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Transformation as a Subject of Economic Theory

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Introduction

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 as such, and on the ability of the theorists to deliver anything meaningful. This applies *a fortiori* for the economics profession, where a peculiar version of the impossibility theorem is about to emerge, postulating theoretical economics' inherent inability to say anything meaningful. And not only the uniformed public is ready to adopt such a defeatist stance. Having presented an earlier version of this paper to a research seminar at a large European university, the dean of the economics faculty commented: in the end, there is not much to be discussed, since the two topics are unrelated. At an other occassion yet another distinguished professor noted: economists nowadays are like philosophers, they do not care much about reality. They prefer their self-contained theorical constructs, and the art is in discussing minor modifications within the high guild.

But also among the area specialists there is a feeling on discontent. On the one hand we find self-justifying accounts over the accomplishments of traditional Sovietology (*Shroeder*, 1995, esp. pp. 226-7) while simultaneously conceding the field's inability to predict the collapse of the Soviet system (let alone its timely sequence). This is certainly an analytical flaw and reflects the tradition of highly esteeming the collection of facts and figures, and playing down the importance of the analytical and theoretical frame in which these are integrated. The scientific issue clearly is not how high or low was the Soviet GDP, or the share of defence spending/the main subjects of the piece quoted above/, but what we should think of the reasons for the collapse, as well as of many signs of decay, or of the nature of the crisis that has triggered the change in the régime.

Understandably there has been much irritation from the very outset over the „intrusion" of outsiders to this area, who have been critisised for a large number of fallacies, most prominently for having imposed their academic culture and inherited preoccupations over the political consultancy and decisionmaking process (*Murrell*, 1995, p. 169). This feeling has evolved into bulky volumes and vitriolic pieces, like the book of *Lavigne* (1995, pp. 248-9), speaking about the all-embracing neoliberal party-line, or of *van Brabant* (1998, p. 462 and 477-9) blaming neoliberalism for being irrelevant on growth and modernisation issues, or *Przeworski* (1998, pp. 412-5 and pp. 427-8) cautioning against a naive minimalist approach to state interventionism, and the list could be extended at will by contributions from authors from the region itself.

The task of the present paper is to try overcome this obviously improductive counterposition, where rehashing one's own stance or preferences often dominates the interchange of ideas. My basic thesis is that economics, when interpreted in sufficiently broad terms, can meaningfully contribute to interpreting facts and figures, provide an analytical frame for arranging them and even suggest some lines of action for policymaking. Meanwhile, doing so economics - like any other discipline - can not aspire for exclusiveness. The subject of analysis is a multidimensional, and in many respects open-ended, socio-economic process where several approaches are justified. However, methodological considerations, as well as practical experience caution against too much eclecticism, where the concurring paradigms of various social sciences are mixed up. As *Prybyla* (1998, p. 359) aptly put it, „one should be sceptical about one-size-fits-all constructs but also shun deconstructivist academic wooliness 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 visions may well never coincide, this might well not reflect more than various viewspoints, as of a same nude, painted from the back or from the side, or face-to-face. Thus in what follows I shall adopt a consciously one-sided, narrow economic approach and try to figure out what this area of research has to contribute to understanding systemic change, and vice versa, what new impetus can be derived from the experience of this area for general economics.

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 somebody having contributed to the area. Both in general and even more *in concretu* it can be a hard nuck to crack. Just to take the best known examples: *Adam Smith*, the founding father of the discipline was the professor of ethics and was considered to be mostly as a philosopher, so was *Friedrich August von Hayek*. *Gunnar Myrdal* has been seen as a general social scientist, *James Buchanan* as a political scientist, *Myron Scholes* and *John Harsányi* as mathematicians, *Arthur Lewis* as a „developmentalist" and to mention the most recent, *Amartya Sen* as a moralist and nutritian, while *Fogel* - chairman of AEA - as a statistician, and *North* as economic historian. Given that the Nobel in economics has a history barely over 30 years, the typical wiev of a median theoretical department of economics, paraphrased above, is certainly representative of a tendency trying to narrow down the field in a way, which is not supported by any commonly accepted standard, except fashion, focusing on technicalities above all. But in this narrow sense even *Milton Friedman,* with his recurring refusal to quantify many of his statements, many qualify as a heretic.

It would seem easy to disqualify the above approach, had it not been the entire postwar history of economic thought that the fight for formalisation 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 Palgrawe (*Eatwell* - *Millgate* - *Newman*, eds., 1987), where the time of publication already precludes any reflection on systemic change. But it is not just a matter of time that a second edition is unlikely to contain a heading on transformation. It is the *genre* which differs a lot.

