Postcommuni Transformatio and the Social Science

CROSS-DISCIPLINARY APPROACHES

edited by
Frank Bönker, Klaus Müller,
and Andreas Pickel

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Chapter 2

Transformation as a Subject of Economic Theory

László Csaba

After a decade of systemic changes in central and Eastern Europe there is a growing pessimism among specialists and the general public, both on the subject of transformation as such and on the ability of the theorists to deliver anything meaningful. This applies a fortiori to the economics profession, where a peculiar "impossibility theorem" postulates theoretical economics' inherent inability to say anything meaningful about reality. And not only the uninformed public is ready to adopt such a defeatist stance. After my presentation of an earlier version of this paper to a research seminar at a large European university, the dean of the economics department commented: In the end, there is not much to be discussed, since postcommunist transformation and economic theory are unrelated. On another occasion yet another distinguished professor noted: Economists nowadays are like philosophers, they do not care much about reality. They prefer their self-contained theoretical constructs, and the art is in discussing minor modifications within the high guild.

But there is a feeling of discontent also among area specialists. Here we find accounts of traditional Sovietology that praise its accomplishments yet simultaneously concede the field's inability to predict the collapse of the Soviet system (let alone the sequence of events) (Schroeder 1995). This is certainly an analytical flaw and reflects a tradition of putting more emphasis on the collection of facts and figures than on the analytical and theoretical frame in which these are integrated. The decisive academic issues clearly are not the size of Soviet GDP or the share of defense spending, but the reasons for the collapse of

Communism and the nature of the crisis that triggered the regime change.

Understandably, there has been much irritation from the very outset over the "intrusion"of outsiders into this area. They have been criticized for a large number of fallacies, most prominently for having imposed their academic culture and inherited preoccupations on the political consultancy and decision-making process (Murrell 1995b: 169; see also chap. 4). This feeling has evolved into bulky volumes and vitriolic pieces that speak of an all-embracing neoliberal party line (Lavigne 1995: 248f.), blame neoliberalism for being irrelevant on growth and modernization issues (van Brabant 1998: 462, 477–79), caution against a naïve minimalist approach to state interventionism (Przeworski 1997: 412–15, 427f.), or attribute the alleged regionwide recession to a "hands-off" approach (Poznanski 1999; see also his contribution to this volume). The list could be extended at will by contributions by authors from the region itself. Most recently, Joseph Stiglitz (1999), the former chief economist and senior vice president of the World Bank, accused international agencies and advisors of having imposed their noninstitutional and one-sided visions on policies of transition countries, thereby making already bad things worse.

This chapter tries to overcome this obviously unproductive confrontation. My basic thesis is that economics, when interpreted in sufficiently broad terms, can contribute meaningfully to the interpretation of facts and figures, can provide an analytical frame for arranging them, and can even suggest some lines of action for policy-making. This does not mean that economics—or any other discipline—can claim exclusivity. The subject of analysis is a multidimensional and, in many respects, open-ended socioeconomic process, so that the use of different approaches is justified. However, both methodological considerations and practical experience caution against too much eclecticism or the mixing of competing paradigms from various social science disciplines. As Jan Prybyla (1998: 359) aptly put it, "One should be sceptical about one-size-fits-all constructs but also shun deconstructivist academic woolliness for which everything depends on everything. Attempts trying to present a theory of everything and in the end, of not very much at all, are not a suitable substitute for even an inadequate economic theory." In other words, while sociological, economic, and political science or social anthropology perspectives may well never coincide, this frequently does not reflect more than different viewpoints—just as the same nude can be painted from the back, from the side, or face-to-face. Thus in what follows I adopt a consciously one-sided, narrow economic approach and try to figure out what this discipline has to contribute to our understanding of systemic change and, conversely, what economics can learn from the postcommunist experience.

Economics and/in Reality

It is an interesting subject of philosophy and methodology what is, or is not, a science, who does or does not qualify as an economist, or which scholar has contributed to an area. Both in general and even more *in concretu* this poses difficult questions. Just to take the best-known examples: Adam Smith, the founding father of the discipline, was a professor of ethics and was considered mostly a philosopher, and so was Friedrich August von Hayek. Gunnar Myrdal has been seen as a general social scientist and policy advisor, James Buchanan as a political scientist, Myron Scholes and John Harsányi as mathematicians, Arthur Lewis as a "developmentalist,"Amartya Sen as a moralist and nutritionist, Robert Fogel as a statistician, and Douglass North as an economic historian.

