

János Kornai
By Force of Thought

Irregular Memoirs of an Intellectual Journey

Extracts

Téma

The Fifties: Waking up

Late in the summer of 1954, I was staying in a resort facility at Lake Balaton when I met Sándor Haraszti, who had just been released after several years in prison. He had been a Communist back in Horthy's time; and after 1945, he had become editor in chief of the Communist newspaper, *Szabadság* (Freedom). We would sometimes meet, and I had respectful feelings of friendship for him. He was the father-in-law of Géza Losonczy, who was to die tragically as a martyr of the 1956 Revolution. Losonczy had been one of the heads of the Party daily *Szabad Nép* when I started on the paper. Our acquaintance was superficial, but he was generally known as an old and respected Party member. Haraszti had been arrested in 1950 and Losonczy in 1951. It emerged later that Rákosi and his associates had wanted to include them in a public trial in which János Kádár would have been the main figure accused. That second public trial after Rajk's never took place, but the men were kept in prison until released by the advent of the New Course.

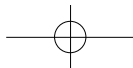
My meeting with Sándor Haraszti had undermined the moral foundations on which I had hitherto based my Communist convictions. The Party had lied in

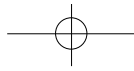
János Kornai

is Allie S. Freed, Professor of Economics Emeritus at Harvard University. After his appointment in 1986 as Professor of Economics at Harvard University until his retirement in 2002 he divided his time between Cambridge, Massachusetts and Budapest, Hungary, where he was a research professor at the Institute of Economics of the Hungarian Academy of Sciences till 1992, when he became an emeritus fellow of Collegium Budapest, Institute for Advanced Study. His books include Overcentralization of Economic Administration (1956); Anti-Equilibrium (1971); Economic of Shortage, perhaps his most influential work (1980); The Socialist System. The Political Economy of Communism (1992); The Road to a Free Economy (1990); Highway and Byways (1995); Struggle and Hope (1997) and, most recently, Welfare in Transition (2001), co-authored by Karen Eggleston. The above excerpts appear by arrangement with MIT Press Cambridge, Massachusetts and London, England, who will publish the book later this year.

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Memoir





saying that Haraszti was guilty, and I had believed the lie. If that trial had been based on a lie, then the others must have been as well. We were surrounded by lies and I, stupidly, had believed them without question. And, willy-nilly, I had been spreading some lies myself.

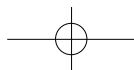
The change in my ideas began on a meta-rational, not a rational, plane. My *faith* in communism was shaken as I recognized the lies and brutality around me. The collapse was in the moral foundations of my worldview.

If that, to use a geological metaphor, was the deepest layer, there was another above it, in the rational range: the *epistemological* basis of Marxism. Marxism calls itself the theory of *scientific* socialism. It dissociates itself from unscientific versions of socialist ideas, branding them naive and utopian. According to its own assertions, Marxism alone presents a scientific method for researching society and comprehending the body of knowledge about it.

I broke with Marxism because I became convinced that it lacked foundations in precisely this respect. I am aware that I am on shaky ground when I start expounding on scientific theory and methodology. There is no agreement among philosophers who specialize in the subject on what makes a statement 'scientific' or even on when the truth of a statement can be deemed to be confirmed. Nor is it the purpose of this intellectual autobiography to try to decide such matters. All I am trying to do is to present my personal history.

Up until 1955, in my eyes the closed character of the Marxian intellectual edifice and its transparent logical structure offered sufficient evidence for saying it was not only closed and logical, but *true* as well. When I started to internally revise this theoretical conviction, full of disillusionment and suspicion as I was, I began with increasing decisiveness to take another approach—to compare the theory with reality. Its importance was enhanced by my bitter experience of being deceived. How did the "theory of value" relate to real prices? How did the theory of "pauperization" square with historical tendencies in living standards? How did the "theory of capitalist crises" reflect the changes in business cycles in real life? How did the theory of "classes" and "class warfare" compare with the actual stratification of society and social conflicts? The problem was not just that the theories performed badly in all these comparisons, that the Marxist dogmas failed to match reality. The main trouble was that Marx himself and his later disciples did not feel the primary intellectual duty to apply the elementary criterion of scholarship: testing their ideas against reality. Marxism is not the only school in the social sciences to commit this original sin, but in 1955–1956 I was calling on Marxism to meet the fundamental requirement of science and compare theory with the real world.

I started to see why the allocation of resources works badly in a socialist system. The function of prices in such an economy is almost impossible to explain. Marx certainly never undertook to say in advance quite what has to be done in a socialist system, but it should at least have been shown in his writings what happens under capitalism. Simply posing the question sufficed to make it clear to



me that Marx had failed to provide the answer. His works contain frequent references to prices being set by "competition." But how? Volume 1 of *Capital* floats in the air. Its main contention, that work is the sole creator of value, epitomizes a proposition that cannot be tested or refuted—in other words that is nonscientific. Nor is there any way to derive by strict deduction from the main proposition in volume 1 the system of ideas in volume 3, where surplus value is "converted" into average profit proportionate to capital. Here some testable, refutable propositions appear but they perform poorly when confronted with reality. What was putatively the explanatory theory of profit explains very poorly what factors actually generate profit in capitalist reality. In a word, I concluded that Marx's theory of value was inapplicable to reality.

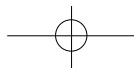
The other Marxist economic propositions have been deposited onto the layers discussed so far. Let us take just one as an example: the assertion about *pauperization of the working class*. It does not follow in a deductive way from any previous statement. If one accepts Marx's theory of value, the theories about average profit and production price and all other auxiliary propositions in *Capital*, it is equally consistent with them to say that the living standard of manual workers declines, stagnates or rises relatively (compared to that of the other groups) or absolutely (considering changes over the long term). As for empirical verification, history has sharply refuted the Marxist tenet about pauperization as a long-term trend. Statistics clearly confirm that the material consumption of working people in all countries operating under a capitalist system rose substantially within one or two centuries and their living conditions improved.

