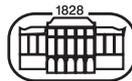


THE NEW POLITICAL ECONOMY OF EMERGING EUROPE

Second, revised, updated and extended edition

By

László CSABA



AKADÉMIAI KIADÓ · BUDAPEST, 2007

ISBN 963 05 8196 5

© László CSABA, 2007

All rights reserved. No part of this book may be reproduced
by any means, or transmitted or translated
into machine language
without the written permission of the publisher.

Published by Akadémiai Kiadó
H-1519 Budapest, P. O. Box 245

Printed in Hungary

To my wife

CONTENTS

1. FOREWORD TO THE SECOND EDITION	11
2. FOREWORD – WHY THIS BOOK?	17
Preliminaries and Background	17
What Is New Political Economy?	19
Why Not Eastern Europe/Transition Economies?	25
The Map of the Project	27
References	29
3. A COMPARATIVE OVERVIEW OF EMPIRICAL EVIDENCE	32
On Catching up	33
Production and Performance of the Transforming Economies	35
<i>Summary View</i>	42
Utilization of Gross Domestic Product	44
The Social Side of Transformation: Inflation and Unemployment	47
External Trade and Finance	55
<i>Trade – Exports</i>	56
<i>Import Markets: Potential or Problem-Field?</i>	58
<i>External Finance</i>	61
<i>Foreign Direct Investment – Do We Possess a Key?</i>	64
Some Preliminary Conclusions from Empirical Evidence	68
References	70
4. THE DEVELOPMENTAL DIMENSION OF POST-COMMUNIST CHANGE	88
The Thorny Path to Growth	89
Emerging Economies or Developing Societies?	97
<i>Varietas delectat?</i>	104
References	105

5. TRANSNATIONAL MARKETS AND LOCAL POLITICS – THE POST-COMMUNIST DIMENSION	109
State Desertion, State Failure or Subversive Globalization?	109
Policies and Erosion in Emerging Economies: What Is the Nexus?	113
The Experience of Frontrunners	117
The Same Seen from the Angle of the Mouse: Laggards in Transition ...	123
The New Economy – a Defining Feature of 21 st Century Development	129
On the Co-Evolution of the State and Market	132
References	135
6. ‘GLOBALIZATION’ AND ‘EUROPEANIZATION’: A DOUBLE CHALLENGE FOR EMERGING EUROPE	139
Global Challenges – Transnational Processes.....	140
‘Europeanization’ as a Special Challenge	148
New Members: The Challenge of Third Wave of Transformation.....	152
On Social Stress: No Pain, No Gain?	157
References	160
7. LIMITS TO ACCESSION-DRIVEN TRANSFORMATION IN CENTRAL EUROPE	164
Transition and EU Integration: Do They Fully Overlap?	164
European Governance in Global Perspective.....	168
European Standardization and Developmental Tasks.....	173
Europeanization: Is There a Move to a Single Standard Solution?.....	177
Toward Multi-Level Governance – By Default?	179
References	181
8. A NON-STABILITY AND ANTI-GROWTH PACT FOR EUROPE?	184
What Is at Stake?	184
The Stability and Growth Pact and the Single Currency	186
The Stability and Growth Pact and the New EU Members	193
The Gradual Adoption of the Euro?	200
Conclusions.....	208
References	210
9. RE-INTERPRETING RULES-BASED BEHAVIOUR IN EUROPE	214
Rules-Based Fiscal Policy in the European Union	215
Initiatives to ‘Patch Up the Pact’	220
Re-Interpretation of the Pact.....	225
Perspectives of the New EU Member-States	228
Theory and Practice	233
References	235

10. POST-CRISIS PERSPECTIVES ON THE FUTURE OF THE EU.....	242
Three Overlapping Crises and Enlargement	244
Taking Stock and Looking Ahead	247
Do New Members Really Form a Coalition of the Nasty?.....	251
The Potential for Institutional Innovation and Further Enlargement.....	254
References	257
11. RUSSIAN TRANSFORMATION: FROM STATE FAILURE TO MARKET FAILURE?	259
Russian Economic Change: A Bird's Eye View	260
The Russian Market Order: The Wild East and/or State Capitalism?	267
How Can We Test If Russia Is Indeed a Market Economy?.....	274
What Matters for Growth? Prospects for the Russian Economy.....	279
References	285
12. MARKET SOCIALISM: THE VIABLE IMPOSSIBLE?	298
China – A Challenge to the Post-Washington Consensus	299
Why Marketize Socialism? Salient Features of the Classical Model.....	302
<i>Salient Features of the Soviet Model: Poetry and Reality</i>	302
Reform Socialist Models	310
<i>Bukharin's New Economic Policy</i>	310
<i>The Yugoslav Model: Decentralization and Self-Management</i>	315
<i>The Hungarian Alternative – The New Economic Mechanism</i>	319
<i>The Chinese Model</i>	322
Is China a Developmental Alternative?	326
Concluding Remarks.....	337
References	339
13. PRIVATIZATION, REGULATION AND REGULATED MARKETS.....	345
Privatization, Regulation and Efficiency.....	345
Private Property and Multi-Level Governance.....	349
Interrelationships between Welfare and Financial Sector Reforms	353
Regulatory Regimes: The Case for Transnational Solutions	357
The European Dimension	362
References	366
14. INSTITUTIONS AND GROWTH: WHAT IS THE NEXUS?.....	368
Endowments, Policies and Culture.....	369
Formalization and Enforcement	373
Policies and Transnationalization	375

The Politics of Economic Reform	378
The Role of Financial Institutions	380
Self-Correction and Coordination in Complex Systems	385
Rules, Ideas and Policy Discretion.....	388
The Democratic Development Alternative	390
References	393
15. SUBJECT INDEX.....	397

1. FOREWORD TO THE SECOND, REVISED AND EXTENDED, EDITION

It is a great honor and pleasure for me that the Publisher has agreed to bring this second, extended edition of my monograph on emerging Europe to the international audience. The first edition has aroused considerable interest, over a dozen of reviews have been printed in Hungary and abroad, including Japan, Germany, Russia, Great Britain, the Czech Republic and Bulgaria. The book has won the Award of the Publisher in 2005, granted on the base of majority voting of the Section for Law and Economics of the Hungarian Academy of Sciences. The book has been in classroom use at several universities. Besides Hungary, I am aware of French and Japanese schools using it in their various curricula. Owing to the generosity of the Publisher a substantially revised and also extended and updated Hungarian edition has also been launched in 2006 (Csaba, 2006). The latter version has also received considerable attention, both in the electronic and printed media. Finally the present edition is an in-between: it retains much of the original text, but *complements it with two new chapters, unsurprisingly, on the European Union*. Certainly, wherever needed, especially in globalization and EU-related issues, updating of the bits and pieces, reflection on some of the new developments in the literature and 'in the world out there' complement the original version. I tried to improve some of the shortcomings pinpointed by the dozen reviews published so far. But I consciously refrained from re-writing the entire empirical-statistical source material, or from introducing brand new theoretical models. Both requirements may make perfect sense, perhaps in a different project, not however in a revised edition.

Why bother with an *extended and revised new edition* instead of the usual reprint? First and foremost: the basic structure and the line of argument of the previous edition, that was sold out, is still valid. None of the numerous reviewers to date have questioned the fundamental logic of my reasoning, or the major conclusions, which is truly good news for such a contested area as new political economy. Meanwhile, since the previous edition went to press, *the European Union has undergone a major crisis*. The Constitutional Treaty has been rejected in two founding nations, while several others, including Britain and Poland decided to adjourn the referenda over its ratification. In 2004 the European Court of Justice repelled the Ecofin decision on suspending

the Stability and Growth Pact, while the Council in March 2005 agreed to re-interpret those rules that guide the coordinated fiscal framework for the single currency.