Nevertheless, as the statement by the dean of the economics department cited above highlights, there is a clear tendency to narrow the field. It would have seemed easy to disqualify this approach, had it not been for the entire post-war history of economic thought from which formalization and methodological preciseness emerged as the dominant trend. This is well reflected in what is usually seen as the current encyclopedia of knowledge, the four-volume *New Palgrave* (Eatwell et al. 1987). The latter does not include any reflection on systemic change. This is partly due to the time of publication. However, even a second edition is unlikely to contain a heading on transformation. It is the genre itself that has substantially different concerns.

Reviewing the opus magnum in the *Economic Journal*, R. E. Bailey (1994: 661) noted that "the late twentieth century 'student,' it seems, should have the benefit of at least one year's training in economic theory and econometrics beyond the bachelor's level, in order to be at ease with the full range of articles contained in these volumes. These are dictionaries aimed at academic professionals, not educated laymen seeking enlightenment on some obscure piece of professional jargon. Nor are they likely to satisfy undergraduates demanding instant gratification in the rush to complete a late assignment." Further: "for the general reader much of the economic analysis will remain a mystery . . . the sheer weight of technicality provides a distorted view of contemporary economics . . . or more probably, it is the great preponderance of theory relative to application which gives the impression that the entries are unduly technical" (665f.). But later on the same reviewer finds entries on analogy, induction, relativity, and even ideology, which "serve to remind one of the freedom of a pluralistic methodology enjoyed by economics. The evidence of these works is that it is a broad church indeed, and in spite of (or perhaps because of) its manifest failings as a science, it is unlikely to be forced into any oppressive methodological straight-jacket" (667).

A similarly ambivalent picture emerges if one looks at "leading" economic journals. Against the background of a narrow self-interpretation of what economic theory is and should be, it is truly surprising to see a hundred flowers flourish, both in doctrinal and methodological terms. While the tastes of the mainstream and the related promotional practices do have a strong impact, it is hard to ignore the large number of applied research and of nontechnical articles, as well as the presence of several schools of thought. In addition to the *Journal of Economic Literature*, which has a long tradition of publishing survey articles of an entire subject matter for outsiders in a fairly nontechnical fashion and earned a very high reputation for this, the American Economic Association established the *Journal of Economic Perspectives* with the expressed aim to build a bridge between academics and practitioners and to foster mutually beneficial interchanges. For anyone following the papers and proceedings published in the *American Economic Review* it would be easy to cite a large number of pieces on "real-world problems"—from economic growth and financial market problems to social security reform. Likewise such standard-setting European journals as *Economic Journal* in Britain, *Kyklos* in Switzerland, and *Weltwirtschaftliches Archiv* in Germany all exhibit a clear move toward empirically relevant and preferably accessible pieces. It is the expressed editorial policy to downplay technical submissions presenting minor refinements of previously established models. In sum, the omnipresent and almighty mainstream seems to be a strawman.

Aside from differences in subjects, differences in approaches must be taken into account. The strongest alternative to the mainstream is, of course, institutional economics. Institutional economics has two main lines: the old and the new, which differ basically in their perceptions of the mainstream. While the old institutionalism was basically hostile to formalization and the idea of methodological individualism, new institutionalist approaches attempt to use the analytical techniques that have been developed by the mainstream to address organizational and institutional issues that have fallen victim to the mainstream's level of abstraction. Contrary to the strong words normally heard in introductory courses, these two schools are strongly present in the first-rate economic journals and publishing houses. This is more surprising in the case of the old institutionalist school, but the finding is amply documented in the exhaustive reviews by W. Samuels (1995) and G. Hodgson (1998).

Institutionalism represents the renaissance of heterodox approaches in economics. These approaches emphasize the priority of organizational structure in allocating resources and the central role of cumulative causations in explaining processes. "The economy is understood as a system encompassing more than the market and undergoing systemic evolution, in part due to institutional and technological change, and, *inter alia*, the factors and forces actually operative in the economy" (Samuels 1995: 572). While formalization inevitably focuses the attention of analysts on discrete states and their characteristics, the old institutionalists see the economy fundamentally in terms of process. In addition, they are also interested in legal structures and their impact on economic processes.

Because of its broader, evolutionary approach, old institutionalism tends to downplay quantitative evidence and the entire focus on equilibrium solutions that dominates formalized thinking. While considering the state of equilibrium as a highly restricted case of the process they analyze, adherents of this school normally highlight the insurmountable difficulties in specifying comprehensive real-world situations in quantitative models and think that simplifying assumptions, made for the sake of computability, may be crippling (Hodgson 1998: 187). Old institutionalists also stress that the quest for quantitative evidence is somewhat anachronistic, as if we still lived in the nineteenth century, when mechanics was considered as the great science, or in the pre-1989 United States, when the military industry defined the best research and development options. By contrast, both money and attention have been refocused to biology, which is good news for the evolutionary approaches.

