optimal planning, he—unlike Kornai—did not give up the principle of optimization at the end of the 1960s (see below).

77. See, for example, Jánossy (1969). As Bródy (1994, 330) put it, "it almost did not matter to me . . . if Ferkó [Jánossy] published what I said or if I published what he said."

78. "One was permitted to chat about *how* things should be done but the *what is to be done* question, that is, the issue of economic policy, cannot be tackled while the mistakes were made there" (Bródy 1994, 322).

79. Between 1989 and 2004, Bródy served as editor-in-chief of *Economic Systems Research*, the journal of the International Input-Output Association.

80. Bródy was not satisfied with this book and decided not to publish it in English. It received a rather unfavorable review from his close colleagues (Kornai and Simonovits 1981) who missed non-price control, regulatory lags and the softness of budget constraint in Bródy's dynamic model, which they regarded as innovative but unrealistic and sloppy in many ways. Ironically, in the same year, Bródy (1981) published a paper on non-price control in a volume edited by Kornai and Martos. See also Bródy and Farkas (1987), Bródy (1997b).

81. Later Bródy (1994, 307–8) modified his concept of dynamics in the spirit of chaos theory, claiming that often there are neither stable equilibria nor stable cycles in the economy. Accordingly, the change in economic variables is completely irregular, but it stays near the equilibrium (Bródy 2004b).

82. "They believe that they have centralized [the economy], yet, they only created a totally impenetrable layer between the leaders and the ground level" (Bródy 1994, 307).

83. There is no trace in Bródy's writings of any serious reading of the main contributions to the debate.

84. In this capacity he was invited to write an entry on "Prices and Quantities" in the 1987 edition of *The New Palgrave Dictionary of Economics* (Bródy 1990).

85. Close to the end of his life, Bródy (1994, 311) was sad to have accused Leontief of plagiarism in Bródy (1964), the first book he wrote on I-O analysis.

86. This was his doctoral dissertation based on surveys and interviews with employees of state-owned firms on planning in the textile industry, which Kornai managed to publish in Hungarian right after the 1956 revolution and two years later in English. Although the book was regarded in the West as a work of economic sociology rather than economics, it was recognized as the first credible description of how the planned economy works.

87. For more on this, see Kőhegyi 2019. (This paper was supported by NKFIH No. 125374.)

88. At the end of the 1950s, Kornai decided to become a ("normal") Western economist, quit political life, and abandon Marxism but stay in Hungary as a dissenter rather than a dissident without rejoining the communist party. He began to learn higher mathematics and English and read neoclassical authors like Arrow, Hicks, Samuelson, and Solow (Kornai 2007, 123–24, 133).

89. He was a colleague of Alfréd Rényi. It was Bródy who made Lipták acquainted with Kornai. In 1965, Lipták who suffered from a serious mental disorder emigrated to the U.K. and ceased to assist Kornai. His place was filled by mathematical economists such as Béla Martos, Ágnes Matits, András Simonovits, and Jörgen Weibull. In his memoir, Kornai (2007, 157–58) explained why he—unlike a majority of contemporary economic theorists in the West—was exposed to support in quantitative analysis during his whole career. See also Lipták's obituary written by him (Kornai 1998).

90. Kornai's name became known in the West after Oxford University Press published *Overcentralization* in 1959 following the advice of John Hicks (Kornai 2007, 109, 139).

91. They assumed that the central planner allocates input and output quantity requirements among the sectors while lacking much of the information needed for such a decision. In order to fill in the information gaps, the sectoral planners solve their own optimization problems with some programming technique and send feedback to the central planner in the form of shadow prices received from the solution of the dual side of the programming tasks. The feedback signals serve to balance the initial quantity allocations by price adjustment according to the logic of market clearing. The reallocation of quantities is followed by a new round of sectoral optimization procedures and feedbacks. The iteration continues until the optimal plan is reached on both macro and sectoral levels. (The model ignored firm-level planning operations.)

92. However, in the Lange-Malinvaud model the center communicates with firms and top-down information is mediated by prices, in contrast to the Kornai-Lipták model where the center communicates with sectors and the dialogue is mediated by quantities. The bottom-up information coming from the firms is conveyed in the Lange-Malinvaud model by quantities to make the size of excess demand or supply transparent while in the Kornai and Lipták model such feedback is sent by (shadow) prices. As Kornai (2007, 145) remembers, they were not aware of Malinvaud's (1967) solution when inventing two-level planning.