At some point, around the end of 1955, I gave up Marxism. I announced, first to myself, that I was not a Marxist any longer. I would not reject every one of the theory's methods or statements, but I would reject the '-ism' as such, the Marxian intellectual edifice. Sometimes among close friends I would describe my intellectual state by saying I had 'written off' Marxism.*

"That is not be chance, Comrades," was the trite phrase that often began a statement in those days. Those conversant with Marxian philosophy would say that Marx's works never reflected extreme fatalism or a belief that events were predestined. But in daily life under the socialist system, party secretaries and journalists, history teachers and planning office department heads liked to imply there was *no alternative*. The only thing to do was what the historic forces of progress dictated. And those dictates normally coincided with their instructions. There was no other option but to collectivize agriculture. The economic plan was drawn up in a single 'version' that every organization, though formally entitled to accept it or reject it, had to endorse. There was only one party, and that one had to be elected.

During the second half of the 1950s, the opposite idea came at me in many forms and from many sources: there *was* a choice.

* ■ I broke *radically* from Marxist theory and ideology. Yet I went on believing for quite a while that socialism could be reformed. Later, I gradually gave up being a 'naive reformer'.



About that time, I came across the existentialist philosophers. The first work I read was a short piece by Sartre. Then I studied others. My reading of these too (perhaps because that was what I wanted to read into them) was that if there is no God, people are free and obliged to choose. There are no desperate situations in which no choice remains and the responsibility for deciding can be avoided. For me, having had it drummed into me that “the Party will decide,” it was vital to understand that I was responsible for my decisions and I could not blame circumstances.

By then, the model of rational choice had become the main conceptual framework of my thinking as an economist as well. A later chapter deals in detail with criticism of the theory. At this point, however, I want to emphasize one great virtue the model has. Its underlying structure suggests that there is a choice. When it is used for purposes of positive analysis, we must establish retrospectively, even against the facts, what possible but rejected alternatives we had in the past. When it is put to normative use, we must determine just what restrictions independent of ourselves are on our choices. There is freedom of choice within this restricted set of alternatives.

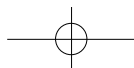
Let me try to sum up where I stood in my choices around 1959. Some people may switch to a new career in a single dramatic turn. It took me some years, from 1954 to 1959, to work out how I wanted to live in the future. My basic decisions had been taking shape through a series of conscious deliberations and improvisations, intertwined with each other. At every moment the room for potential choices was narrowed by pressures from the outside world. There was choice available in each moment. Nevertheless—in retrospect—it could be stated that by 1959 some of my basic decisions had already emerged. I emphasize five of them here.

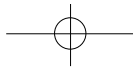
- 1) I would break with the Communist Party.
- 2) I would not emigrate.
- 3) My vocation would be research, not politics. I would not indulge in heroic, illegal forms of struggle against the Communist system. I wanted to contribute to renewal through my scholarly activity.
- 4) I would break with Marxism.
- 5) I would learn the basics of modern economics. I wanted my studies and researches to be part of the Western profession of economics.

The Sixties: The economic application of mathematical methods

As I progressed with my studies, it became clear to me that I would have to study the application of mathematical methods if I wanted to do economic research that matched the standards of the period.

As part of the empirical work on the post-1957 transformation in the methods of economic administration in light industry, I began to be interested in the role of enterprise profits. Western writers considered it self-evident that the motivation of the decision-makers in a firm was linked to the firm’s maximizing its profits.





Well, attempts were made in Hungary after 1957 to give a profit incentive to managers, and even to all workers in light industrial enterprises. Various bonuses were attached to profits, but in a curious way. Profit distributions depended on whether profitability (as a proportion of sales) had improved over a specific level. Managers were encouraged to maximize (in mathematical terms) not an *absolute sum*—the enterprise's profits—but a *quotient*, the ratio between profits and sales. Although many thought that the two kinds of incentive came to the same thing, I saw that they would have different economic effects.

I began to formulate in mathematical form the two types of maximum criteria and the programming tasks associated with them. I cobbled together a model, but I was not satisfied with it.

That is when I came to know Tamás Lipták. Our working relations became ever closer and were augmented by personal ties. It soon emerged that we shared political views as well. Let us not forget it was 1957. Members of the intelligentsia could not keep up an intimate friendship with anyone whom they had political cause to fear.

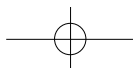
He was an implausibly thin young man (and remained thin later in life). He had a handsome face, a kind voice, and a bright way of speaking that won people over in seconds. His appearance was far from an advantage to him, but women adored him.

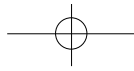
Lipták had an exceptional talent for mathematics. Perhaps it is no exaggeration to say he had genius. Not only did he make use of his huge accumulated knowledge with absolute certainty, but he could bring up unexpectedly some theorem or method he had previously read, just when it was needed. He was an original, innovative thinker.

I learned much from Lipták, who was my private tutor in mathematics for several years. Rather than following a methodical course, he would bring up a subject that was apposite at that juncture. He would recommend reading matter and give me help in following it. Apart from the knowledge I gained with his assistance, I learned a lot from him about how to address a problem. He never tried to hide what abstract assumptions a model was based on or what simplifications of reality it employed. On the contrary, he wanted to present the simplifications in the model with maximum intellectual sincerity.

Let us turn to the economic issue that prompted me to seek out Lipták in 1957. It soon emerged that the problem was hard to handle mathematically if accuracy was insisted on, rather than simplifying matters until the complications caused by reality disappeared. We had to analyze a specific type of nonlinear programming task. We went at it repeatedly. The chapters of the study were written in a dozen versions, each after protracted head-scratching and numerous joint sessions of many hours. Finally, a text of some 250 pages was ready and seemed acceptable to us both.