New institutionalists, as R. Coase (1998: 73) recently explained, are sharing the old institutionalism's unease about the excessively technical orientation of the mainstream. However, they do accept the rational actor model and the idea of maximizing behavior, but are interested also in different coordination mechanisms. Therefore, this line of inquiry has taken great interest in property rights and in the procedures of collective decision making. While old institutionalism is holistic and antiformalist, new institutionalists accept the relevance of formalization. However, they treat historical and structural explanations as complementary rather than opposed to mainstream economics. As two authoritative monographs on these attempts have documented, the marriage is quite fruitful (Rutherford 1994; Pejovich 1995). The cost of heterodoxy is a lower level of abstraction and less general conclusions, which actually turns out to be a benefit in applied areas.

Heterodoxy includes several complementary approaches. One is evolutionary economics, which some consider a branch of institutionalism. While the borderline is anything but clear, evolutionary approaches are much less concerned with the institutional framework than with the market process. Major characteristics of these approaches are the idea of natural selection, the focus on self-generating processes and the interest in the forces that determine the capability of adjusting to a basically hostile external environment (Nelson 1995). Given its emphasis on the natural selection process as opposed to man-made optimal design, evolutionary economics has a great deal in common with the older Austrian school.

Other heterodox approaches address the way decisions are taken and priorities set. Both processes tend to fall outside the focus of formalized analyses, which tend to take them as exogenously given. A. Dixit (1996) has brought together the transaction costs approach and policy analysis by focusing on policy formulation and policy implementation in a multiple principal–agent framework. In his analysis, the multiplicity of principals is the source of an inherently blurred system of incentives.

Economics also can, and indeed is, concerned with the ways preferences are formed. In Amartya Sen's social choice framework, rationality is not reduced to the postulates of *homo oeconomicus*, but includes prudence and even generosity (Sen 1995, 1999). Preferences are not necessarily formed in a utilitarian fashion by ordering gains. Instead states of mind, freedoms, and capabilities of living according to one's preferences must be considered. This aspect, which has also been highlighted by Bruno S. Frey (1997), helps us to better understand the formation of preferences. As Sen himself notes, the social choice approach may be particularly helpful in modeling decisions that supersede microeconomic rationality, e.g., those pertaining to the environment.

This far from exhaustive review of some of the more interesting trends in theoretical economics may suffice for the present purpose. It shows that the international agencies which are ritually targeted by critics of transformation are not the fountain of economic knowledge. Furthermore, the oversimplified world of an introductory microeconomics textbook does not represent the state of art currently available in the profession. Lamenting the neoliberal straitjacket, the imperialism of formalization or the one-dimensional economic man thus means fighting a strawman. Such claims, which feature quite prominently in part of the transition literature, reflect a lack of information at best, and manipulation at worst. Analyses by mainstream economists themselves forecast a revival of an institutional focus and a stronger empirical reasoning at the cost of the previously fashionable deductive-normative approaches (Colander 2000: 130–32). And no lesser authority than Robert Solow (2000: 152–54) cautions against too strong an emphasis on trying to formalize and optimize everything.

Transformation: Theory and Evidence

The above sketched patchy summary of economic theory has indicated an agenda quite different from the one cultivated in the transition countries. Many of the topics that have dominated policy debates in Eastern Europe over the last decade have been misperceived, have lacked relevance, or have been purely ideological in nature. In particular, they have born little or no relationship to the broadly defined subject of economics—i.e., improving well-being by managing scarce resources. From this perspective, the controversy over shock therapy versus gradualism, the idea that any country can opt for a freely chosen pattern of governance, the quest for optimal privatization technologies, the alleged benefits of protectionist policies, and many other subjects seem strangely insignificant.

With the benefit of hindsight, the controversy over the costs of stabilization or the calls for industrial policies have missed the point. I have not come across any modeling that would have offered a credible alternative path or development pattern against which the actual costs of transition could be measured. True, there is no shortage of claims that transition costs were excessive. However, these claims are mere reflections of subjective value judgments, that is, political rather than academic statements, since their originators are usually not able to specify what costs could be considered acceptable at the margin and how such cost reduction could be achieved. References to absolute magnitudes are obviously meaningless, while references to other countries beg the question of comparability. For instance, the idea of deliberate industrial policy or a thickly knit social safety net presupposes a strong state that has simply been nonexistent, since the crisis of the Soviet empire has been triggering the entire process. Had there been a strong Soviet state, most of the former socialist camp would have, willy-nilly, still continued to experiment by tinkering with various sorts of market socialism, instead of trying—and probably often erring in—various modes of transition to some brand of free market capitalism.