These two changes required a specific reflection. Chapter Nine therefore analyzes the controversies over the *new fiscal framework* in the light of economic theory, as exemplified by the Nobel prize laureates of 2004, Kydland and Prescott, who were awarded for their pioneering work over time inconsistency (Kydland and Prescott, 1977), the issue behind the conflict between short and long term incentives for policy makers. Under this angle the chapter analyzes if, and through what mechanisms, the more flexible reading of the Pact can, and indeed will, be conducive to *higher and more sustainable economic growth in Europe*. The re-launch of the Lisbon Strategy in 2005 required the inclusion of such analysis from a long run perspective, while the changes in the rule prompted us to take a stance on the merits of the modified fiscal framework.

Chapter Ten, yet another new inclusion, brings us to those broader debates that have been triggered by the original texts, as well as by a series of conferences I had the chance to attend. In October 2005, just a few months after the first edition came out of print, *accession negotiations have been launched with Turkey and Croatia*. This implies an interesting paradox. For one, rejection of the Constitutional Treaty has resulted not so much from the actual contents of the text, or the lack of its meaning, but from the *percieved* threats in terms of economic insecurity and social stress, from *potential future enlargements*. The median European citizen wishes that the EU first consolidates, at least for a decade, and only later engages in future enlargements. Meanwhile the logic of Europeanization does require immediate and continuous action, since the *convergence of economic and political systems* in those areas which used to be called the European periphery, do not emerge spontaneously, *without an anchor*. The anchor - often by default - has been the European Union, and in operational terms the European Neighborhood Policies of 2005, offering a number of new forms of cooperation for all the countries around the EU, i.e from Morocco through the Palestinian Authority, Moldova, Georgia and from Kyrgyzstan to Belarus. In the new Chapter Ten, an attempt is being made to assess the *potential of these new policies in a strategic perspective*. We analyze if, and to what degree, *institutional innovation* may contribute to overcoming the current dead alley. Certainly the bits and pieces of further enlargement require further elaboration, however it falls outside the scope of the current volume (Csaba, 2007).

Otherwise the main line of argument, as well as the *overall pattern of the project remains unchanged*. The chapter on the Comparative overview of empirical evidence provides the basics needed for meaningful theorizing. In letting the statistics to run, by and large, as they used to be, my intention is to show that the process of differentiation among transition trajectories *has long been observable*. Commenting on the upcoming new data has, certainly, got its inherent merits, irrespective of the more abstract claims of the present project – a good quality example of the former is presented by Gligorov et al (2006), published right at the time of concluding the present edition. Their findings point,

in a number of ways, in the same direction as did the conclusions of our comparative empirical chapter, in terms of policy implications and statistical trends alike. This is good news, as it means that our findings are not time-constrained, nor unnecessarily limited by our reliance on the selected sources, *and survive even closer scrutiny and the control for the newer numbers*. Thus it might be legitimate for us to jump to the more abstract issues instead of further number gazing. Given that failures are much more frequent than successes – ever since – the *developmental issues* and reflections on *transnationalization* follow, which put the transition experience in a global context.

In this global approach the chapters on the EU and its influence on the emerging economies follow. Having arrived in what used to be perceived as safe haven, the frontrunners among the emerging economies are confronted with a series of crises, in the institutional and policy domains alike. *How to avoid the marginalization of European structures*, especially, though not exclusively in finances, is the unifying theme of these chapters.

Having adopted a normative approach to explaining the ways and means to success, intellectual honesty requires us to address what seems to be *the two counter-examples of success* in the 2000s: the institutionless development of authoritarian regimes registering high growth. Interestingly, none of the reviewers so far has questioned the legitimacy of including China, the developmental paradox of the past decades. Moreover in several of the classes and conferences, unrelated to China, such as on the role of money, banking and rule of law, or property rights, the Empire of the Middle comes up as an example. Also, a reflected *inter alia*, on the pages of *American Economic Review*, *Journal of Political Economy* and the *Journal of Economic Perspectives*, the focus of interest of mainstream analysts has also been gradually re-focused, at least in part, on China. And the two mainstream-adjusted outposts of comparative economic research, *Economics of Transition* and even more its US counterpart, *The Journal of Comparative Economics* abound with contributions analyzing and interpreting the Chinese paradox. But attempts to decipe Russian successes under both Putin Presidencies are also in no short supply in similarly prestigious fora of academic analyses (besides the traditional area studies and international relations [IR] journals). This is a more conventional option. However our purpose is to show, *how different the Russian trajectory has been* from the Central European mainstream. For this reason Russia is no longer an emerging economy in the same sense as the new EU members are.

Finally, a neo-institutionalist approach can not but reflect in some detail, i.e. in the two concluding chapters on the role of *regulation and regulatory agencies* in democracies, as well as on the *role of institutions*. Which institutions matter and how do they matter? This is the big unknown, for which a temporary and partial answer is being provided. Suggestions for reforming available national and European institutions in order to foster competitiveness conclude, together with more theoretical reflections on institutions and economic development. In the latter, vein changes in the natural sciences, allowing for the better understanding of the workings of the brain, perceptions and of reasoning, do call for changes in social sciences in general and economics in particular, in terms of

overcoming the mental constraints imposed by the predominant analytical frame bequeathed by Newtonian physics and its mathematical arsenal (North, 2005). This has developed into an entire research paradigm over the past years in the leading academic journals in the UK and the USA, with empirical studies documenting the prevalence of *incoherent* behavior and the ensuing *inability of ordering of preferences* in vast numbers of economic decisions (Cohen, 2005). Individual irrationality may, but does not have to, bring about sizable changes in *aggregate* outcomes, rendering mechanistic extrapolation (and linear modelling) largely irrelevant (Fehr and Tyran, 2005). Measurable items of income relate only loosely to happiness, rendering conventional hedonistic economic hypotheses, such as mobility and more consumption being axiomatically good, as questionable, *especially for public policy applications* (Layard, 2006). Components previously abstracted away, such as trust, good governance, lack of corruption and workplace security/job satisfaction all may, and often do, matter more than an additional unit of monetary income (Helliwell, 2006). And yet another Nobel winner is in the forefront in the attempt to measure *well-being*, rather than material *output* as the end-result of economic activity (Kahneman and Krueger, 2006) – an issue we recurrently discuss in various chapters on development, governance, success indicators and the role of institutions in the democratic development alternative. *The broader approaches*, including new political economy, *no longer count as esoteric outgrowth* of a serious, technocratic and socially blind analytical discipline, some of the mainstream departments let economics to be learnt.

All in all, as the above sketch of the structure may have illustrated, this book is *not* a collection of previously published papers, but an *old-fashioned monograph, following a single line of argumentation*. Though most chapters have been presented to several conferences and also to several readers, the integrity of the overall line of the argument has hopefully remained intact. The inclusion of the two new chapters is justified by the nature and weight of changes that have taken place in the past two years, that require reflections on specific issues. But the *normative insights remain unchanged*. It is perhaps the field of development economics where these new norms have become most pronounced, as recent analyses of the field (Szakolczai, 2006, Szentes, 2006) have illustrated.