Then our work was broken off. Tamás was arrested. Sándor Fekete's *Hungaricus* pamphlet whose journey abroad I had aided, was previously distributed in duplicated form in intellectual circles in Hungary. Tamás had helped by working the duplicator. We had not yet known each other when we both did our bit in that





action. Later, as we began our struggle on the scholarly and not the political front, this political past was something that we found we shared. Tamás's arrest did not come as a surprise to us.

He was well down on the list of the accused. He spent about a year in pretrial detention and then in prison. Once, I wrote out a couple of mathematical problems having to do with our joint research and gave them to his wife, Manyi, to see if she could hand them over to Tamás, for him to think about in his cell. I did not think the problems themselves were urgent, but I thought they might help Tamás, by diverting his attention from prison life.* Poor Tamás, far from thinking about mathematical or economics problems, attempted suicide. It was a catastrophic sign of depression, which would overcome him later. Luckily, his life was saved.

Tamás was still in prison when I was dismissed from the Institute of Economics. I sought help from the Ministry of Light Industry in publishing our book-length manuscript. The ministry was prepared to shoulder the printing costs, but insisted the name of the imprisoned Tamás Lipták could not appear on it. I sought the advice of Alfréd Rényi, Tamás's superior and fatherly friend, and agreed with him that the formula "With the cooperation of the Institute of Mathematics of the Hungarian Academy of Sciences" should appear on the title page instead of Tamás's name. In the end, the study came out in 1959 as a book with a primitive appearance, using duplication.

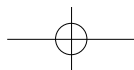
Once Tamás was out of prison, we decided to publish our results in the West. We prepared an article of journal length in English.** Tamás was in his element. I had just started to get to know the style and formal requirements of mathematical and economics journals in the West, but Tamás had been reading them for some years. The content of the article was our joint intellectual product, but its mathematical precision and its Western structure and style were all to Tamás's credit.

At this time and for a long time afterward, no writer of a scientific or scholarly work was supposed to send it to the West without first obtaining permission. The usual procedure was for the researcher, who in most cases would be an employee

* ■ The idea was not completely stupid. It turned out later that some of the 1956 prisoners learned foreign languages and did translations or were writers and tried to compose literary works.

** ■ The Hungarian text was translated as "A Mathematical Investigation of Some Economic Effects of Profit Sharing in Socialist Firms," by (Baron) József Hatvany, whose earlier life deserves a whole novel, not just a footnote. A member of the extremely wealthy, celebrated Hatvany family, he was a nephew of Lajos Hatvany, one of those who had promoted the superlative poets Endre Ady and Attila József; his uncle gave generous support to these two literary giants and many other writers and was an enthusiastic organizing force in Hungarian literature.

József Hatvany had read physics at Cambridge and learned to speak and write English like a native. He had become a Communist in England, and after the war, he felt obliged to return to socialist Hungary, only to be imprisoned as an ostensible British spy. For what else, it was argued, could have induced this enthusiastic young Communist to leave the comforts of academic life in England and a rich, noble family to return home? He was among those released in 1954, but imprisoned again after 1956. After his second release, he earned his living as a translator for a time, seeking worthwhile work that would interest him, as well as earn him money. That is how I came to ask him translate several works of mine. Hatvany later went back to academic life and became one of the intellectual forces behind computer technology research in Hungary, gaining international fame and appreciation in that field.



of a research institute or a university, to present the article to his or her official superior. It would then be passed to that person's superior. If it was politically problematic, it would be sent to the party headquarters as well.* The decision would then be made at one level or another whether it could be submitted for publication in the West.

Lipták and I decided not to apply for any such permit. We simply put the article in an envelope and mailed it. Those who had been accused, among other things, of sending the work of Sándor Fekete abroad illegally were still in prison at the time. Both Lipták and I had been embroiled in that affair. Even if the article we were now sending was free of politics, we were doing something illegal. We both felt it was important not to submit ourselves to a prescribed official procedure. For me, the move was a precedent that I followed thereafter with all my publications abroad. I did not ask for my superiors' permission but simply sent the work straight to a journal or publisher. In this, I did something different from many colleagues in Hungary and other socialist countries, who later complained they had not received authorization to publish abroad. I followed the example of Hašek's Good Soldier Švejk under the Austro-Hungarian Empire: do not ask, for if you do, the answer is going to be no.

We submitted our article to *Econometrica*, the leading journal of mathematical economics. Several years later, I heard that it had come into the hands of Edmond Malinvaud, the journal's French co-editor, who immediately accepted it without changing a word or a comma.** It appeared in January 1962.

The article dealt with a very specific problem, since it analyzed incentives for firms that were used exclusively in Hungary. It underlined that we were examining a *socialist* economy. Even posing the question was an intellectual challenge:

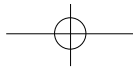
■ The incentive is not determined in advance. Rather than arising naturally or spontaneously out of property relations or institutional conditions, it is decided on a case-by-case basis. This approach was in fact a forerunner of a later line of research—the major body of literature on incentives and the relation between principal and agent.

■ The prices are not determined in advance either. Rather than their being set by the market, there is central price control. The article analyses the rich system of relations between enterprise incentives and prices on the one hand and quantity and composition of production on the other. Which incentive leads to production that is under or over capacity? In which direction does it push the range of output?

I often used to hear Eastern European economists say that they have not published in Western journals because the editors were not interested in their

* ■ As I write in 2004 and 2005 a book containing politically sensitive statements is going through the publication process in China. The first publisher to accept it found the work problematic and sent it to Chinese party headquarters for approval. Publication was not permitted the first time. A second publisher has taken up the task, and permission has been refused again.

** ■ Much later, when I became familiar with the editing and reviewing practices of leading Western journals—such as rejecting most manuscripts and sending others back for repeated revision—I appreciated better what a tribute that immediate acceptance without changes had been.



economy. My experience was the opposite, with that study in *Econometrica* and with my later publications. They attracted interest *precisely because* they had been written by an author living in a socialist country and were reporting on a world distant from that of the editors—but in their idiom, the language of modern economics.