93. Originally, the mathematical task of planning for the 1966–70 and 1971–75 periods included nearly 500 product groups, 52 sectors, 2,000 equations, 4,500

variables, and 2,000 constraints (Kornai 1975, 432–48). According to András Prékopa (2018), the refined and effective decomposition techniques published by George Dantzig and Philip Wolfe in 1960 as well as by Jacques Benders in 1962 were not known in Hungary in the early 1960s. In his 1965 [1967]/1973 [1975] book, however, Kornai (1975, 346, 381) discussed the Dantzig-Wolfe algorithm in great detail. By that time, he was also familiar with a version of the Lange-Malinvaud model using that algorithm. Nonetheless, he decided to apply the so-called "plan improvement" algorithm invented by Lipták (which Kornai named a "naïve variant" of the Dantzig-Wolfe technique) to adjust to the lack of computing capacity in the country. They did not expect this technique to reach an optimal solution but only to approach it somehow. In this way, they sacrificed important properties of the Dantzig-Wolfe algorithm such as convergence, finiteness, and monotonicity.

94. Although the program of optimization did not lack utopian elements, no serious mathematical economist in Hungary came up with a radical cybernetic vision of central planning. Such a vision was rarely proposed even by old-school planning officials (cf. Sík 1966). True, initially, Kornai did not deny that creating large computer networks hosting so-called model pyramids might make sense. Hungarian economists remained immune to an alternative utopia, too. It was cherished in the close vicinity of Hungarian optimal planners by Tibor Liska whose program of "entrepreneurial socialism" envisaged the replacement of central planning by a loose collection of competing business plans proposed from below by small private enterprises.

95. In his later works Kornai liked to call Lange's "competitive solution" naïve (see Conclusion).

96. At that time, with young members of his research team, Kornai made attempts, with no particular success, at building a vast macro-simulation model of the Hungarian economy to test alternative paths of growth. The model did not exclude optimization *ab ovo* (Kornai 2007, 232).

97. On the pride Kornai felt over the rapid fulfillment of this promise from among those he made to himself after 1956, see Kornai (2007, 154, 159–62). In his eyes, Westernization included cooptation in international academic networks of scholars like Frisch, Malinvaud, Stone, and Tinbergen who showed interest in macro-planning. In the beginning, such a cooptation did not conflict with recognition coming from equilibrium theorists.

98. Here is a remark by Mária Augusztinovics expressing the irony of the situation. Following the publication of *Anti-Equilibrium*, her boss in the Planning Office chided her as follows: "Why do you always jitter about the national income deficit? Kornai has already said that equilibrium is not necessary" (Augusztinovics in Laki 2006, 30).

99. The book "will make a fine obelisk on the burial mound of the general equilibrium theory," commented Arrow on the draft (Kornai 2007, 178). Let us not guess here whether Koopmans and Arrow (or Jacob Marschak and Roy Radner with whom Kornai also discussed his draft) were simply polite and did not want to frustrate a gifted scholar who, owing to his provenance, lacked the education and methodological sophistication they had, or were inclined to self-criticism and even self-irony. The optical illusion was rather a sort of cultural misunderstanding: probably, Kornai understood both the interest in his iconoclastic research program and the compassion felt for his difficult career as an acceptance of his heavyweight criticism. He might have been misled also by the staunch opposition by Cambridge economists (particularly, that of Miklós Káldor) to general equilibrium theory. Kornai repeated and also anticipated some of Káldor's arguments, therefore, Hahn's malicious review actually may have targeted Káldor while Kornai was the scapegoat. On the relationship between Káldor and Kornai, and the similarities of their research programs, see Mihályi (2017).

100. Bródy also disliked Kornai's doubts about scientific abstraction but did not air his grievances publicly. This is how he remembered later: I could not share "his opinion that there is no fruit but apple, moreover, there is no apple but only a certain kind of apple, and in fact, . . . only this apple here at the bottom of the basket. This meticulousness leads nowhere" (Bródy 1994, 326). See also Pete (1996).

101. Instead of "mainstream economics," he wrote "neoclassical school" in the Hungarian original (Kornai 2005, 195).

102. Browsing through the bibliographies of his major works published before 1989, one finds a few authors such as Jacob Marschak, Roy Radner, and Herbert Simon who might have lured him in this direction. However, most of those experts who later became recognized as the *crème de la crème* of new institutionalism such as James Buchanan, Ronald Coase, and Douglass North were missing in the references. On unexploited opportunities in this regard, see Grosfeld (1992). Kornai also could have returned to Hungarian sources from the 1940s to couple mathematics and institutional analysis (see note 17).