This is certainly *not a mainstream book*, as one of the reviewers of the first edition aptly noted. But, as Professor Alexandre Lámfalussy writes in his recommendation to the Hungarian edition, this has to do with the conscious attempt of the author to avoid *clichés* and intellectual quick fixes. In so doing it is my hope to have contributed to *a number of open-ended debates that run in parallel but on different levels of abstraction*. One is the level of *economic theory and methodology*, an attempt to come up with propositions of how to conduct economic research in a socially responsible and relevant fashion. A second level is that of *area studies*, by shedding light on what transition has meant on a global scale, and how to interpret local developments in a global perspective. Third, at the level of *European Studies*, the overdue reform of the *European Union* may be more effective, if policy debates make use of the insights from more academic analyses. Fourth, understanding big countries, outside the scope of self-contained

area or country studies, is an important task for *comparative approaches*, which may shed light on some of the less trivial aspects of these specific paths of development. Finally, as fifth, the role of *institutions* and regulatory *agencies* is perhaps one of the most relevant, still the least elaborated area of modern social science research, *way beyond the boundaries of economics*.

Therefore, it is at least my sincere hope, that not only various strands of the economics profession, but perhaps also people working on improving public administration, on setting up and reforming international agencies, civil groups interested in defending human rights and privacy in the era of internet revolution, legal scientists, political scientists and specialists of IR may, perhaps to a different degree, profit from confronting our insights with theirs. In so doing a *cross-fertilization* of approaches may emerge, which, for the present author, would translate into a *more relevant* brand of economic research than the one currently dominating many of the mainstream departments.

Last, but not at all least, it is my duty to express my gratitude to those who facilitated my work on this second edition. The International Relations and European Studies Department of the Central European University under then Chair Julius Horvath and its doctoral program, the CEU facilities, library and staff, have all been of great assistance. The doctoral school at the Faculty of Economics and Business at the University of Debrecen with colleagues and students have also provided intellectual stimulus. It is a great pleasure to see so many of them, from both doctoral schools, developing into mature and independent researchers and civil servants, already publishing their ideas, and not only in Hungary, but also abroad. Zoltán Ádám, István Benczes, Pál Czeglédi, Matthias Gruber, Dóra Gyórfy, László Jankovics, Zdenek Kudrna, Balázs Pálosi-Németh are perhaps the most promising, though this list is fortunately far from complete. MA students from the Budapest Corvinus University, taking my class on Emerging Markets have contributed, through comments and criticism, to improving the preliminary texts. I tried to heed most of the critical commentary of the dozen reviews of the first edition and yet another dozen reviewers of the revised Hungarian edition, without sacrificing the overall line of my argument. And needles to say, the endurance and supportive love of my family, my wife Csia/Gabi, our grown-up, university student children, Zoltán and Orsi/Hedgehog, has been a source of endless inspiration and provided the purpose of the entire exercise.

Budapest, 31 July 2006.

László Csaba

References

- CSABA, L. (2006): *A fölemelkedő Európa* (Europe on the rise). Budapest: Akadémiai Kiadó.
- COHEN, J. D. (2005): The vulcanization of the human brain. *Journal of Economic Perspectives*, vol.19.no.4. pp3–24.
- CSABA, L. (2007): The eastward enlargement of the EU: a great success story or a last nail in the coffin of the EU? In: YAGI, K.–MIZOBATA, S. eds: *Melting the Boundaries: Institutional Transformation in a Wider Europe*. Kyoto: Kyoto University Press (in print).
- FEHR, E.–TYMAN, J.-R. (2005): Individual irrationality and aggregate outcomes. *Journal of Economic Perspectives*, vol.19.no.4. pp43–66.
- GLIGOROV, V.–PODKAMINER, L. et al (2006): Broader-based growth resilient to global uncertainties. Vienna: *WIIW Research Reports*, no.328 – (July).
- HELLIWELL, J. F. (2006): Well-being, social capital and public policy: what's new? *Economic Journal*, vol.116.no.510. pp34–45.
- KYDLAND, F.–PRESCOTT, E. (1977): Rules rather than discretion: the inconsistency of optimal plans. *Journal of Political Economy*, vol.85.no.3. pp473–492.
- KAHNEMAN, D.–KRUEGER, A. B. (2006): Developments in the measurement of subjective well-being. *Journal of Economic Perspectives*, vol.20.no.1. pp3–24.
- LAYARD, R. (2006): Happiness and public policy: a challenge to the profession. *Economic Journal*, vol.116.no.510. pp24–33.
- NORTH, D. (2005): *Understanding the Process of Economic Change*. Princeton, New Jersey: Princeton University Press.
- SZAKOLCZAI, GY. (2006): A gazdaságfejlődés elméletének megújulása: az első, a második és az új generáció. (Renewal of the theory of economic development: the first, the second and the new generation). *Valóság*, vol.49.no.2. pp1–35.
- SZENTES, T. (2006) : Az állam szerepe a felgyorsult globalizáció korában (The role of the state in the age of accelerated globalization). *Köz-Gazdaság* (a quarterly of the Budapest Corvinus University), vol.1.no.1. pp15–46.

2. FOREWORD – WHY THIS BOOK?

It is not the time to venture for anything longer than 3–4 pages, or a 40-second-long interview clip in the evening news. Still, with the time passing there emerges a need to take stock of what we have learned. The personal history of the author and the evolution of science often call for taking stock and looking ahead. If done in good time, the outcome might be of interest to a wider readership, and may even contribute to the promotion of knowledge in a particular field. Also knowing that there are no such things as ‘finally settled matters’ in any of the sciences, passage of time per se may justify our reassessment of the state of art in a given area. This area for me has been the study of Central and Eastern European economies and societies in a comparative, preferably global, perspective.

Preliminaries and Background

Two decades ago, when working on *Eastern Europe in the World Economy* (Csaba, 1984/1990) the question of the time was if and how command economies of the Soviet type can *survive* the fundamental changes that had been evolving in technology, warfare, economy, trade and also finance. The *puzzle* was that despite obvious shortcomings and the ever more pronounced signs of ossification, the system of the Soviet empire and related economic structures and political institutions still survived. While the insight of Ludwig von Mises (1920/1976) on the non-viability of any economic system lacking capital markets and the resultant misallocation of resources obviously held, the prophecy of the imminent collapse of Communist economies has not, at least for over 70 years (cf. e.g. Hanson, 2003). Our finding at the time, though packaged in what was feasible under contemporary Byzantine language, had been that the cumulation of strains was clearly observable. Moreover changes in the international embeddedness of the system were largely unfavorable for its continued existence. By the time the English language edition of the volume appeared, this insight was already turned into practice, with the disintegration of the Soviet block in 1989–91.

More than a decade ago, working on *The Capitalist Revolution in Eastern Europe* (Csaba, 1994/1995) there were *three basic puzzles* to be solved. One related to the fact that the collapse of the old regime has not triggered that imminent welfare improvement what has been expected not only by the population in the region, but also by mainstream economics¹ as well as a sizeable body of the analytical and policy-making literature (e.g. Collins and Rodrik, 1991). Instead, disorganization and contraction proved to be much more lasting than most contemporary agents, both in policy-making and the academe, would have expected. The thesis on the 'crisis of transformation' has established itself (Köves, 1992; Lavigne, 1995; van Brabant, 1998; more recently in Bogár, 2006). Against this contemporary mainstream, my monograph argued that path dependence and the nature of institutional change both require years, rather than months, if changes are to be meaningful and lasting.