A blighted attempt at a 'fabricated trial'

As I write this, I have at hand a photocopy of a letter written to me on October 14, 1964, by John Michael Montias, an American economics professor at Yale University. He was planning to come to Hungary on a scholarship and requested some advice on his work. He also mentioned that he had begun to learn Hungarian.

The letter had a short history behind it. I first learned about Montias's work from his writings, and then I met him in person in Budapest in 1963, where he was taking part in a conference on mathematical economics. Montias attracted the attention of all the participants at the conference when he volunteered to do a simultaneous translation from Russian into English of a presentation by Leonid Kantorovich. He also interpreted the ensuing debate in both directions. We afterward met once more in Venice in 1965 at a conference of Western and Eastern economists specialising in the study of the Soviet Union and Eastern Europe.

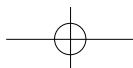
I found the letter addressed to me not in my own collection of letters but in the secret service archives. An American professor's personal letter to me had been opened and photocopied in Budapest, then sealed again and delivered to me by the Hungarian post office. Everybody suspected that things like that often happened, but it was still a strange feeling to have firsthand evidence of it.

At the time, I only saw bits of the 'Montias case.' But I have managed now to reconstruct more or less the full story from the police records.

Montias was an outstanding figure among American Sovietologists. Most of his colleagues knew only one of the many languages spoken in the Soviet region, whereas Montias spoke several and was able to read even more. Although most Sovietologists in those days were qualified economists and had extensive knowledge of the political and economic situation in the Soviet Union or certain Eastern European countries, they were not skilled in the theories of modern economics written in formal mathematical language. Montias, in contrast, belonged to a new generation that handled such modern tools with great skill.

I was happy to help Montias prepare for his trip to Budapest. I gave him some advice on his choice of topic and offered to introduce him to Hungarian colleagues. We exchanged letters several times. Montias handed in a regular application, in which he described his scientific program and Hungarian connections. My name was among those in the relevant part of the application form.

While in the foreground everything was proceeding according to the rules and practice of international scientific relations, behind the scenes the secret police



continued to work at full throttle. The first warning came from Czechoslovakia: there was reason to believe that Montias was a CIA agent.*

As the date of the visit approached, more and more people became involved in the case. They fished out the 'material' they had on the Hungarian scholars whom Montias had named in his application. There were some other names in the files as well, but what I managed to find out from the archives was what actions were taken concerning me.

Counterespionage officials must have thought they were going to make a big catch!

My files in the various branch offices of the III/III Department for the period between 1956 and 1959 were retrieved, as well as those for my trips to England in 1963 and 1964. Then Captain Z. Z. wrote a summary report taking stock of everything they had against me.

While studying the written documents of the secret service, I came across transcripts of tapped telephone conversations dating from 1963. A list was drawn up with the names of everyone I had contacts with. This produced nothing of interest for them. The list contained the names of friends in Budapest, relatives, and economist colleagues. Here is a quote from one of the scripts: "Helga? Olga? Paid Kornai money." Here the police agents used a Hungarian slang term, applied when, for example, a man is paying money to his mistress for her services. Very suspicious. Kornai is being paid by a woman... As a matter of fact, it was a dear acquaintance of long standing named Elga, to whom I had lent some money during the summer vacation.

There is a comment on Captain Z. Z.'s report, handwritten by his boss, advising him not to use me for 'throwing under'. I consulted experts to find out what that meant. The secret service had to find an agent who was, in secret service terms, reliable and obedient, and who could gain the confidence of the person under surveillance. Such an agent was said to be "thrown under" the person under surveillance, and would in turn provide the secret service with useful information.

Let us return to the visible surface. Montias's application was accepted by the Hungarian cultural relations bodies and he was granted a visa. So Montias arrived. He met and talked to several Hungarian economists. He started to learn Hungarian. He lived the usual life of foreign visitors, went to the opera with his wife, and sometimes ate out with Hungarian colleagues. We wined and dined them, too.

It transpires from the files that he was shadowed all along. His phone was tapped and he was followed about in the streets. I read the observers' reports: absolutely nothing comes out of the piles of pages. We lived on Pusztaszeri út, and so instead of referring to us as Kornai and Laky (my wife), the report called us

* ■ Documents recently obtained reveal just how closely the secret services of the Communist countries cooperated. The Czechoslovak state security bureau conducted a secret search in 1963 of Montias's apartment in Czechoslovakia, and found in his coat a slip of paper with my name and home telephone number on it. This they reported to their Hungarian colleagues (IH 1656. 2/2-2358, p. 3. Date: May 8, 1964. Also IH 34-4-797/1965, p. 4. Date: April 23, 1965).

"Mr and Mrs Puszta." Montias could certainly not appear in the confidential internal reports as Montias. Instead, he was given the name "Zimelio" (and in the street observation reports, he features as "Master," for a change).

The plan took shape. Montias would be accused of obtaining—by misusing his position as a visiting research scholar—classified information about the Hungarian economy and Comecon (the Council for Mutual Economic Assistance). The Hungarian economists with whom Montias was in touch were interrogated.

I was interrogated too. I have recently reread the reports. None of the witnesses made any accusations against Montias. None of them confirmed the suspicion that Montias had collected classified information.

Before long, Montias was expelled from Hungary, effective immediately. An article was published in a newspaper explaining Montias's expulsion. He was accused of gathering secret information, of spying. This accusation, however, could not be supported by facts, testimony, or any other evidence. There was not going to be a spectacular espionage trial.

The story is remarkable in many ways. It is a case that illustrates that the world of scholarship was not a privileged or protected area. The tentacles of the totalitarian state reached deep into it, and it was observed and terrorized.