If transformation is the child of crisis, traditional sovietological comparisons of various goals and plans could hardly serve as the best measures to evaluate governmental success or failure, let alone the progress of the entire process—especially since many of the most relevant aspects of the market order emerged spontaneously, quite outside the framework of formal institutions and legislation. This is most striking in the case of the expansion of the private sector. In this area, stagnation of the grand—and most contested—privatization projects could neatly coincide with a blossoming of new start-ups, as well as with a creeping process of spontaneous privatization. The speed of actual—not purely formal, bookkeeping—change in ownership patterns and restructuring did not prove to be among the independent, freely calibrated variables, the magnitude of which might have been a matter of deliberation.

Likewise, the rate of growth, one of the most important macroindicators, could not be set at will, as in a modeling excercise. It proved to be the outcome of a series of intended and unintended processes triggered by governmental actions and spontaneous processes, external and internal factors. If this is so, the very idea of a growth-generating policy seems to be misplaced. Governments do not, even in theory, possess the means whereby they could be efficient and thus could be held accountable for mastering a task that is in any case not theirs alone. While many of those trained in observing the rituals of the planned economy would interpret the predominant spontaneity as a sign of crisis, or as chaotic, adherents of the evolutionary or Austrian schools would consider it as perfectly normal, since the market economy is the only order that is the outcome of spontaneous human action, not of human deliberation or human design (Hayek 1989).

There are, however, areas where public policy does shape the actual mold of the otherwise spontaneous order. One of these fields is inflation, especially when structural factors, liberalization, productivity gains, and overall uncertainty, coupled with low credibility of the government and the newly created financial institutions, all point toward sustaining inflation. Meanwhile economic theory does provide a host of arguments proving the vices of inflation and the virtues of price stability.

Transformation experience has added two important empirical findings to our common knowledge. First, the evidence for the twenty-seven countries in transition shows that there has not been a single case in which softer policies would have resulted in a smaller drop in output or a smaller decline in living standards over the longer run (Gelb and de Melo 1997; Fischer et al. 1997). Furthermore, no country could enter a path of sustained growth without having brought down inflation to at least moderate levels. Conversely, all countries that could sustain disinflation did enter a growth path. The later disinflation occurred, the longer the transformational recession lasted. Premature reflation or growth based on the expansion of domestic demand, by contrast, invariably triggered renewed austerity, from the Czech Republic to Romania, in each and every case (Csaba 2001).

There are two interesting related findings. First, the expansion of growth to, say, East Asian levels has nowhere materialized. The reasons for this are a subject of controversy. Some argue that inadequate savings and overextended welfare spending do not allow for a real takeoff and a growth rate of GDP of above 4–7 percent (Gomulka 1998: 36–40). Other, more theoretical writings indicate deeper causes and note, first, that steady-state growth cannot be meaningfully raised by governmental policies and, second, that the growth-promoting effects of disinflation are limited (Barro 1997).

From this perspective, countries such as Poland, the Czech Republic, and Hungary can no longer count on the effects of disinflation. Instead, new and more lasting sources of endogenous growth need to be created, like those related to human capital formation (an area rather neglected in the previous decade). The latter would be important not only for growth, but for more general welfare reasons as well. As A. Young (1998) has demonstrated in a formal model, research and development outlays may not necessarily translate into higher output, yet, can be welfare enhancing by improving the well-being of the average consumer.

A related issue is why inflation has not come down from the moderate levels, as had been expected by most of those working with formal models. While bringing inflation down from very high levels has been expansionary rather than contractionary (Easterly 1996), the evidence on the effects of reducing the inflation rate from moderate to single-digit levels has previously been mixed. While an authoritative compendium of papers, published by the IMF and the National Bank of Hungary, presents theoretical and empirical evidence supportive of further disinflation, contributions to the same volume indicate in detail how intricate the credibility and coordination problems that need to be overcome are (Cottarelli and Szapáry 1998).

Here we have arrived at an important point, where quantitative empirical evidence has helped to overcome previously hotly debated theoretical issues in development and transition economics. While historically and institutionally oriented pieces offer analytical insights into how certain processes come about, quantitative tests help to arrive at generalizable conclusions that integrate bits and pieces stemming from different episodes. One of the more reassuring elements of the ongoing controversy is that, despite all methodological differences and disputes, formalized methods have delivered comparable cross-country evidence that is useful to settle at least some of those disputes.

Another area of major controversy was liberalization. It has several dimensions, as prices, wages, trade and financial markets, and later asset markets have to be freed from various entry and price-setting barriers. Whether this has to be done remains a subject of political debate, but it is no longer a matter of economic controversy. What remain highly controversial though are the speed, the scope, and not least the sequencing of liberalizing steps. Those dissatisfied with the current state of affairs either consider the price of liberalization as too high or regard the sequencing as mistaken. However, this view does not take the lower level of development and the legacy of decades of isolation into account. The ensuing distortions, rigidities, and behavioral deformations have increased the costs of adjustment in the postcommunist countries.