The second puzzle had been, at the time, how little mainstream economics – journals and analysts alike – took note of what has been an epochal change in the history of mankind. Also, as Djankov and Murrell (2002, pp. 740–741) rightly stress, this has been a unique opportunity to experiment and *test competing economic theories*. This had not been the way contemporaries tended to see affairs. Calls for studying and translating existing/established textbooks, as well as of institutions were the main propositions. The – over-optimistic – expectations of EU accession only contributed to the advocacy of institutional imports as shortcut solutions (e.g. Portes, 1991). While some may consider it just professional inertia, the experience of the past fifteen years has shown that both during European accession negotiations, and even following the Enron scandal, the tendency of *benchmarking* and of trying to *impose/import 'first best' practices* has never subdued. My argument at the time stressed the historic uniqueness of the process and called for more innovative solutions. With the benefit of hindsight, and given the increased influence of benchmarking as a component of global governance practice, re-examination of the issue seems warranted. The third puzzle thus may be that with the increasing distance of the Communist heritage in terms of time transition countries *may lose their uniqueness*. They may become more like other emerging economies – if successful – or to underdeveloped economies, if state failure and market failure is their dominant feature.

Yet, a decade later the long engagement with the EU has become a formal marriage, with 10 countries of the region plus Malta and Cyprus joining in. The insight that like in medicine, not each individual survives the same therapy, thus knowledge of analytical insights do not replace *specific investigations and applications*, in terms of institutions and policies, has become part and parcel of established wisdom, also at the level of international financial institutions and the policy-making community (e.g. Krueger, ed., 2000; Meier and Stiglitz, eds, 2001). Looking through the major academic journals as well as catalogues of major Western publishing houses, we do not find anybody of stand-

¹ Once artificial distortions are eliminated, efficiency must improve, by and large of itself, short of interventions that account for the contrary.

ing who would not devote, with more or less regularity, time, space and money to analyzing the experience of systemic transformation.

Currently there are *several new puzzles* to be solved. First, the explanation for *success and failure* of once Communist economies should be provided *within a single analytical framework*.² Second, the experience of China, and more recently Russia, seems to defy the odds: an authoritarian political system can co-exist with largely decentralized economic controls with foreign direct investment (FDI) playing a major role in shaping economic structures and competitiveness. Finally, much of what used to be subject of transition literature increasingly merges with more general *development studies* (Ofer, 2001). This is the case in the larger number of not very successful cases, and with European studies, with a smaller number of success stories. These puzzles may also objectively call for stock-taking anew. These also require reflection on the experiences of growth and the lack of it, of *financial sustainability* and the lack of it, of political conditions favoring the emergence and *taking root of institutions of the civilized market economy*; last but not least, *conditions for good governance* and the lack of those. The latter term – used sometimes as a buzzword by the international agencies – means no less than the *return of the political* into economics. The devil, exorcized by the Walras–Samuelson line, now returns through the backdoor. This justifies our sticking to the term ‘political economy’, despite the unfavorable connotations burdening these as a legacy from the Communist period.

What Is New Political Economy?

What is then the *new political economy*, the approach wishing to merge the analytical insights of mainstream with the major role played by institutions and policies in implementing these? There is no easy answer, since in the social sciences, unlike in natural sciences, the evolution of generally accepted terms and analytical frameworks is less than uniform. In the 18–19th centuries, when the study of the economy emancipated itself from administrative sciences and law, the generally accepted term was *political economy*. National economics or public economics (*Nationalökonomie*) focused interest on macro-processes rather than on the individual household, that was to be studied by business management. While the Walrasian revolution implied a microeconomic angle, the then mainstream of the profession, centering around Alfred Marshall and John Maynard Keynes sustained the rigorous delineation between micro and macro. This holds to the point that modern macro-economics is normally originated with Keynes (a reason why Lucas [1996] in his triumphant counter-revolution declared the death of macro).

However, with the breakthrough of Nobel winner Paul Samuelson, first in terms of teaching, then in terms of analytical approach, and further with the

² For such attempts cf. e.g. Roland (2002) and Falcetti et al (2002) and Blanchard (1997), more recently Kolodko (2006).

victory of the counter-revolution led by Robert Lucas³ and Gary Becker, both awarded by a Nobel Prize for their work, the study of economic processes has become increasingly technicized, formalized in mathematical equations, and separated from the social sciences. This had to do with the quest for methodological rigor and for quantitative results. The term, introduced generally by the ever revised textbooks of Paul Samuelson (and William Nordhaus, 1992), *economics* reflects the discipline's self-delineation, a demarcation line against the soft and qualitative social sciences, that do not attribute the same significance to methodological rigor, as economic professionals do. Following Lucas, the quest for micro-foundations of macroeconomics, i.e. the need to base macro-analyses on micro-economic insights and results rests on two premises. One is the need to formalize presentation. The other is the consequent application of *methodological individualism*, most radically applied by the extension of microeconomic approaches to all human activities especially following the path-breaking work of Theodore W. Schultz (1973) and Gary Becker (1976 and 1996) (for an assessment cf. Lazear, 2000).

The self-contained – and often self-referential – evolution of mainstream economics resulted in a theory, capable of explaining erratic and chaotic changes via models, be that chaos or the stock exchange. On the other hand, insights from these theories are often at *conflict with practical exigencies*, most extremely shown by the 1998 autumn crisis of the Long Term Capital Management hedge fund, where the previous year's Nobel winner was a major analytical advisor.⁴ More problems emerged when academic insights were to be translated into universally valid policy packages, that were dubbed by John Williamson (1994, pp. 17–19) as the Washington Consensus. The per se valid insights had a long and arduous journey into policy implementation. As documented in our previous monograph, as well as in the *Transition Reports* of EBRD, there used to be a tendency to use abstract models and insights, applying under certain conditions and levels of abstraction only, as immediate policy guidelines. This has backfired. By the 2000s common wisdom, both in the academe and in the international financial institutions, has become aware of the need to take into account *the context* and of the fundamental role *institutions* play in shaping actual outcomes. For instance the role of fixed exchange rates cannot be understood in separation. Likewise private property though is in general more efficient than public or mixed forms of ownership, but only if side conditions for a long chain of bringing about efficiency is being observed (an issue we shall analyze in separate chapters). And the growing *literature on*

³ For a concise assessment of the terms 'counter-revolution' and 'death of the macro' cf. the excellent survey of Júlia Király (1998).

⁴ This paradox does not denounce Myron Scholes and Robert Merton. The LTCM was caught not so much by the Russian crisis proper than by its spill-over. As B. Pálosi–Németh called to my attention, LTCM did not hold Russian bonds, but had encountered a series of difficulties from the early months of the year. The spillover that was sudden and not justified by the size of the Russian market or the share held by Russian papers in total global investment portfolios, was the final drop in the fate of the hedge fund. The crux of the mis judgement was timing and location, not a matter of academic quality. More on this cf. (Edwards, 1999).

policy reform, stemming from the 1980s and 1990s has been confronted with the need to include institutions, policies and public preferences/choices, that have been often contradictory to what mathematical models would have suggested as rational. Only the works of Kahneman and Tversky (1979) awarded by a Nobel Prize in 2002, have provided scientific (psychology-based, empirically tested) observations on such issues as why risk-taking and risk aversion is *not* symmetric (as postulated by a model).⁵

Evolution of economics, as well as of its interaction with practice (i.e. testing of theories) have triggered a development that is most important from our perspective. The 20th century's academic development tended to de-emphasize institutional, historic and generally speaking contextual factors and stressed rigor and *analytical coherence, even at the cost of social relevance* (cf. more on that in Baumol, 2000). By contrast, experiences with policy reform in developing countries, as well as with the multiplication of policy failures in post-Communist transformation have *triggered a contrarian move*. The need to *integrate* the insights of other social sciences, of context and to create a better *interaction* with the policy-making and business community laid the groundwork for an approach that we call *new political economy*. As elaborated in detail elsewhere (Saint-Paul, 2000; Hibbs, 2001; Csaba, 2002) this approach does not question the validity of insight and relevance of the mainstream theories. It attempts, however, to *integrate these in policy-relevant insights*, and wishes to incorporate the feedback to and from the institutional and policy processes. This line of research has constantly been present in mainstream journals up to the point of analyzing electoral competition and the impact of heterogeneous pattern of districts on the outcomes (Collender, 2005).