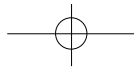
While there was continuity between the Rákosi and Kádár eras, there were some essential changes as well. In the days of the old ÁVH, all the players would have been arrested—the American professor and his Hungarian friends—and tortured until they confessed and it could be confirmed in court that they had been spying for the CIA all along. Even then, in 1965, the interrogations were depressing and very trying for the Hungarian witnesses. But we did have the chance to refute the false accusations.*

At the time of the Montias case, there were two opposite political trends on the Hungarian political scene. The "soft-liner" or reform-oriented forces were out to establish friendly relations with the West, especially in culture and science; the 'hard-liner' or anti-reform forces, however, tried to seize every opportunity to blight East–West relations. Uncovering an American agent would have come handy to the hard-liners, but the plan turned out to be a failure in the end.

Two postscripts need to be added to the story.

In 1970, I spent six months at Yale University. There Mike and I met and talked a lot. We did not discuss the story of his expulsion. He—rightly—felt it proper not to bring it up, as he was aware that I would be returning to Hungary. Later, we would meet each time I visited Yale. I even gave a talk at the institute he headed. Montias would have liked to visit Hungary again in the 1970s, but the Hungarian

* ■ I have written in an earlier chapter that after 1956, I resolved to become a member of the Western economic profession. I was well aware of the risks involved. Still fresh in our memory were the times when the mere fact of having a 'Western connection' was enough to make one suspect, and when innocent professional interaction would be classified as 'spying' in trumped-up criminal proceedings. This nightmare haunted us for as long as the Communist system existed. We know now, *in retrospect*, of course, that there was no Stalinist restoration. But nobody then, *in advance*, could say for sure that such an attempt at restoration would necessarily fail.



authorities did not grant him permission to enter the country. His name was not removed from Hungary's *persona non grata* list until the final hours of the old regime in 1989.

Unfortunately, his Hungarian adventure rather dampened Montias's enthusiasm for 'Sovietology.' This was a real loss to the profession. He gradually turned to art history. His books on seventeenth-century Dutch painting are widely regarded by art historians as classics in the subject.

I have to finish this story with a sad report: Mike died recently, in 2005.

The other postscript relates to a Budapest experience of mine. When I first asked to look into the files of the Montias case in 1998, I was granted permission—with a number of restrictions, however. I was not allowed then to make photocopies of the documents. While reading the files, someone from the office was to be present. Perhaps he was told to keep an eye on me, lest I try to sneak out one of the files secretly. He was a pleasant man. After I had finished reading, he started to chat, saying, among others things, something to the effect of "Well, yes, we never managed to catch the man." I could not quote him word for word. I did not tape the conversation so as to be able to report his words verbatim. This much is certain: he made this remark as someone who fully identified with the counterintelligence people working on the case, in the first-person plural, ironically and with some tone of regret in his voice. A weird continuity.

The Seventies: How I became an academician

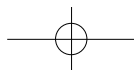
The Hungarian Academy of Sciences, following the Soviet pattern, had several functions. I was its paid employee, since I worked at one of its institutes. It employed several tens of thousands of people.

At the same time, the Hungarian Academy of Sciences also had the traditional function of a national academy, declaring itself the body of the best scholars in the country. But how were they judged to be the best? The qualifying procedure prescribed by the statutes of the Academy resembled that of the great Western academies. After careful prior assessment the members of the Academy would elect new members by majority decision in a secret ballot.* But it is worth considering more closely how the election process went in practice. This can be followed through my own example.

Scrutiny of the Academy's charter at that time reveals that its members were expected to have "the most progressive worldview";** in other words, a member must be a Marxist-Leninist. That was a troublesome criterion for those recom-

* ■ Membership has two grades: corresponding membership and full membership. Obtaining the lower grade was the big hurdle; a strong argument for the nomination had to be made by a full member. The next step was easy. Corresponding members became full members almost automatically after a time.

** ■ Section (1) of paragraph 1 of the statutes of the Hungarian Academy of Sciences prescribed that the Academy should pursue its activity "on the basis of the scientific worldview of dialectic materialism."



mending me as an academician.* When the question of my membership was first raised in the early 1970s, I had already gained a reputation at home and even abroad. Perhaps more awkward still, the American Academy of Arts and Sciences had elected me an honorary foreign member and I had had other marks of international recognition as well. The Kádár regime, at that time, was anxious to demonstrate how civilized, cultured, and Western intellectual life in Hungary was. How did it look if a Hungarian researcher was a member of the American Academy but not of the Hungarian Academy? Consideration of my membership could not be postponed any longer.

Most of the academicians were party members. It was generally known that the 'official' nomination meetings of the sections were preceded by the meetings of their party members; there they agreed on which of the possible candidates to vote for and against. Since the Party members formed a majority of the committee, they ultimately decided who was going to be a member.

My nomination was defeated at the first attempt, during the preparation for the election of members in 1973, right at the first phase—the meeting of the Academy's party members. From what I have gathered from the archival material now available, the relevant department of the party headquarters prepared a report for the Politburo, the supreme decision-making body of the party, on the preparations for the 1973 General Assembly of the Academy. This contained an account of whose nominations the party members supported and whose they rejected. The Political Committee considered my nomination and "did not support" (i.e., forbade) it.

The following election was scheduled at the 1976 General Assembly. The usual preparatory procedure took place. This time only a thin majority opposed my Academy membership at the party members' meeting.

As usual, the relevant department of the Party headquarters submitted its report on the preparations for the Academy's 1976 General Assembly in October 1975, dealing, among other things, with my case. The report refers to the Politburo's negative decision two years before and adds the following comment: "The situation has, in essence, not changed." This time the Politburo did not decide immediately on the nominations to the Academy, but resolved to return to the question later.

After that, the story took an unexpected turn. Not much later, the meeting of the Academy's section of economics and law (Section IX of the Academy of Sciences) was held; there all the members of the section, not just the party members, took part. This body 'decides' *de jure* on the nominations. On paper, I could not have attained a majority in the secret ballot if every party member academician had voted in accordance with their previous decision—that is, if each had voted against me. To general surprise, my candidacy was approved by a large majority. Clearly, party members who had not spoken up in support of my

* ■ Some individuals 'volunteered' for membership, lobbying academicians for recommendations and trying to persuade other academicians to vote for them. Others felt such self-advertisement to be unworthy and trusted the choice to their colleagues' professional conscience. I was among the latter, in this case and with other honours too. Those proposing me as a member in the 1970s did so on their own initiative, according to their own judgment.

candidacy at their own meeting nevertheless had flouted party discipline under cover of the secret ballot and voted for me after all. Apart from me, another economist (with whom the party had no political problem) received the necessary majority.

