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- 1968 Tour d'Horizon

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Back cover: *The First Race at Pest on June 6, 1827. Coloured lithograph by János Schmid after a drawing by J. E. G. Prestel and Alexander Clarot. National Széchényi Library, Budapest. Three stars of the Hungarian turf (from top to bottom): Overdose, the emerging sprinter, Kincsem, unbeaten in 54 starts in the 1870s and Imperiál, the wonder-horse of the 1960s.*

János Kornai

The Joys and Woes of a Researcher

I consider it an exceptional honour to be appointed an honorary doctor at your university, the Krakow University of Economics. I am moved by it, with feelings of joy that resemble those of a farm worker reaping the harvest. But having chosen that metaphor, I must add that the pleasure of harvest is preceded by the tough work of ploughing and sowing, and continual concern about whether the harvest will be flattened by hail or ruined by a storm. And in some years that happens, the pains-taking work is fruitless, and the farmer does not reach the happy moment of harvest.

Let me turn from that metaphor to my own life and say something about research, not farming. I will seize this chance, the ceremonious moments of receiving an honorary doctorate, to say a few personal words about the joys and woes that come with the work of an academic like myself.

Research

There is always great joy in hitting upon the problem to be solved. I am excited not only by the task of solving the puzzle, but by the realisation that I have a puzzle before me. I cannot generalise here about the experiences of others, but in

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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, Massachussets 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 *(1959);* Anti-Equilibrium *(1971);* Economics of Shortage (1980); *perhaps his most influential work;* The Socialist System. The Political Economy of Communism *(1992);* The Road to a Free Economy *(1989);* Struggle and Hope *(1997) and* Welfare, Choice and Solidarity in Transition: Reforming the Health Sector in Eastern Europe *(2001), co-authored by Karen Eggleston. On June 19, 2008 János Kornai became an Honorary Doctor of the Cracow University of Economics, the 14th honorary doctorate he has received. This is the address he gave on the occasion.*

my case, I do not always know at the start of some research project exactly what I want to examine. I am not moving toward some well-defined target, and I would have been in trouble if I had had to state in advance for a bid for EU funding exactly what task I was undertaking. When we arrived in 1975 in Stockholm, where we had been invited for a year, my wife and I talked a great deal about how the year should be spent. It was a few years after my book *Anti-Equilibrium,* and at first I thought I might write an "Anti-Equilibrium Revisited". However, I was to hold a series of lectures on the operation of the socialist economy to PhD students at the University of Stockholm, and I was prompted to provide for them a comprehensive picture of how the socialist economy worked. Meanwhile I decided that the shortage syndrome would have to provide the iron frame to which I could apply my other ideas. The comparison is not a forced one. The architect designing the appearance and internal structure of a building may experience joy similar to mine when the drawings for the building emerge out of many sketches, showing the building as a whole. My wife often remembers how I called out to her amidst the sounds of typing (for this was before the age of personal computers): "I do not know what the end of this story will be, but I already feel I am progressing with it as swiftly and straight as an arrow." Those were moments of joy indeed.

People often recall the cry of Archimedes: "Eureka!"—I have it! That feeling is not confined to scientific immortals like Archimedes. It is felt by our kind of researchers as well.

I am glad to say that moment in Stockholm was not my only experience of that kind. I was in Helsinki when I wrote the first half of my book *The Socialist System.* The most important chapter of it, in my opinion, is the one that explains the chain of causality connecting the various spheres of the socialist system, and suggests how far they can be seen as decisive elements in the political structure and ideology. We were walking in bright sunshine through an open-air museum on one of the islands when my mind became filled with a picture of mosaics. I sat down and quickly noted down my ideas. These are the moments of enviable bliss in a researcher's life.

But nobody should think that the months or years of each research project consist only of such happy hours. Most of the time is spent, let us face it, on humdrum work. Nobody in our profession can be a Picasso, able to put down on a piece of paper in moments a sketch that others consider a work of genius. We have to drudge, for days, months and years on end. We have to read masses of specialist literature to cover the antecedents to our work and the conclusions being reached by rival researchers. We have to gather and process data, make calculations and prepare tables. And having reached the stage of writing a piece, we have to weigh each word and repeatedly revise each draft. There is no denying that this involves suffering rather than enjoyment. Performance calls for more than discernment, intuition and an analytical capability. It also needs patience or *Sitzfleisch,* as the Germans call it. You want to jump up and do something else, and it takes self-control to order yourself back to your books and computer.