It is interesting to note, that also in the more applied version of *international studies* literature a *similar turn*, away from abstract grand sociological theories of the 1960s, towards more economically informed analyses, based on rational choice models, and generally a view quite resembling to *homo oeconomicus* could be observed ever since the 1970s. In his overview of the postwar development of American political science and developmental literature, Richard Higgott (2004, chapters 2 and 4) describes in great detail the emergence of *new political economy as a discipline and an academic approach*. He calls it as a mixture of rationalist politics, public choice and policy analysis, that together have injected the previously missing economic component in the development debate from the early 1970s.

One may wonder if, and to what degree, this change of the heart implied an attempt at revenge, that is to re-conquer those fields where mainstream economics has been marching into, due to its immense ability to *generalize and formalize* questions (as well as suggesting at least testable and falsifiable hypotheses). The latter approach is present with a number of journals published under this name, such as *Review of International Political Economy*, or *New Political*

⁵ It is hardly surprising that an application of this empirical insight was extended to the workings of the capital markets, cf. Barberis, Huang and Santos, (2001). A broad analytical assessment of the work of Kahneman is offered by Hámori (2003).

Economy, or the *Review of International Studies* and more recently *Global Social Policy*. However, as the above quoted book of Higgott proves with a detailed literature review, the US brand of new political economy, having emerged in development studies, emphasizes though the importance of collective choices and institutions, still remains clearly within the liberal tradition (if for no other reason, because of its methodology). In our analysis the case for multi- or cross-disciplinary approaches can be taken as given.

It is equally important to note what William Tabb (1999) rightly calls as the *two cultures in economics*. In his broad review of the history of economic thought the above cited volume proves that besides the formalized mainstream, an equally powerful presence of institutionalism and structuralism could be observed over the past hundred years or so. Moreover many of the major breakthroughs in social sciences and economics have been produced by the heterodox approaches. Therefore, the clear dominance of formalized approaches in the leading journals and in professional pecking orders may represent more a *fashion trend* than a *sound academic judgement*, based on generally accepted methodology/scientometrics/of the sciences in general. If we were to judge by the number of citations, it would be hard to consider the influence of such authors as Mancur Olson, Friedrich August von Hayek, Lord Peter Bauer or Walter Eucken, Albert Hirschman or Amartya Sen as second rank, marginal or not having contributed to the evolution of what is considered to be the hard core of the profession. It is interesting to observe that in recent years 'outdated' institutionalist approaches regained academic respectability. In 2005 the Richard Ely lecture of the American Economic Association (AEA) was held by Oli Williamson (2005), the iconic figure of the old school. In 2006 a special section of the AEA annual meeting and of the *American Economic Review* was devoted to issues of political economy, and yet another section or issues analyzing lessons from China, India and Russia.

In venturing into new political economy, therefore we are not at all entering un-chartered waters. In the second half of the 20th century several *school-molding personalities* of the economics profession have contributed to this line of thought. Let us recall only the Nobel-winning oeuvre of James Buchanan (1992) and his public choice theory, called in the European continent as constitutional political economy. A rallying point of this line of thought, across the Atlantic, is the journal *Constitutional Political Economy*, where the Freiburg tradition of intensive interaction between law and economics is being sustained at a high professional level. The stronghold of European mainstream political economy, particularly international political economy is the Swiss quarterly, *Kyklos*, bravely declaring itself the only socially relevant economic journal, not written by referees and editors, but authors themselves. Similarly interdisciplinary orientation features two other international political economy journals, the libertarian *CATO Journal* and the understandably left-leaning two mainstream developmental fora, *Third World Quarterly* and *World Development*. Meanwhile the *Journal of Development Economics* is a stronghold of a variety of approaches applying mainstream techniques and analytical angles to issues of development. By contrast, the editorial policy of *The World Economy* defines it as a pol-

icy forum, though several academic papers have been published by the editors in the recent years. Several political economy papers have been published in the two leading journals of the American Economic Association, *The Journal of Economic Literature*, as well as the intentionally less technical *Journal of Economic Perspectives*. Yet another American quarterly, having changed editors and taken over by Palgrave, *Comparative Economic Studies* moved from area studies to political economy, and carries interesting contributions to the area.

Another Nobel-winner, Douglass North has been constantly stressing the importance of institutions in longer term economic development, as well as the interaction with political institutions. Likewise, from among the 2001 Nobel winners, George Akerlof (1970) and Joseph Stiglitz (1993) have long emphasized the consequences of *imperfect information* for the *applicability* of models based on full information and unlimited capacity to process these. And let us not forget one of the first Nobel-winners, Kenneth Arrow (1950), much of whose output relates to social costs, political selection and welfare issues. In recent years the expansion of 'economic imperialism' political economy analysis has come back to the mainstage. It may be sufficient to cite to recent contributions to the *Quarterly Journal of Economics*: one on the liberal bias of US media (Grosche and Milyo, 2005) and one on the causes of state religion (Barro and McCleary, 2005). Gone are the days when some colleagues could afford to disqualify these approaches as 'poetry', falling outside the scope of the economics discipline proper. Moreover the Nobel lecture of Ed Prescott (2006) comes back to the traditional way in demonstrating how new policy experience and formalized research methods have been interactive also during those decades, when it was not in vogue.

Many people would contest our choice. Neo-institutionalists, a school aiming to build a bridge between the methodological individualism and formalism of the mainstream on the one hand, and more traditional, historically informed institutionalists on the other, often try, without much success, to ally in one church in order to reintegrate broader approaches. Structuralist and neo-Marxian approaches try to do the same.

The term '*new political economy*' (NPE) in the sense we use it is reflecting the American developmental literature parlance, as explained in Higgott (2004). This usage is also akin to the German ordoliberal economic tradition. This has been sustained primarily around two yearbooks – *ORDO* and *Jahrbuch der Neuen Politischen Ökonomie*. In so doing, a liberal approach to *economic systems* is combined with historically informed institutional analysis. Furthermore, matters of *public choice* (such as the choice between a high tax – high service versus a low tax – low service economy) are being treated as *endogenous* variables, rather than exogenous factors, as in the mainstream. While any choice is contestable, in my view the term best conveys to the English speaking audience of what can be expected. The up until recently marginal position of institutionalism in the English speaking academic economics is an additional argument against the usage. In his presidential address providing a broad overview of the area Paul Joskow (2003) of MIT singled out *public policy applications among the weak points* (or limited success areas) of new institutional economics.