The situation was complicated, however: two more economists, whose candidacy the party headquarters had been promoting, received exactly 50 percent in the secret ballot. According to the letter of the Academy statutes, their names could not go forward, as they had not received a majority.

The functionaries responsible for Academy affairs found this outcome an embarrassment. There was Kornai, politically suspect, gaining a majority, while two reliable people of theirs did not. It was too grave a problem to resolve in the formal venue of the Academy or even at the middle level of party management. The matter had to go up to the country's supreme political body, the Politburo of the Communist Party.

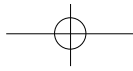
In January 1976, the Politburo again discussed the issue of nominations. A new submission was prepared, and in the debate, the submitter of the report stated that they had inquired widely about my work; this time, the opinions had been favourable. Finally, János Kádár took the following view (quoted word for word from the minutes): "As far as these changes are concerned, I believe Kornai is acceptable, because the political aspect is not so relevant. It is difficult to compare, but we are not dealing with party functions or party membership, but with the Academy of Sciences, where it is possible that some people with minor political problems will, if their scholarly work is otherwise positive, become productive and useful members of the Academy. So his not meeting the ideological rigour of old party members is not a reason to exclude him."

With that, the issue was decided. My membership in the Academy could now follow the regular course.

Yet a need for balance was still felt at party headquarters. The Academy statutes were bent and the two names with precisely 50 percent approval were also put forward at the General Assembly. With three reliable new people joining the Academy along with Kornai, the required balance would be ensured. And that is what happened: four new economists became corresponding members in 1976.

That is how the autonomy of the Academy was maintained in the 1970s, with sovereign secret voting and valid statutes. In the course of selection, the assessment of genuine scholarly performance was mixed, as in every sphere of life, with the Communist Party's desire to wield power. Selection according to political criteria left a marked impression on the composition of the body of the Academy, with consequences that are felt to this day.

As well as whittling down the Academy's independence, the incident also illustrates another important phenomenon: the willingness to make concessions typical of the cultural policy of Kádár and György Aczél. Kádár and his men did not recoil from making occasional compromises. My 'admission' to the Academy was one such compromise, if some recalcitrant party members, taking refuge behind the secret ballot, so wished. At the same time, they wanted to ensure that the



leading positions remained in their hands; if such a compromise was the formal condition of their exercising their political will within an 'autonomous' organization, those faithfully following the Party line should be the majority.

Building a new home

In 1974 a condominium for ourselves and four other families was completed. Legally, this type of undertaking was known as 'self-built construction,' which meant that there was no outside contractor to coordinate all the necessary activities. The future occupants themselves hired the people to do each task, either tradesmen with official permits or moonlighters from the 'grey' economy. They also had to procure most of the materials and equipment for themselves.

As the work ground on, my wife and I had increasingly become the unwitting managers of the work. We found ourselves up against the reality that it was nearly impossible to obtain the building materials required. Finding clinker bricks or bathroom tiles meant long searches and, if need be, painful reductions in our quality requirements.* We passed all the Stations of the Cross endured by consumers in a shortage economy, which I later put into a systematic order in *Economics of Shortage*. In each case, we had to choose between searching, waiting, making forced substitutions imposed by chronic shortages, or giving up our buying intentions. Apart from the product shortages, we came up against labour shortages as now one and now another skilled tradesman failed to turn up and thereby stalled the whole operation. We had to realize that shortage leads to corruption. We learned how much to give to the warehouseman at each factory to obtain the missing material and what brand of brandy it was appropriate to present to the official at the district council who issued the permits**

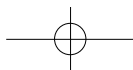
I visited the West quite often in those years, and on each occasion, I took a shopping list with me. Not a list of things that were cheaper in the West than in Hungary, or of what local specialities were worth buying, but a list of what could not be had at home and could be found in a normally functioning market economy.

An experience I had in Moscow seemed to emblemize the East–West difference in buyer–seller relations. Taxis stand in lines outside airports in the West. Passengers hop in and say where they want to go. Taxis pulled up intermittently in Moscow, to be besieged by passengers asking where the taxi was going. Those wanting to go where the taxi driver fancied going got in. Who should choose the destination: passenger or taxi driver?

Everyone constantly had similar experiences. Whomever I spoke to, from research workers to cleaners, from company managers to drivers, would be full of

* ■ I had to draw on connections in Szolnok to obtain a bathtub. The one found was slightly faulty, which we brought 100 km to Budapest.

** ■ János Kenedi published a little diary-like book about his similar experiences of building privately. He wittily chose as his title an old slogan of Mátyás Rákosi's: "The country is yours, you build it for yourself." The book was printed in the series *Magyar Füzetek Könyvei* (Hungarian Pamphlet Books) published by a group of Hungarian émigrés in Paris.



tales of annoyances and trials related to the shortage economy. All of these had settled in me, and they emerged when I set about writing a book about shortage.

The Eighties: Moving to Cambridge

It was felt in the economics department at Harvard some time in 1983–1984 that they needed a specialist in the Communist economies. The search may have gained urgency because Abram Bergson was preparing to retire. He was the most prestigious American scholar in Sovietology and, apart from that, made an important contribution to the theory of welfare economics with the introduction of the social welfare function named after him (the Bergson-Samuelson function). At some point in the search process, my name came up. I was personally known well by colleagues at several American universities. Some I could count as friends, but I happened not to have close relations with anyone at Harvard, apart from meeting in passing with a professor or two. So they wanted to get to know me.