103. He did not dissuade his students and younger colleagues from studying neoclassical authors. On the contrary, he told them to learn to know what they eventually had better not accept (cf. next note). See the discussion between Kornai and J. M. Kovács on these controversial aspects of *Anti-equilibrium* (Laki 2006, 14–17, 28–30). On Kornai's ambiguous impact on the Eastern European reception of mainstream theories in the West, see Vincze (1996), Klaus (1997), and Laki (2006). See also Gács and Köllő (1998), Maskin and Simonovits (2000), Bihari et al. (2018), and Simonovits (2018).

104. Introducing a Hungarian-language volume of Arrow's selected works in 1979, he wrote this: "I still consider the criticism expounded in my book *Anti-equilibrium* legitimate. . . . An economist who is not profoundly familiar with general equilibrium theory cannot be an educated expert mastering the profession seriously. . . . What is needed is not to reject [this theory] arrogantly but to surpass it in a well-prepared, critical and constructive manner" (Kornai 1979, 9–10).

105. In *Shortage* he even admitted that, instead of rejecting the notion of equilibrium as such, he should have only criticized the Walrasian concept of equilibrium (Kornai 1980, 143–47).

106. Here, he claimed that "truly harmonic growth is promoted by clever planning," and in itself the market does not produce harmony but can correct the plan (Kornai 1972a, 141).

107. Cf. Simonovits (2003) on what he calls "the Hungarian school of control theory." He lists Bródy (1973) among its important works. See also Martos (1990).

108. Kornai's cautiousness was reflected by the fact that he resisted the temptation to switch to a description of the planned economy as an overwhelmingly in-kind regime.

109. *Shortage* only contains two partial models built by Simonovits and Weibull on forced substitution as well as on queuing and friction, respectively, in its annex.

110. Nonetheless, in *Shortage* he confronted its members such as Robert Barro, Robert Clower, Herschel Grosmann, David Howard, and Richard Portes with reasonable verbal arguments on aggregate excess demand, household savings, and labor supply in a planned economy (Kornai, 1980, 476–80).

111. For other important counter-arguments, see Davis and Charemza (1989), Gomułka (1985), and Soós (1985).

112. Yet, during the 1980s promising attempts were made to study cases and build models to refine and/or test the principal hypotheses of *Shortage*. See, for example, Kapitány, Kornai, and Szabó (1984), Kornai and Matits (1987a, 1987b), Szabó (1988), Goldfeld and Quandt (1988), and Lackó (1989).

113. This concept motivated by consumer theory in microeconomics was to represent the situation in which a state-owned firm can count on a bail-out by the central planner if the firm's revenues do not cover its costs. Kornai (1986a; 2007, 265–67) regretted that, in 1984, his article on the soft budget constraint had been rejected by *American Economic Review* because of the excessively verbal style of his research project.

114. They include Erik Berglöf, Yingli Qian, Richard Quandt, Gerard Roland, Mark Schaffer, Jörgen Weibull, and Chenggang Xu.

115. For more on this, see Szabó (2015).

116. On the protracted reception of new institutional economics in Eastern Europe, see Kovács (2012).

117. Since the 1960s, he taught and researched at various Western universities from Stanford to Stockholm, and became a professor at Harvard in 1984 but never cut his relations with Hungary, claiming that his research material lay on the Eastern side of the Iron Curtain and admitting that there he always had a chance to rely on excellent mathematicians. An ahistorical question: would he have been more successful in modeling the soft budget constraint if Lipták had still been around?

118. Approaching 1989, Kornai gradually left the terrain of mathematical economics and devoted his time to the study of late communist reforms and the completion of his 1992 book on the *Socialist System*, a synthesis of decades-long research on the planned economy. For more on this, see the next volume of our series.

119. Facing the difficulties was not tantamount to admitting and explaining the failure. Just like Bródy and Kornai, the other former champions of optimal planning in Hungary also have not given a detailed historical account to this date about how and why their project ceased to exist.

120. Tardos (1968) tried to build a formal model for the regulation system of NEM, which was based on the Dorfman-Samuelson-Solow model of linear programming but did not test it by means of detailed calculations. Among those who started working on optimal planning at the turn of the 1950s and 1960s, just a very few (such as András Nagy and Tardos) anchored themselves in reform economics so firmly that,

from the 1970s onward, they stopped building quantitative models. This also meant that their interest in new institutional economics did not result in authentic formal models describing the planned economy undergoing market reforms.

121. Kornai (1986b, 1725–28) accepted some of Hayek's views indirectly, through passing judgement on Lange. It was only in 1991 that, criticizing state-led privatization, Kornai (1992a) referred to the Hayekian stance against "constructivism" approvingly the first time. In his book *The Socialist System* he admitted that "Hayek was right on every point in the debate [on socialist calculation]" (Kornai 1992b, 476).