In particular he listed economic development, corporate governance, globalization and network liberalization among the issue areas, where the shortcoming is manifest – and these are crucial for the focus of this book. New political economy, by contrast captures exactly these features. This is also the research project of Yale professor T. N. Srinivasan, a leading figure of development economics. His term ‘neoclassical political economy’ (Srinivasan, 2001, p. 510 and 522) implies basically the two salient features of our approach: *endogenizing the state* and collective action, as well as introducing these in the *standard analysis* in order to produce policy relevant and interesting new outcomes.

Finally we must admit, that many scholars, also in the English speaking world, use *NPE* in a different meaning. Scholars of *international relations*, having fought a long time the dominance of the ‘neo-liberal paradigm’ attempted to regain ground by an alternative called ‘political economy’ (a good quality example is Gilpin, 2001). This approach is, by and large, a reinvention of classical political economy, or, in most other cases, constitutes an attempt to apply *political science paradigms on economic matters*. Understandably, their findings tend to be at odds with those of the economics mainstream, though not necessarily implying a ‘turn to the left’. If for no other reason, the different methodological approach allows for a *limited, if any interaction with our understanding of new political economy*, as defined above in terms of a subchapter of broadly defined general economics discipline. Neo-Marxist authors gathering around the journal *Review of International Political Economy*, as well as radical/critical analysts contributing to the Carfax journal *New Political Economy* produce completely different pieces on different paradigmatic grounds.⁶ The eldest journal, carrying such a name, *The Journal of Political Economy* is being edited in Chicago, and unsurprisingly, has evolved into a typical mainstream journal over the past four decades. On the other hand, the *European Journal of Political Economy*, published by the Dutch academic publishing house Elsevier, brings articles that could be denoted in German parlance the economic theory of politics. Rather than working on the interaction of economic and political processes, authors of this journal adopt the mainstream economic angle and arsenal, analyze political processes, such as pension reform or tax evasion through the lenses of the mainstream paradigm. This approach adopts *one level higher abstraction* in analyzing issues than we do. Our choice, not competing with any of the former, but complementing these, attempts to highlight *developmental, regional and post-Communist specific features* of economic processes. However, since there is no single ‘right way’, the only sincere method for us is to remain straightforward in stating our aims and scope also in terms of using the core term *new political economy* in ways and for the purposes described above.

⁶ In the former pieces what I would call institutionalist modelling, in the latter radical policy analyses are offered; both legitimate on their own right, still being a different genre from our output.

Why Not Eastern Europe/Transition Economies?

Finally the term *Emerging Europe* needs to be defined. As in so many other instances, new concepts evolve with the resurgence of new phenomena. Following the Second World War, development economics emerged as a revolutionary sub-discipline, swimming against the tide of contemporary mainstream (cf. the broad overview of Waelbroek, 1998; Szentes, 2002). Reflecting the normative approach of the '40s and '50s the new states having emerged on the ruins of colonialism tended to be called 'developing' countries. However, their problem, as described in a voluminous literature, has remained to date *underdevelopment*,⁷ i.e. the inability to enter a path of financially, environmentally and socially sustainable growth. Reckoning with this fact, from the 1960s, also reflecting the non-aligned movement of Nehru, Nasser, Nkrumah and Tito, the term 'third world' was introduced. However, in the 2000s with the irreversible demise of the bipolar system, the term 'third world' is a hollow one. *Non-OECD countries differ more substantially from one another than they are unlike of OECD economies*. Furthermore, differentiation among non-Atlantic countries is much bigger than most analytical terms would allow (such as 'developing country', with Korea, Turkey and Mexico already being members of the OECD).

Facing realities, the financial literature of the 1980s introduced the term *emerging markets* for those non-Atlantic nations, where development was *not just a polite diplomatic euphemism for backwardness*. Especially following successful stabilization in countries like Chile, Spain, Israel, Mexico, coupled with the growth miracle of East Asia in the 1970s and '80s it made no business sense to lump up decaying and disintegrating quasi-states with the ones having registered the highest growth rates globally, owing to their outward looking industrialization policies. Therefore this term reflects a *long run growth*, potential and real, but by no means cyclical in nature. In emerging markets, to put it shortly, the *trend rate of growth is palpably above the average of OECD countries*. Therefore, for them catching up is not just a mirage, a promise too frequently misused by nationalist politicians for the sake of state building (while disregarding the preferences and interests of the poorest and the middle-classes alike).

In a similar vein, it is high time to get rid of the term *transition economies* (in some extreme neologisms we could read even transitional economies, that imply, grammatically the ephemeral nature of these). One of our fundamental hypotheses, to be proven across the present monograph, is that the 28 – and with the independence of Kosovo and Montenegro already 30 – post-Communist countries cannot and *should not be lumped into a single analytical category*. The Communist legacy, that used to mold their common experience fifteen or twenty years ago, no longer count as decisive. Some of them are integrated in

⁷ As Higgott (2004, Chapter 3) rightly emphasizes, the theory of underdevelopment emerged as a reaction to both neo-Marxist and liberal views, but received moderate attention and appreciation in mainstream academe. More on that in Szentes (2002).

the EU, others disintegrate into smaller entities in Southeast Europe and the Caucasus (perhaps also in Central Asia). Yet some other countries follow specific, national models of capitalism (that is count among the less successful developing nations).⁸ As we shall show in the next (and following) chapters, even if we disregard the entirely different experience of China, these countries have shown *markedly different trajectories of development* over the past 15 years and longer. These have to do with path dependency, culture, policies, geography and many other matters. *In proper perspective, the Communist interlude of 40 or 70 years can hardly be taken as the sole defining moment of history.*

As a consequence, Kyrgyzstan is unlikely to be more similar to Slovenia, than to other Central Asian countries, only because of several decades of Russian rule. It is common knowledge, that the Soviet structures, imposed arbitrarily from above since the late 1920s, failed to create post-capitalist conditions. Nowadays the Central Asian societies struggle against typical features of *post-colonial situations*, such as dual society, nation building, captive state, corruption and despotism. These are clearly *not* the problems faced by the Central Europeans. With the time passing these *differences are likely to increase, rather than decrease*. Therefore it may be plausible to suggest at this point that divergence is likely to be the defining feature of the times to come; a point that is to be cross-checked across the various chapters, empirical and theoretical alike.

If there is a modicum of truth in our hypothesis, it does make academic sense to apply a new analytical category, *emerging economics*. This term stresses the commonality of issues in terms of market reforms and democratization both with *Southern European*, now EU nations like Spain, Portugal and Greece, as well as with *non-European* countries having entered a phase of sustaining growth as a trend, such as Chile, Mexico, Taiwan, Korea. The big challenge for the coming decades will see the following: if how and when the countries among the Mediterranean basin, starting up with Turkey and Morocco, can transform themselves in emerging economies.

Thus *the focus of this book* is, like that of its predecessors, what once used to be called, mistakenly, but then politically correctly, Eastern Europe. *The angle of analysis* is the global economy. Today the defining processes, such as the information and communications technology (ICT) revolution, the new economy, the evolution of such previously unknown forms of organization as the virtual firm or the virtual bank, are added to the centuries-old processes of industrial, trade, financial and organizational intertwining, that carry most recently the misnomer of *globalization*. While disagreeing with the over-emotional and politicized uses, we may also refer to this as a shorthand form in describing/alluding to the many novelties that together constitute *a brand new environment* for an ever growing competition among localities.