I was invited first to give a lecture, followed by dinner and a professional discussion over the tablecloth. Next came an invitation to fill the prestigious Taussig guest professorship for 1984–1985. I was to move there and the department would find me accommodation. I would teach just one course and spend the rest of the time on research.

At the age of fifty-six I had to start from scratch. The main subject on which I lectured was titled “The Political Economy of the Socialist System.” My teaching was addressed exclusively to graduate students in programs leading to a master’s degree or a doctorate. Some Western textbooks touched on the subject and I assigned occasional chapters as recommended reading, but I wanted to present the subject in my own way. I had never attended any other professor’s lectures on the same subject, so I could not know how others were doing what I had to do. I made use of the huge published literature, of course, but ultimately, I shaped all I wanted to say and all the material I wanted to present, from the first sentence to the last. Furthermore, mine was not a settled, developed subdiscipline such as standard microeconomics or macroeconomics. There the reality defined by the theory is relatively fixed, and changes from year to year are almost imperceptible. In contrast, talking about the socialist economy was like shooting at a moving target. I started teaching at Harvard in the mid-1980s, by which time the Communist camp was in ferment and one event of world-historical importance was followed by another. In the Soviet Union it was the time of glasnost and perestroika. In Beijing’s Tiananmen Square millions demonstrated—and the protest was followed by bloody reprisals.* And finally, in 1989 the Berlin Wall was falling. I had to recast the course as “The Political Economy of Socialism and the Post-Socialist Transition.”

* ■ During the dramatic occurrences in Beijing, my wife and I were practically glued to the television, watching live the eruption—and suppression—of the Chinese student movement. Anybody who had lived through 1956 in Hungary could see many similarities. We had deep sympathy for my Chinese students and shared their apprehension as the events unfolded.

Although my lectures differed from others in spirit, that was by no means unusual. Harvard University (and many other institutions of American higher education) strove expressly for diversity. Students could choose from a range of intellectual strands, philosophies, and scientific schools of thought.

Amartya Sen divided his time between the economics and philosophy departments and for many years ran a philosophy seminar with Robert Nozick. When Nozick's *Anarchy, State and Utopia* appeared, he was viewed as a brilliant new star in the firmament of libertarianism.* Sen was at the opposite end of the political spectrum. He researched problems of poverty and famine and called unconditionally for the state to play a redistributive role. The difference in their views never disturbed their sincere friendship and it gave their joint seminar its spark of excitement.

There were various schools of economic thought within our department and representatives of political views that not merely differed but were bitter rivals. Among the faculty were Steve Marglin, a radical left-wing economist, and a few old-style Keynesians. Another member was Robert Barro, who engaged in theoretical research while contributing regularly to the conservative *Wall Street Journal* on daily policy issues. Certain professors served in presidents' administrations for longer or shorter periods, before returning to the university. By the time I arrived, John Kenneth Galbraith was over 80 and retired, but he still came into the department periodically. (We also saw his erect, lanky figure regularly at the swimming pool.) Apart from the enormous reputation he had gained with his books, he had belonged to the circles of Presidents Kennedy and Johnson and served as U.S. ambassador to India. Martin Feldstein coordinated the massive organizational and educational task of providing undergraduate teaching in economics on a mass scale. He belonged to the other political hemisphere, having served President Reagan as senior adviser for several years. Another colleague was Larry Summers, until he left teaching for public service as vice president of the World Bank. He was later deputy secretary, then secretary of the treasury in the Clinton administration. Thereafter he returned to Harvard. Although he nominally remained a member of the faculty of the economics department, he became president of the university in 2001. Following his resignation, effective at the end of the 2006 academic year, he is expected to return to the department as a distinguished professor. The economics department, like the philosophy department, had its pair of professors—in its case, Robert Barro and Gregory Mankiw—who would display their clashing views before the audience at their joint seminars.

1990 onward: A pessimist turned optimist

Understanding the concept of a system has been a central topic throughout my scholarly career. I had been engaged for decades in comparing contemporaneous systems. There was now a unique chance to observe and interpret what it meant for different systems to appear consecutively. Though everyone keeps

* ■ Nozick came to alter his earlier, radically libertarian views on several issues. A bold thinker, excellent writer, and man of warm humour and kindness, he died in 2002, at the peak of his powers.

using the expression 'change of system,' there is still no consensus about what it means. Indeed, some current interpretations are strongly opposed, and the disagreement causes confusion in political discourse.

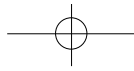
I clarified this problem for myself as I worked on my book *The Socialist System*, whose title underscores that 'system' is a central concept in it. Let me sum up here the three characteristics that I see as distinguishing the 'great' systems: (1) political structure and associated dominant political ideology, (2) property relations, and (3) coordination mechanisms (the relative weight of market coordination, bureaucratic coordination, or other mechanisms). The numbering is not random but refers to the rank order of the three principal components. These three characteristics go on to determine other important features of the system: behavioural regularities, enduring market forces, and so on.

The characteristic features of the classical socialist system are a monopoly of power held by a Communist Party opposed to private ownership and to the market and the predominance of public ownership and bureaucratic coordination. The characteristic features of the capitalist system are a regime friendly toward private ownership and toward the market and the predominance of private ownership and market coordination. The change of system occurred when the socialist system yielded to a new system exhibiting the characteristics of capitalism. Demagogic, populist remarks are often made about how the change of system has not occurred yet. I am afraid those who preach such texts have no idea what "system" or "change of system" really means.

Of course, the mere fact that the transition between the great systems has occurred, that capitalism has succeeded socialism, leaves many questions open. There are many types of capitalism. Some assign a significant role to the state and some a less significant. Some have obtrusive and some unobtrusive inequalities in the distribution of power, rights, or wealth and income. Some provide stronger and some weaker incentives to technical advance, and so on. The real issue for further debate is not whether the change of system has occurred, but what specific direction the changes are taking. The connecting normative question is, what direction would we like to go in? And that brings us to the issue of value judgments.