122. For example, as shown earlier, Kornai (1967a [1975]) was still optimistic about centrally planned investments, and a total abolition of directive planning did not feature even in his writings on market reform during the second half of the 1980s (e.g., Kornai 1986b). Here, he rejected the attraction of "Galbraithian socialists" to large-scale state intervention but avoided to suggest the termination of five-year plans or at least of the gigantic central development programs. In his opinion, "ex-ante coordination" (whatever it may have meant) should have remained an important task of the central planner (1710, 1730–32).

123. For example, Augusztinovics played a leading role in modeling long-term plans for 1970–85, 1975–90, and 1980–2000. These were the least risky types of central plans: they were regarded as futurological visions rather than regular plans that had to be endorsed by the Politburo and fulfilled by the economic actors at all levels of the hierarchy.

124. Bródy's (1978, 180) opinion about state planning in both the East and the West was more than skeptical: planning "can be hardly left to the usual sort of politicians who will promise whatever is popular . . . and have a time horizon much shorter than the horizon considered in an economy-wide plan. . . . A plan is actually conserving the very power structure that gave rise to it."

125. Augusztinovics (2000, 17) was even more mistrustful: "the neoclassical theory does not want to understand but to cover up the reality of the capitalist economic system."

126. As young scholars in the 1950s and 1960s, Bródy and Kornai did empirical research in numerous firms (engineering and textiles, respectively) and gathered ample insider experience also about how the Planning Office and various branch ministries worked.

127. The trauma of the post-1956 retributions had a long afterlife. Kornai was not fully rehabilitated by his return to the Institute of Economics in 1967; the secret police did not stop harassing him from time to time. The last time Bródy had to undergo a disciplinary procedure in the communist party was in 1988. Yet, their fears stemmed increasingly from concerns about losing their jobs and privileges such as relative freedom of thought and travel as well as proximity to top decision-makers in the reformist camp while their worries about violent repression dwindled. Nevertheless, forced emigration (like in the case of some members of the Budapest School in the 1970s) remained a credible threat.

128. For more on this, see Kovács (2012, 2016).

129. For example, Gács and Lackó (1973) was a promising attempt to examine the behavior of central planners (instead of helping them improve planning) but their early initiative was hardly followed by their colleagues.

130. Kornai, for example, abandoned optimal planning, in which he had relied on econometric analyses, continued to apply econometric research in his later works, was active in the Econometric Society, but—as mentioned—insisted on the broadest possible designation and preferred to call himself a mathematical economist. This is how he remembered mathematical economics in Hungary during the 1960s: "we <two-levelers> formed one faction, but other groups emerged as well, such as the <input-outputters,> the econometricians, and the operations researchers. They often overlapped" (Kornai 2007, 153).

131. In the beginning, it was only Ede Theiss who worked in the spirit of this slogan in Hungary. He died at the end of the 1970s.

132. Kornai later became an exception in this regard when his disciples helped him verify the empirical relevance of his concepts.

133. For instance, at the Institute of Economics Mária Lackó investigated investment cycles, Miklós Ördög worked on the estimation of consumption functions, and György Simon on that of sectoral production functions. The example of László Hunyadi is revealing. As a self-taught econometrician, he had worked on planning models until—completing a large project on the impacts of change in energy prices on the Hungarian economy—he realized that the Planning Office ignored such econometric analyses, and in the mid-1980s he decided to move to Karl Marx University to teach instead of struggling for recognition within the economic administration (Hunyadi 2014).

134. Yet, the first article in Hungary on the "Lucas critique" was published by Kamilla Lányi as early as 1977.

135. Interestingly enough, a main motivation of this group came from sociologists (such as László Füstös and Róbert Manchin) who already applied first-generation statistical software (e.g., Socprog) in their empirical surveys, launched by István Kemény, on poverty and ethnicity (Köllő 2021).

136. The attraction of neoclassical concepts for certain econometricians survived Kornai's attack on general equilibrium theory. For example, when Ziermann (1977) reported on the annual meeting of the Econometric Society she presented not only the new results in times series analysis (to which she also contributed in the field of dynamic factor analysis) and multivariate regression but also in research on Pareto efficiency and decentralized allocation mechanisms.

137. Interestingly, the inertia was not overcome by some of promising international ventures (such as the LINK project, in which András Simon represented Hungary and the IIASA (International Institute for Applied Systems Analysis) where Ernő Zalai spent years in the first half of the 1980s) since these research communities were more pluralistic and did not exclusively favor the idea of general equilibrium.

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