There are a number of instruments to be found in the research chamber of torture. One merciless instrument is research failure. You realise after long struggles that you have strayed up the wrong road, which does not lead anywhere. One of my experiences of great failure has a bearing on Poland. Oscar Lange's oft-quoted model of socialism confirms that under certain conditions, market socialism is capable of the same performance as market capitalism. I took issue with these conclusions in the Hungary of the 1960s. I felt almost sure that market socialism would not fulfill its promise in reality, obviously because some of Lange's assumptions made sense only on the page of a journal, but not in practical reality. So with mathematicians and computer specialists, we set about designing a simulation model that was supposed to represent a dynamic economic system similar to Lange's type of socialism. We wanted to build into it some relations that would represent the frictions of real systems, such as the rigidity of prices and the distortions of information. We followed good instincts in trying to approximate the model to reality as closely as possible, but that made it ever more complicated. Yet however we tried to adjust it, it refused to work. We spent weeks and months on it. Perhaps another, more ingenious team than ours would have succeeded, but we had to admit defeat and finally abandon the plan.

One central idea in *Anti-Equilibrium* and *Economics of Shortage* was that the actors on the two sides of the market are not equally strong. Either one or the other is stronger—we face either the buyers' or the sellers' market. One of the main system-specific features of the alternative economic systems—capitalism or socialism—is the direction in which the scale is permanently tipped: the general relative strengths of buyers and sellers.

Many people incline to admit there is truth in this assumption. Unfortunately I failed to create a mathematical model adequate to the problem. I tried to obtain help from other, better designers of models, but I failed to find a real intellectual partner for the task. I knew I had something very important to say, but I could not express myself well enough. Something similar might be felt by a poet who experiences something but cannot put it into words, or a composer who fails to write down the music audible in his or her head. I have a recurrent bad dream of standing at the rostrum of a great conference and not being able to utter a word. Psychologists would call it a typical anxiety dream. There has been more than one message of mine that I have not managed to express adequately. This is one of the worst torments in a researcher's life.

Publication

Let us move on. The research is done. The next phase is to publish it. It is a proud, joyous feeling when your work comes out, and you have the printed article or book in your hand. An author of a newly published work has something of the joy felt by a mother in her newborn child. But there are previous phases as well, and these are not easy to endure.

Let me mention first the historical period in which we authors were subject to pressure from the Communist political regime. Those whose message had a political content faced a difficult dilemma. What forum of publication should they choose? The more critical their work and the deeper it delved into the fundamental attributes of the socialist system, the stronger the dilemma became. If they were intent on telling the *whole* truth, they could only use illegal channels of publication. If they chose those, they laid themselves open to persecution, of themselves and possibly of their readers. Certainly, each samizdat publication reached relatively few readers, and illegal "flying university" lectures were attended by relatively few listeners. In addition, there were the torments of harassment or even incarceration, and a general feeling of isolation.

The other alternative—which I chose in the years of repression—was to devise writings intended for legal publication. But that meant undertaking self-censorship in the interests of legality. I always insisted on writing *only* the truth, but I had to accept that I could not write the whole truth if I wanted my work to see legal publication. I refrained in the late 1970s and early 1980s of stating in *Economics of Shortage* that the ultimate explanation for the dysfunctional attributes of the socialist system lies in the political structure, the autocracy of the Communist Party, and the political activity and institutional system suggested by official Marxist-Leninist ideology.

Self-censorship is a demeaning torment. The only comfort is that for the price of it, you can put some recognised truth into many people's hands. *Economics of Shortage* was read by several tens of thousands in Eastern Europe and sold a hundred thousand copies in China. Was that a comfort? Yes and no. It is a good feeling repeatedly to meet people in China or Russia, or here in Poland, who tell me the book opened their eyes. And it is an ineradicable woe to think how much of what I wanted to say had to stay inside me in those days, when I wrote the book.

Let me add for completeness that there is a special feeling of pleasure associated with self-censorship. What was the cleverest way to evade the censors and eagle-eyed editors and watchful supervisors at party headquarters? How could I smuggle in a message between the lines? How could I write 22 chapters of *Economics of Shortage* so that a reader with a quick ear could guess right away the contents of the missing 23rd and 24th chapters? That cat-and-mouse game provides an intellectual enjoyment well known to writers, poets and social scientists under all sorts of dictatorship.

The change of system in 1989–90 came as an intellectual liberation, a first experience of the joy of full expression. That joy is not something Western colleagues can appreciate. It is a natural condition for them, but far from self-evident to us Eastern Europeans. Only those who lived in the shadow of the Berlin walls of censorship and self-censorship could appreciate their collapse.

Even now, the joy of freedom of speech is not unalloyed. For research economists, it is often restricted by narrow-minded interpretations of the ideas of mainstream economics. The truly significant exponents of that productive school

of thought, people like Arrow or Samuelson, never sought a thought monopoly. On the contrary, they positively encouraged those who came up with new, unorthodox ideas. But mediocre and biased economists often put up obstacles to the publication of work that falls outside the mainstream. I usually describe myself as someone who has one foot in the mainstream and the other stepping beyond it. I have had papers rejected by anonymous referees, sometimes complemented by arrogant sermons. Or the referee and the editor have tried to impose their ideas on me through "conditional acceptance" of the work: do what they prescribe and they will publish your article, otherwise they will not. I have not bowed to them in any such case, preferring to seek another journal to publish it. Yet there is no denying that such curtailment of authorial sovereignty is embarrassing, even demeaning. In my case, my status does not depend on the number of publications any more. I have been through the process of obtaining "tenured professorship", which is highly dependent on publication record, and I have bidden farewell to that position, after retirement and becoming professor emeritus. But I can understand the difficulties facing my younger colleagues. Should they follow their own path bravely? Or should they choose and work on a subject that will win the sympathy of referees and editors? Which is better: intellectual conformism or the risk of isolation and a break or retardation in one's career? Such dilemmas also belong to the dark side of the researcher's calling. None of us can run a triumphal march. Those who set out to be researchers have to reckon with the woes of publication as well.