In retrospect, it is also no longer legitimate to question whether export-oriented growth is feasible. Exports of central European countries have expanded at a staggering pace in the 1990s. This was achieved through a reorientation of trade from soft CMEA markets to OECD markets, so that the EU now accounts for a larger share of trade in Hungary, Poland, and the Czech Republic than in Portugal or Spain. Opening up the economies helped orient structural change in that it contributed to the supply response and the overcoming of bottlenecks and shortages. Quite in line with Latin American experience (surveyed in Nogues and Gulati 1994), sustainability of trade liberalization proved to be the key. While more gradual opening strategies reverted to protectionism, sustained liberalization helped unlock other factors of economic growth, provided it was part of comprehensive reforms and could thus induce mutually reinforcing adjustment processes.

These findings are supported by the experience of the transition countries: those that could sustain the large coherent macropolicies across the decade, such as Poland and the Czech Republic or Hungary, have benefited from the same improvements. Meanwhile, those countries where policies proved unsustainable (e.g., Romania, Bulgaria, Albania, and Russia) or where such policies have never been launched (e.g., Ukraine or Serbia) are lagging behind. This is of course the macro view, which is of little comfort to those who have been crowded out of business or laid off. From the point of view of theory, however, it is the macro level that really matters (Solow 2000: 151).

Finally, privatization, the third pillar of economic reform in transition countries, needs to be addressed. In the literature referred to in the introductory section we recurringly find the view that the problem of ownership change has been overemphasized vis-à-vis other issues. In retrospect, it is hard to share this view. The best proof is indirect: none of the countries that has experimented with various "third way" solutions or has opted for a higher rather than lower share of public ownership has achieved higher growth rates, higher capital accumulation (both human and physical capital included), or lower unemployment. The comparisons that underpin this interpretation are rather trivial: the Baltic states compared to Belarus, Poland compared to Ukraine, Hungary compared to Bulgaria send unambiguous messages. Thus the burden of proof must be reversed. Those advocating a slower pace of privatization and more state guidance have to come up with more convincing arguments.[1]

These observations do not justify the ideological posturing of the early transition period. Nor should ownership change be mistaken for progress in economic well-being, especially in the short run. I still adhere to my earlier position that there is a natural rate of privatization[2] that is hard to override through direct government intervention, since the process is an outcome of the interplay of a series of factors and agents, domestically and internationally (Csaba 1995). For example, those who wanted to conclude major deals at the low ebb of the emerging market crisis were disappointed or simply stripped of a lot of assets. But the proof that a relatively fast emergence of a system based on private property is feasible and represents a guarantee against a backslide to statist totalitarian practices has indeed been delivered. Comparing Russia with Belarus or Slovenia with Serbia-Montenegro clearly shows that even those countries where structural reforms were delayed have achieved demonstrable gains in systemic terms compared to those that are still tinkering with improving the socialist heritage, manipulating old institutions via traditional techniques.

Privatization in these terms ceases to be a primarily quantitative issue, a plan-target to be attained at a fixed point in time. Rather, and quite like in market economies, it develops into a qualitative issue, namely, of delineating the proper spheres of business and politics, business and state administration. From this perspective it is not the number of commodity exchanges or the number of private shareholders that defines the path of progress. Also, the share of nonstate property in total national assets ceases to be a relevant criterion of measurement. What really matters is whether we see Korean or Italian type of intertwinings, whether we see politically unbiased allocative decisions, whether we witness the emergence of a capital market and banking arrangements that put up institutional barriers so that politically leveraged decisions will not occur in the future. From this perspective, it is the scope rather than the size of the capital market that really matters. It is the presence of strategic investors in the financial sector and the extent of foreign ownership that are useful indicators. With a decreasing share of industry in GDP—27.5 percent in 1999 in Hungary—generalizations based on the progress of ownership change in the industrial sector or even only in manufacturing seem to be of historical value at best. Meanwhile, progress or regress in the financial sector, transparent or nontransparent ways of conducting business, availability or lack of prudential arrangements, and disclosure requirements according to international standards are the real criteria of evaluation, rather than any arbitrary set of quantitative plan-targets.