In the following pages we attempt to provide empirical evidence and theo-

⁸ For a regular comparative overview of facts and figures proving this point in great empirical detail cf. *Transition Report, 2006*. London: EBRD and *Economic Survey of Europe*, no. 2. (2005). Geneva and New York: a publication of the UN Secretariat, that was regrettably discontinued whit the cited issue.

retical insights of *what has changed* and contribute to solving our three puzzles listed above. If we manage, it might be a contribution to a better understanding of both the *region* and the potentials (and limitations) of *economic analysis*. Many of the chapters emerged from various conference initiatives, from which I benefitted, especially from open exchanges with my colleagues. Still I do hope that *an overall structure* emerges, that proves the points raised. The book contains original research and I hope to contribute to the international debate on these contentious issues, not aspiring for a 'final say' or a 'defining moment' in offering my insights for critical commentary of the readers.

The Map of the Project

The present monograph starts in Part One with the comparative overview of *previously available evidence*. This is done in two parts: empirical and theoretical. Starting with *statistical* evidence and its interpretation we can address some of the traditionally controversial issues of transition theories. We can assess progress and failure and test the empirical and policy relevance of competing theories. This long-term view allows for grouping the transition economies and answer the question if the Communist legacy is still dominant. Following up this overview we try to put the transition experience in a broader perspective of *development*. This stresses the role of institutions, the quality of regulations and the importance of external anchors, as well as of consensus building.

Part Two is devoted to *global issues*. This is an attempt to overcome the somewhat parochial nature of many transition analyses that tend to remain hostage to traditional area studies approaches. In so doing the challenge of the ICT revolution as well as the various processes transgressing the border and the regulatory capacities of nation states come to the fore. We address the interaction of global, regional and local processes in bringing about policies and outcomes. We also ask if and *to what degree European integration*, the present arrangements of the EU, allow for meeting its Lisbon goals, that is becoming *the most competitive community of the globe*. We find that the two challenges overlap only partly. By the same token, so the argument in the chapter on the limits of EU driven transformation, new member states must take advantage of the *specifics of Europeanization*. The latter means that instead of the previously dominant copying new, innovative and constructive approaches are needed both at the national and European levels. Two chapters are devoted to the Stability and Growth Pact (SGP), an institutional reflection of economic insights on the uses of the rules-based conduct. It has come under severe criticism both from old and new member states. The present monograph addresses this controversy from the point of view of long-term growth and finds that *monetary and fiscal solidity allow for growth to sustain for long periods*, that is for decades rather than months. And this is precisely what is the task to be mastered by the new members through their adoption of the accomplishments of ICT and institutional innovation alike. In this context we address in a new chapter the impact of EU enlargement to its broader neighborhood. How far can Europe be stretched, and under what conditions?

Having drafted what is seen as the mainstream of development in Part Two, the following Part Three is devoted to analyzing two *control cases* that seem to defy all economic theories, that of Russia and China. Russia experts tend to be absorbed, up until today, in the view dating back to the Slavophile writer Tutchiev claiming that "Russia is by definition unfit for rational reflection". By contrast we claim to be able to interpret Russian stagnation under the Presidency of Boris Yeltsin and recovery under Vladimir Putin in terms of standard economic theories. In our China chapter we are looking for analytical answers to the mystery of sustaining economic growth despite the fact that China follows unconventional policies and disregards the role of formal institutions, considered to be focal in the post-Washington Consensus and in modern economic theory alike. Putting the Chinese experience *in comparative perspective* we try to offer a solution in terms of conventional growth theory, claiming that *there is no Chinese puzzle*. Countries with a potential for major structural changes in favor of more productive activities do have a chance for lasting growth as long as this one-time factor sustains, that is for a decade at least.

Having addressed the case and counter-case, Part Four is on the potential *theoretical inferences*. Here some of the basic policy relevant issues of economics are being addressed. In one chapter the issue of *privatization and regulation* is being addressed. The more the market is seen as an institution, the more important the quality and the form of regulation becomes. Relying on the experience of OECD countries and new public management literature the argument comes up in favor of more competition/market coordination in ever growing areas, while sustaining regulation that allows for transparency and observance of the public good. Finally the big theoretical questions of *how exactly institutions matter for growth, which of them and through what mechanism* are being addressed. In this summary chapter the experience of frontrunner transforming economies and those of other OECD economies are being compared. The importance of *formal* institutions in turning the market a modern and *civilized* arrangement is being underscored.

As can be seen, the attempt is to produce an *overview of major issues*, even without trying to be exhaustive. Not only constraints of time and space, but also the open-ended nature of development calls for temporary, rather than definitive conclusions. Such unexpected crises, as the 1997–99 financial contagion, the burst of the IT bubble, or the implications of software terrorism have shaken the once over-confident mood of analysts. I am sure that the present overview is only one among the possible competing interpretations. If this revised book further triggers professional controversy in the spirit of *sine ira et studio*, all the better.

In writing the monograph I have benefited from the excellent facilities, the library and the intellectual aura of the Central European University. Discussions with my colleagues at the Department of International Relations and European Studies allowed me to improve many shortcomings of the original version of the individual chapters of this book. Most of these have additionally been presented for critical comments to one or more international con-

ferences in Hungary and abroad. The intensive feedback I received from participants allowed me to overcome a number of shortcomings of the original/first/second drafts.

I have profited from working in a multi-disciplinary environment at the CEU with highly motivated post-graduate students in a culture of controversy. My special thanks go to Julius Horvath, Erin Jenne, Róbert Becsky, and Dóra Gyórfy, who have read various chapters of my piece line by line, and provided regular critical feedback on style and substance. With the usual caveats their support is greatly appreciated. The publisher improved my text significantly through careful editing. My special indebtedness is due to the late József Kormányos. Some other colleagues working on the specific fields volunteered to read and comment chapters related to their area of specialization. I benefited from these and tried to accommodate most of the criticism that I could. I also benefited from working with my colleagues and PhD students at the Faculty of Economics and Business at the University of Debrecen. While trying to accommodate the inflow of new pieces of information and critical commentary alike, I hope to have managed to keep the structure of the argument of the monograph clear-cut and self-contained. Last but not at all least, the devotion of my family (my wife, Csia/Gabi, our kids, Zoltán and Hedgehog/Orsi) has been a source of energy and inspiration, which I can hardly reciprocate.

Budapest, 28 July 2004

References

- AKERLOF, G. A. (1970): The market for 'lemons': quality uncertainty and the market mechanism. *Quarterly Journal of Economics*, vol. 84. no. 3. pp. 488–500.
- ARROW, K. J. (1950): A difficulty in the concept of social welfare. *Journal of Political Economy*, vol. 58. no. 2. pp. 328–346.
- BARBERIS, N. – HUANG, M. – SANTOS, T. (2001): Prospect theory and asset prices. *Quarterly Journal of Economics*, vol.116. no.1. pp. 1–53.
- BARRO, R. – MCCLEARLY, R. (2005): Which countries have state religions? *Quarterly Journal of Economics*, vol. 120 no. 4. (nov) pp. 1331–1370.
- BAUMOL, W. (2000): What Marshall did *not* know: The contribution of the 20th century to economics. *Quarterly Journal of Economics*, vol. 115. no. 1. pp. 1–44.
- BLANCHARD, O. (1997): *The Economics of Post-Communist Transition*. Oxford: Oxford University Press.
- BECKER, G. (1976): *The Economic Approach to Human Behavior*. Chicago and London: The University of Chicago Press.
- BECKER, G. (1996): *Accounting for Tastes*. Cambridge/Mass: Harvard University Press.
- BOGÁR, L. (2006): *Bokros újratöltve* (Bokros refilled). Budapest: Kairosz Kiadó.
- BRABANT, J. van (1998): *The Political Economy of Transition*. London and New York: Routledge.
- BUCHANAN, J. M. (1992): *Piac, állam, alkotmányosság* (Market, state and constitutionalism). Budapest: Közgazdasági és Jogi Könyvkiadó.
- COLLINS, S. and RODRIK, D. (1991): *Eastern Europe and the Soviet Union in the World Economy*. Washington D. C.: Institute for International Economics.