Reception and responses

After the first phase of research and the second of publication comes a third: the afterlife of the work. Will there be reactions? Will it exert an influence?

This phase does not start normally with publication in print, but much earlier, when the researcher first presents his or her research findings at seminars or as a draft circulating among colleagues. If the work is really good, then there will be very welcome appreciative comments from the outset, and these belong to the joys of research. In my experience, there are also comments of other kinds, expressing disparagement ("We knew that") or doubt ("What use is that?"). And those are still better than the comments of those who misunderstand what one is saying or twist one's meaning, more or less intentionally.

Such reluctant, sometimes provocative or even malicious comments have cropped up constantly alongside my successes—at the stage where the author is still passing the manuscript around. I remember how our later very successful work *Kétszintű tervezés* (Two-level Planning), co-authored with Tamás Lipták, was received by two referees at my place of work. One greeted it warmly and the other poured scorn on it. This may rob some of any desire to continue. Certainly it calls for great confidence in one's ideas to prevent oneself being deterred.

Later, after publication, come the reviews and citations—or not, as the case may be. Some authors say they are only concerned with the higher truth and pay

no regard to its reception. I doubt that they are being sincere in that. Those who research and write wish to influence people. At least they would like their colleagues to give some attention to their work. And if an economist talks about topical matters and public affairs, he or she would like not only fellow academics but the general public to pay heed.

The reception may be unpleasant or even insulting for political reasons. I write something and people attack it, because the attackers' world view and political philosophy differ from mine. This has to be accepted by those whose message has a political content. It is a bad feeling to read attacks on one's work, but it can be mingled with a little pride at having written something that "got people going" a little. At the beginning of the 1990s, I coined the expression "premature welfare state" and there have been several hundred references to it since, to my delight. But it feels bad when a Hungarian sociologist with whom I have been friendly for decades does not wish to talk or respond to a greeting, because she feels my criticism of excessive state spending beyond what is appropriate to the country's level of development is a stab in the back for humanity and modern civilization.

There are also successes and failures, joys and woes alike awaiting economists who step beyond narrowly interpreted academic life, into the border areas that impinge on science and politics. It is a joy if they find their advice is taken, and a fairly bitter experience to see it ignored. I have undergone both in the last few years, often hardly distinguishable when the joys of influence and woes of indifference or resistance have been combined.

But let us turn back again from the borders of science and politics to academic life. The degree of joy or woe depends not only on the actual degree of influence exerted, but also on the degree of influence the researcher had expected. The sense of joy and the sense of woe depend on the difference between aspirations and results. Let me add a few personal words here.

When I try to assess my work with hindsight, I think it is important to consider not just single theoretical assertions or relations, but also the outlook and methodology with which I approach the problem. This type of approach I termed the "system paradigm" in one of my studies. I had a good many favourable reviews for my book *The Socialist System* (important entries in the list of joys), but the appreciation was usually confined to the way my work had contributed to an understanding of the socialist system. What I had aspired to was to see the methodology of the book noticed and utilised by others who happened to be dealing not with the socialist system, but with some other socio-historical formation. That kind of reception was lacking.

If I mention this occasionally to close friends, they ask why I expect so much. Were my aspirations lower, my chagrin and discontent would be less.

That is true, yet perhaps not. The wise advice is in vain because our bodies do not contain a clever little dial for regulating our aspirations, our demands of ourselves and the level of influence expected. If we had one, we could turn our

aspirations right up or right down like a gas ring. But the level of aspirations cannot be regulated, or at least, I have never managed it.

Great ambition is the great motive force. Those whose gas ring burns low may be calmer and more balanced, but their performance is the lesser for it. It is the feeling of discontent and unfulfilled ambition that goads us into greater performance. Although this often comes at the price of feeling low and disillusioned, it may also spur us on to new, difficult scientific tasks.

I see quite a few elderly researchers in this hall, who will certainly understand the feelings I am talking about. But there are young people sitting here as well, and I hope they also have grasped what I have said about my own career. If they shrink from the torments of a research career, they should step back now. But if they are prepared for such a career, they should reckon with the woes and the great, special, incomparable joys that a research career brings. Believe me, it is worthwhile.

Let me once again thank the Krakow University of Economics for choosing to add to the list of joys that I keep, not the list of woes, by awarding me this high honour at this fine celebration today. ❧

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