From this at least two conclusions can be inferred. First, the type of market economy that emerges in individual transforming countries depends on how the issues discussed above have been resolved. Second, privatization is going to have efficiency-enhancing effects on two levels. One is the immediate one—static efficiency. Overcoming flawed incentives can produce miracles in terms of profitability and productivity by resulting in a better use of available assets. In dynamic terms, however, investment has to be factored in. Depending on the way new investment is financed, the potential for catching up, inherent in the Solow model (1956), is going to materialize. It is important to underline that the neoclassical catch-up model presupposes the availability of precisely those institutions and incentives for appropriate resource allocation that are just emerging in the transition economies. Thus their progress in financial sector reform will likely determine the efficiency gains from privatization in the short and medium run, as well as the growth-generating effects of ownership change in the longer run.

Lessons from the East Asian and Russian crises and contagion of 1997–99 have underscored the importance of institution building as the core of sound and sustainable development. As a most insightful summary paper of Stanley Fischer (1999) explains in detail, the crises have not proved the overall line of financial liberalization wrong, but they have definitely called attention to the eminent need for better regulation and higher transparency. What deserves attention, however, is that none of the fundamental reform suggestions go in the direction of capital controls or undoing liberalization in the financial sector. Instead, they all aim at fairer burden sharing by inducing more transparency and by "bailing the private sector in." Our more general considerations of the need to incorporate institutional and qualitative elements in devising a policy framework are directly reflected in proposals to update and tighten the Basle core principles on capital adequacy (Caprio and Honohan 1999). One of the most intriguing lessons of the Asian contagion has been the risks associated with mechanical quantitative risk assessment models. In particular, cross-ownership may render capital adequacy numbers irrelevant. The wide-ranging proposals all point toward enhancing personal responsibilities of bank managers, increasing risk awareness, raising reserve requirements, and introducing old-fashioned solidity requirements and disclosure practices. Such precautionary measures are a more appropriate reaction to financial market volatility than rationing or state discretion. These findings apply to transition as well as to developing countries, that is, to both groups of emerging economies.

Thus, a strong state rather than a minimal state, a competent state administration rather than a cheap state administration, is required for a successful transformation. A strong state is not necessarily a strongly interventionist one since its indicator of success is output (effects brought about by its intervention) rather than input (the number and variety of measures instituted to ward off responsibility for outcomes). Moreover, those who can be confident in the efficacy of their actions may not feel the same need to intervene.

On the Political Economy of Second-Generation Tasks

What has been said in the two previous sections leads to issues of political economy. The available evidence clearly shows that no country can avoid the tasks and pains of economic reforms, provided wealth is to increase and development to be sustained. The ongoing differentiation of the group of transforming economies also implies a differentiation of research subjects. For those studying the laggards, a great deal of policy analysis remains to be done. They have to address the question of how to graduate from the present stage to a more mature one. Although this is interesting per se, it no longer raises open theoretical issues. For those studying the more advanced or front-runner cases, a new agenda emerges. The countries that have successfully mastered the first-generation tasks now have to create financially sustainable welfare systems, find new models for rural development in densely populated regions, and address the problem of environmental decay. What is at stake is the redefinition of the role of government in an increasingly complex and globalized world.

This implies that what John Williamson (1994: 26–28) has called the Washington consensus has indeed remained a shared vision of good policies for advanced and less advanced countries alike. This consensus has informed the deliberations leading to European Monetary Union (Lányi 1997) and has found its way into development economics. As J. Waelbroeck (1998: 341f.) rightly stresses, the original term is misleading insofar as it suggests an almighty global government located at the IMF headquarters, which is imposing its views on a large number of undisciplined agents. If nothing else, the failure of the IMF to manage the contagion effect of the East Asian crisis on emerging markets in 1997–98 has overwhelmingly demonstrated the lack of such a supranational coordination mechanism, operated consciously by a group of technocrats.[3] On the contrary, what we witness here is voluntary compliance of a growing number of countries with a set of shared principles. These countries are supporting democratic values not only for efficiency considerations, but also for their own sakes. Therefore, it seems legitimate to follow Waelbroeck's suggestion and speak of "one world consensus." The underlying convergence of rules and values has been related to global interdependence, the information revolution, and the globalization of financial markets, as well as the resultant flow of ideas. These shape the norms and expectations of a growing number of opinion leaders and citizens alike.

The commitment to the Washington consensus has two implications. First, at the level of theory, there is no longer the confidence instilled by Keynes that macroeconomic equilibrium can be restored by simple devices. Economists now understand that rational expectations make economies difficult to control and that phenomena such as lack of credibility can deprive policies of their effectiveness.

Second, policy advice should not be directed toward a group that is but one among many shaping outcomes. Many other actors—from households to foreign investment funds—need to be convinced of the virtues of reforms. In line with the public choice school, policy-makers should be seen as self-interested actors maximizing their own gains rather than as selfless heroes. Thus, the target audience is the educated public at large, and the job is to improve governance, i.e., to elaborate procedures and institutions that incorporate proper theoretical findings into the interaction of state, private agencies, and groups of individuals.