- COLLENDER, S. (2005): Electoral competition in heterogeneous districts. *Journal of Political Economy*, vol. 113. no. 5. pp. 1116–1145.
- CSABA, L. (1984): *Kelet-Európa a világgazdaságban*. Budapest: Közgazdasági és Jogi Könyvkiadó – revised and extended English edition was subsequently published under the same title as: *Eastern Europe in the World Economy*. Cambridge and New York: Cambridge University Press, 1990.
- CSABA, L. (1994): *Az összeomlás forgatókönyvei*. (Scenarios of a collapse). Budapest: Figyelő Kiadói Rt. – an English version was published under the title *The Capitalist Revolution in Eastern Europe*. Cheltenham/UK and Northampton/USA: Edward Elgar Publishing Co., 1995.
- CSABA, L. (2002): Transformation as a subject of economic theory. In: BÖNKER, F. – MÜLLER, K. – PICKEL, A. eds: *Post-Communist Transformation and the Social Sciences: Cross-Disciplinary Approaches*. Boulder, Colorado/USA: Rowman and Littlefield, pp. 39–58.
- DJANKOV, S. and MURREL, P. (2002): Enterprise restructuring in transition: A quantitative survey. *Journal of Economic Literature*, vol. 40. no. 3. pp. 739–792.
- EDWARDS, F. (1999): Hedge funds and the collapse of LTCM. *Journal of Economic Perspectives*, vol. 13., no. 2. pp. 189–210.
- FALCETTI, E. – RAISER, M. – SANFEY, P. (2002): Defying the odds: initial conditions, reforms and growth in the first decade of transition. *Journal of Comparative Economics*, vol. 30. no. 2. pp. 229–250.
- GILPIN, R. (2001): *Global Political Economy: Understanding the International Economic Order*. Princeton: Princeton University Press.
- GROSECLOSE, T. – MILYO, J. (2005): A measure of media bias. *Quarterly Journal of Economics*, vol. 120 no. 4. pp. 1191–1238.
- HANSON, Ph. (2003): *The Rise and Fall of the Soviet Economy (1945–1991)*. London: Pearson Education.
- HÁMORI, B., (2003): Kísérletek és kilátások – Daniel Kahneman. (Trials and prospects – D. Kahneman). *Közgazdasági Szemle*, vol. 51. no. 9. pp. 779–799.
- HIBBS, D. (2001): The politicization of growth theory. *Kyklos*, vol. 54. no. (2–3), pp. 265–286.
- HIGGOTT, R. (2004): *From Colonialism to Globalization: a Genealogy of Extraterritorial Politics*. Cambridge: Cambridge University Press.
- JOSKOW, P. (2003): ‘New institutional economics: a report card’ – plenary closing address of the outgoing President to the 7th convention of the International Society for New Institutional Economics, entitled ‘*Institutions and Change*’, Budapest Corvinus University, 11–13 September.
- KAHNEMAN, D. and TVERSKY, A. (1979): Prospect theory: An analysis of decisions under risk. *Econometrica*, vol. 47. no. 2. pp. 263–291.
- KIRÁLY, J. (1998): A makroökonómia vége, avagy egy megkésett Nobel-díj. (The end of macroeconomics, or a belated Nobel Prize). *Közgazdasági Szemle*, vol. 45. no.12. pp. 1082–1095.
- KOLODKO, G.W. (2006): *The World Economy and the Great Post-Communist Change*. New York: Nova Science Publishers
- KÖVES, A. (1992): *Economic Transformation in Eastern Europe: The International Dimension*. Boulder, Colorado/USA: Westview Press.
- KRUEGER, A. ed. (2000): *Economic Policy Reform: The Second Stage*. Chicago and London: Chicago University Press.
- LAVIGNE, M. (1995): *The Economics of Transition*. Basingstoke and London: MacMillan.
- LAZEAR, E. (2000): Economic imperialism. *Quarterly Journal of Economics*, vol. 115. no. 1. pp. 99–146.
- LUCAS, R. E. (1996): Nobel lecture: Monetary neutrality. *Journal of Political Economy*, vol. 104. no. 3. pp. 661–682.

- MEIER, G. and STIGLITZ, J. eds (2001): *Frontiers of Development Economics: The Future in Perspective*. Oxford and Washington: Oxford University Press for the World Bank.
- MISES, L. von (1920/1976): Economic calculation in the socialist commonwealth. In: NOVE, A. and NUTI, M. D. eds (1976): *Socialist Economics: Selected Readings*. Harmondsworth/UK – New York – Auckland: Penguin, pp. 75–91.
- OFER, G. (2001): Development and transition: emerging but merging? In: Ten Years of Transition in Eastern European Countries. *Revue d'Économie Financière*, special issue, no. 10. pp. 107–145.
- PORTES, R. (1991): The path of reform in Central and Eastern Europe. *European Economy*, special issue, no. 45. pp. 1–23.
- Prescott, E. (2006): The transformation of macroeconomic policy and research. *Journal of Political Economy*, vol. 114. no. 2. pp. 203–235.
- ROLAND, G. (2002): The political economy of transition. *Journal of Economic Perspectives*, vol. 16. no. 1. pp. 29–50.
- SAINT-PAUL, G. (2000): The new political economy: Recent books by Drazen and by Persson and Tabellini. *Journal of Economic Literature*, vol. 38. no. 4. pp. 915–925.
- SAMUELSON, P. and NORDHAUS, W. (1992): *Economics*, vols I–III. New York: McGraw-Hill.
- SCHULZ, Th. W. (1973): The value of children: an economic perspective. *Journal of Political Economy*, vol. 81. no. 2. part 2 (March–April), pp. S2–S13.
- SRINIVASAN, T. N. (2001): Neoclassical political economy, the state and economic development. In: SRINIVASAN, T. N.: *Economic Policy and State Intervention*. (edited by V. S. Narayana). Delhi: Oxford University Press, pp. 504–530.
- STIGLITZ, J. E. (1993): *Economics*. New York: W. W. Norton.
- SZENTES, T. (2002): *A Critical Theory of International and Comparative Economics*. Budapest: Akadémiai Kiadó.
- TABB, W. K. (1999): Two cultures in economics. In: TABB, W. K.: *Reconstructing Political Economy*. London: Routledge, pp. 1–16.
- WAELEBROEK, J. (1998): Half a century of development economics. *World Bank Economic Review*, vol. 12. no. 2. pp. 323–352.
- WILLIAMSON, J. (1994): In search for a manual for technopols. In: WILLIAMSON, J. ed.: *The Political Economy of Policy Reform*. Washington, D. C., Institute of International Economics, pp. 9–47.
- WILLIAMSON, O. (2000): The new institutional economics: Taking stock, looking ahead. *Journal of Economic Literature*, vol. 38. no. 3. pp. 595–613.
- WILLIAMSON, O. (2005): The economics of governance. *American Economic Review*, vol. 96, no. 2. pp 1–18.