Many people in Hungary and the Eastern European region are disappointed. They expected the change of system to bring something different, something greater and better. I am not thinking just of those who actually did badly—losing their jobs, descending the income scale, or being deprived of privileges. Disillusionment is also found among many members of the intelligentsia, who have not lost financially or have even benefited, who have not suffered any personal harm and may have gained extra recognition. They are embittered by the widespread dishonesty and mendacity they see, and by the dissipation of state wealth. They shudder at the sterile verbal battles in politics, the undiscovered corruption, the revelations that are never followed up or that fail to even take place, and the interpenetration of business and politics. It offends their sense of justice to see flaunted wealth and dire poverty juxtaposed.

I share those feelings of bitterness and indignation. But I would not add that



I am disillusioned. Disillusionment comes from having expected more, and my expectations must have been more modest than those of many friends and acquaintances who feel that the change of system has let them down.

In one of my articles I referred ironically to those who see history as a convenient supermarket, where the attractive features of various systems are put in the shopping basket and taken home in a combination that best meets one's taste. History offers 'packages,' including 'existing' capitalism with its own immanent, system-specific problems.

In 1983, I wrote an essay titled "The Health of Nations," briefly reviewing the pathologies of seven grave diseases: inflation, unemployment, shortages, excessive growth of foreign debt, disturbances of growth, malignant inequalities, and bureaucratization. (Plenty more could be added to the list, of course.) I then took the risk of saying that there is no such thing as a disease-free socioeconomic system, but we can choose our diseases. Let us be glad to have developed a social system that suffers from only two or three of those diseases. In the worst cases, countries suffer from four or five.

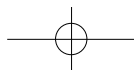
I was not surprised to find that the transition from socialism to capitalism brought mass unemployment. The most we could do was to struggle to keep it down, but it could not be overcome altogether. I was not surprised that income differences suddenly grew. Radical equalization would be impossible, but it was worth making an effort to help the needy and guarantee to all the conditions required for human dignity.

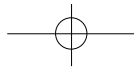
A number of factors tended to make my expectations more realistic than those of my peers. I am a professional researcher specializing in comparative social science—it is my trade. My studies had been focused for decades on exploring the nature of socialism and capitalism and comparing them. And my image of the developed capitalist countries had come not from books or short visits as a tourist, but from daily experience over periods amounting to several years. I took the opportunity to compare the written accounts with what I saw with my own eyes and can safely say I have no illusions about capitalism.* Despite its detrimental and morally nasty features, I concluded I would sooner live under the capitalist system than in the happiest barrack in the socialist camp.

I have to add another explanation of why the change of system did not disillusion me. Deeply ingrained in me is a rule of analysis: positive and normative approaches to a phenomenon need to be kept strictly separate. We all have a right to our dreams—how sad if poets gave them up. But I am irritated at people who style themselves social scientists and mix utopia with reality, especially if they make a virtue of their mental confusion and shrug it off when their dreams are compared with realistic possibilities.

I am sometimes more or less alone in the company of intellectuals with my view that the change of system we have been living through is really a huge achieve-

* ■ I see similarities between two clusters of illusions. The New Left built fantasies on a socialist utopia and turned in disgust from the socialism that actually appeared. At the same time, many members of the intelligentsia before the change of system built up a distorted picture of 'the West' and its democracy and market economy. Faced with real capitalism, they were aghast, as they compared it not with realistic expectations, but with their own imagined utopia.





ment. My feeling of taking part in a fortunate turning point in history did not end with the first euphoria of 1989–1990. It persists today, a decade and a half later.

Before the systemic change, I tended toward a variant of pessimism. Pessimism cannot preclude action, I argued in 1983 in the “Health of Nations” study mentioned earlier. I quoted Camus’ novel *The Plague*. Rieux, the physician, is talking to his friend Tarrou, who has been helping him combat the plague. “ ‘Yes. But your victories will never be lasting; that’s all.’ Rieux’s face darkened. ‘Yes, I know that. But it’s no reason for giving up the struggle.’ ”

I write as follows in my preface to the American edition of *Contradictions and Dilemmas* in 1986:

I must warn the reader: this is not an optimistic book. But neither is it pessimistic. There has been a Hungarian tradition for centuries: you are resigned or desperate or angry and a happy outlook is uncertain or improbable—and yet, you work hard and honestly for improvement. Those who have read classics of Hungarian drama or poetry (some are translated into English), or have listened to Bartók’s music, will know exactly this contradictory mood. Perhaps a member of a grey and non-philosophical profession, like that of an economist, can follow the same tradition.

In me, the mix of optimism and pessimism has tipped toward optimism since the change of system. It would be irresponsible to sketch in a few lines a balance of the favourable and unfavourable aspects of the changes in progress in the country and the world—even another long book would be insufficient for that. I will mention only a few phenomena to illustrate the kinds of changes that have affected my attitude toward life. I do not deny the problems and I face up to them, but I see a different future since the chance of freedom has opened up before hundreds of millions of people in the post-socialist region. Following that opening, a new, great wave of democracy has augmented other, earlier waves of democracy. In historical terms, the area of the world under tyranny has narrowed and the area covered by democratic institutions, stronger or weaker, has widened considerably. Though the process may be coming to a halt in some places or even going into reverse, I am convinced that such setbacks can last only briefly. I, too, shiver to learn of the dreadful deeds of extremists, and I sense what the unforeseeable consequences may be if weapons of mass destruction come into the hands of terrorists. Nevertheless, I believe that the historical process of the spread of democracy, in line with the trend of former decades and centuries, will continue.

Production is growing everywhere, though unevenly, technology is developing, and the quantity of resources available for human consumption has grown. I am fully aware that there always are new difficulties to overcome. Still, I am not going to bewail the woes of a consumer society or an aging society or the spread of computers. To me, it is an advance if lights burn in villages and sewage is piped away, if epidemics are stemmed and life expectancy is extended, and if people are better linked by modern information technology and telecommunications. I have turned into an optimist who recognizes the problems and wishes to alleviate them. ■