This state of affairs has further implications. There is a growing convergence and similarity of the agenda of policy reform in the various country groups under discussion. However, this does not make the lives of analysts and policy advisers easier. In the case of second-generation reforms, there is no "proven way." The goals to be attained need to be discussed and elaborated. Normative concepts, such as those proposed by János Kornai (1997), can help to specify the agenda. Yet, there is no such thing as a single optimal outcome set by technical desiderata or considerations. Owing to the differing nature of tasks, the lengthy democratic decision-making process cannot be avoided. This is a major difference from the early transformation period. In the case of pension reform, for instance, the speed at which legislation is adopted says nothing about the sustainability or the quality of the arrangement; nor does the size of the fully funded pillar serve as a point of orientation or a success indicator. Fully funded systems can crash and so can pay-as-you-go systems.

As a consequence, issues such as political feasibility or coalition building are gaining relevance vis-à-vis traditional considerations of coherence or analytical simplicity. Dynamic modeling may be one of the more important analytical tools in forecasting what kind of compromise is self-defeating and what might be self-sustaining. Quantitative modeling does play a crucial role in specifying financial preconditions as well as conditions under which these can be met. The longer the time horizon of a reform the greater is the relevance of these approaches and the need for integrating them into the decision-making process.

One of the fundamental questions in redefining the role of the state in a democratic society is whether it is going to be captive to or regulative of vested interests (Rodrik 1996: 22–24). The thinner the dividing line between interest representation of various sorts and public organs, as well as between business and administration, the lower is the probability of arriving at Pareto-optimal outcomes. Corruption and rule evasion, often seen as a smaller evil or even as a lubricant in backward societies, may become serious impediments to change for the better. This might prove a particular challenge to postcommunist societies, where the traditionally extended underground economy and a decade of most ineffective legislation have made norm-breaching behavior more accepted than in most advanced countries.

More common causes of government failure in the second-generation tasks include: (1) inability to make commitments; (2) coalition formation and bargaining that dilute the agenda; (3) destructive competition, inherent in party politics, that thwarts the implementation of generally approved measures; (4) uncertainty about the consequences of change; (5) the simplicity constraint (the best professional arguments do not always sell well); and (6) secrecy of bureaucratic decision making, leaving the door open for special interest legislation (Stiglitz 1998b: 8–16).

There is no trivial textbook solution to these challenges. Misalignment of incentives is a nearly inevitable by-product of the type of democratic decision-making processes that were shown above to be inevitable in an attempt to find the best and most feasible solutions for reconstructing entire macrosystems. Making use of sociology and organizational science in devising the appropriate task force might be a tremendous challenge. In Hungary, for instance, public finance reform tended to founder more than once on the composition of the task force, drawn basically from among vested interests. The failure to combine technical economic imperatives with the political requirements necessary to secure public acceptance of reform policies at a later stage of implementation often resulted in stalemate. Only crises and coups, deplored by many analysts, could move things out of such a state of low-level equilibrium.[4] At the same time, improvised solutions resulting from these exigencies have rarely been sustainable and have delivered only part of what is technically achievable at any given point in time.

Another problem of consensual decision making has to do with the tendency to dilute concepts through excessive use. The more universally accepted a concept is, the higher the likelihood of it being broadened to the point where it loses its meaning. This has happened to such core concepts as the social market economy. As the excellent survey by D. Cassel and S. Rauhut (1998: 17–24) has demonstrated, political compromises and unintended consequences in the pursuit of a social market economy in Germany, in particular the extensive redistributive practices, reflect not only unfavorable real-world conditions but also the limits of the very concept of a social market economy. Therefore, it seems that this concept should be used only for denoting an approach, an emphasis on *Ordnungspolitik* and the idea of a competitive order trying to strike a balance with social justice, rather than for deriving concrete policy prescriptions.

And here we have come to an important conclusion. As the example would be easy to replicate for a series of widely shared concepts, from subsidiarity to optimal regulatory areas, there is no quick fix. Subscribing to the interpretation of what economic theory has to say that is offered in this chapter does not impose a straitjacket on further research. While providing a framework for conducting productive discussions, it is unlikely to settle disputes over methodology or policy proposals. Quite in line with the original Hayekian idea, what is likely to emerge is competition among systems, not in the sense of capitalism versus communism, but in the sense of varieties of capitalism (Dimitrov et al. 1999; Bara and Szabó 2000). This competition is already very real. In the West, the Swedish and the American model exist. In China, Russia, or Africa, unintended but powerful processes of decentralization have increased the number of different trajectories. The more we focus on the study of real-world systems and are thus forced to take into account empirical evidence, the more inclined we might be to follow Hodgson's call to redefine economics as the study of systems (Hodgson 1998: 190). In so doing we will not be forced to give up the traditions and methods of the discipline. Rather, these traditions and methods can continue to serve as an analytical framework for incorporating perspectives and worldviews provided by other branches of social science. Following Marshall, we would see economics not as a body of concrete truths but as an engine of discovery. For this point of view the mastery of science involves the acquisition of analytical skills needed to formulate relevant questions to which answers can be sought. This implies that with cross-cultural transmissions economic ideas also mutate, as W. Barber (1995: 1947) correctly observes. The point in sticking to the rituals of each discipline is, among others, retaining consistency of the viewpoint as well as the ability to communicate cross-cultural realities. It is a common language that makes specific and individual experiences transmittable to others.

Respecting the limits of a paradigm is also a way of becoming more open to competing approaches. In any event, it will always be the task of the policy advisor and policymaker to decide on the most suitable policy mix to achieve specific goals under given circumstances. No amount of compromise on the part of scientists with competing perspectives can deliver such a policy solution. On the contrary, it might be a good idea if policymakers and laymen could be sure what kind of questions economists, as opposed to sociologists or historians, are more likely to answer in a satisfactory manner, and which they cannot even attempt to answer. Thus, issues pertaining to wealth creation and maximization may be more properly addressed to this guild than are moral issues, legitimating principles, justice, and many other facts of life that are taken for granted by economists. One of the more obvious ways of appreciating other approaches and disciplines is to expect them to deliver answers to questions on which economists claim ignorance, though their presence is already implicit in economic assumptions or propositions.[5]

This is not a call to return to the intuitive economics of the nineteenth century. On the contrary, as the insightful analysis of W. J. Baumol (2000) shows, the contribution of twentieth-century economics was precisely that enhanced rigor, more abundant data, and more sophisticated methods allowed for more powerful empirical studies. Similarly Solow (2000: 151) underlines that macroeconomics will always remain the heart of economic theory "for the best possible reason: the need to understand current events, especially unfortunate ones, and formulate policies—even benign neglect is a policy—to deal with them. For the same reason, the characteristically close connection between macrotheory and empirical work is unlikely to change."

Notes

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1. The Chinese experience, which has not followed the overall trends described above, is often invoked as an objection to the mainstream theory of transition. For my view on this contentious issue, see Csaba 1996, where I argue that the two cases cannot be compared for various theoretical and methodological reasons.
2. Just like NAIRU, the natural rate of privatization is neither constant nor exogenously determined, but follows from the changing pattern within the system. See Phelps (2000) on NAIRU (nonaccelerating inflation rate of unemployment).
3. This is likely to become even more explicit, if only part of the propositions of the Meltzer Commission for reforming the IMF are adopted (Meltzer and Sachs 2000).
4. For instance, the Polish wrestling over stabilization in 1987–89 left no way out but the shock stabilization by Balcerowicz. Another case in point might be the March 1995 Bokros Package. It was elaborated secretly and surprised even some cabinet members.
5. The economic approach has challenged a variety of disciplines by pushing its adversaries to take stock of what their paradigms have to deliver (Lazear 2000). Likewise, though probably in a less explicit fashion, economists have drawn on new findings from biology, psychology, and mathematics. Our knowledge about actors is greatly influenced by these findings.

Chapter 3

The Crisis of Transition as a State Crisis

Kazimierz Z. Poznanski

At the center of this analysis of postcommunist transition are two of its most striking aspects—state failure and foreign ownership. The analysis begins with an attempt to explain the regionwide recession not as an inevitable legacy of communism but as a state-made production collapse. It is not state errors of judgment but the interests of its movers—reformers and others—that are behind this negative turn. Next, the analysis turns to the question of ownership reform to show that the same private interests are responsible for the state's creation of a dysfunctional capitalism where capital is mostly foreign and labor is domestic. Both these problematic outcomes are presented as sufficient to consider the whole postcommunist transition in the region a failure. Given the critical role of the state in derailing reforms, the failure of transition has to be viewed as a state failure.

One should look at the transition from communism to capitalism not from the perspective of its end position, but from its beginnings. This is how evolutionary thinkers from the so-called Viennese school, such as F. A. Hayek (1988), K. Popper (1943), or J. A. Schumpeter (1992), advise us to examine social reality. All things have their beginnings, and what happens thereafter and what transpires at the end are somehow determined by these starting points (Poznanski 1993). For this very reason, if one is unable to explain the genesis of something, one is also unable to understand the phenomenon at stake. The common, if not routine, way of examining the transition today, however, is to look the other way around, from the end point backward. This is the case in large part because the transition is taken mostly as an objective and not as a process. So understood, the

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