Rewieving the *opus magnum* the *Economic Journal* notes on introduction: „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 ”(*Bailey*, 1994, p. 661)... 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 unduely technical” (ibidem, pp. 665-6). But later on the same reviewer finds - approvingly - 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” (ibidem, p. 667).

Thus, on the one hand, we find a community of scientists defining everything in their own terms, unlike other disciplines, not by their subject of study. Moreover we do find a dictionary which defies the term by containing articles whose technicality is at the level of - narrow circulation - academic journals, exposing contrarian positions, and is not even meant to make the life of its user easier, or help in practical application of any sort.

Skipping the inconclusive, though always emotional and amusing, academic debate on methodology and the proper ways, we may well assume that economics is what is written by those who identify themselves so, and/or are active in shaping economic activity at various levels. This leads us to a positivist, rather than the currently predominant normative, view of affairs. Leaving political and business applications aside, economics in thus what is produced as such by the leading journals, naming themselves under this heading, as well as by the publishing houses of the like. „Leading” may thus be defined by impact factors, citation indices or publication leading to a sustained echo and promotion in the academic world. These are currently - quite like in natural sciences and partly for linguistic reasons - the leading Anglo-Saxon journals, where the position is, or may be, defined by refereeing fees, rejection rates or the frequency of presence of Nobel Prize winners, or other school-molding personalities of the profession.

The real surprise comes when one is inclined to look into more than one of these journals more than once in five years. Against the background of the self-interpretation of what theory is and should be, it is truly surprising to see 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 oversee the large number of applications, verbal pieces and the presence of several schools of thought. Besides the *Journal of Economic Literature*, which established a long tradition of bringing papers, giving an overview of an entire subject matter for outsiders in a fairly non-technical fashion, (and earned a very high reputation for this), the AEA established *The Journal of Economic Perspectives* with the expressed aim to build a bridge between academics and practioners, to foster mutually beneficial interchanges. Anybody following the papers and proceedings published in the *American Economic Review* would be at ease to cite any amount of contributions on „real world problems", from economic growth to social security reform, economic history and country studies, financial market problems and health care economics. In sum, unless the „median departmental view" is accepted, the omnipresent and almighty mainstream seems to be a strawman.

Besides the differences in subjects, differences in approaches must be taken account of. 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 perception of the mainstream. While *old institutionalism* is basically hostile to formalisation, as well as to the idea of methodological individualism, *new institutionalism* attempts to integrate the analytical techniques, developed by the mainstream, to address organisational and institutional issues, non-existent by definition at the level of abstraction, customary at the level of exposition of the mainstream.

Contrary to the strong words normally heard at introductory courses (to induce students to master the mathematics needed to operate quantitative techniques), these two, more traditional, more verbally oriented schools are strongly present in the first rate economic journals and publishing houses. This is more surprising of the old institutionalist school, but the finding is amply documented in the exhaustive reviews of *Samuels* (1995) and *Hodgeson* (1998).

Institutionalism represents the rennaissance of *heterodox approaches* in economics. The paradigm of this approach is the priority of organisational structure in allocating resources, and the focal 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, p. 572). While formalisation inevitably focuses the attention of analysts on discrete states and conditions defining these, the old institutionalists see the economy as fundamentally processural (and in this sense there is a bordering with Marxism as well). Also fundamental is the interest in legal structures and in their nexus to real economic processes.

Because its broader, evolutionary approach old institutionalism tends to downplay quantitative evidence and the entire focus on equilibrium solutions, dominating formalised thinking. While considering equilibrium situation as a highly restricted case of the process considered by them, adherents to this school normally highlight the insurmountable difficulties in specifying comprehensive real world situations into quantitative models, and think that simplifying assumptions, made for the sake of computability may be crippling (*Hodgeson*, 1998, p. 187). Adherents to this school highlight: the quest for quantitative evidence is somewhat backward-looking, as it still were the last century with mechanics seen as the great science, or pre-transition US, when the military industry defined the best R+D options. By contrast, both money and attention has been refocused to biology, which is a good news for the evolutionary approaches.

New institutionalists, as *Coase* (1998, p. 73) recently explained, are sharing the old ones' unease about the overtechnicisation of mainstream. However, they do accept the rational actor model and maximising behaviour, but are interested also in the very imperfect macro-coordination mechanisms of various sorts.

Therefore this line also took great interest in the property rights, as well as in the procedures of how macrodecisions are being taken. While old institutionalism is holistic and anti-formalist, new institutionalists accept the relevance of formalisation. This, however, is not the core point for them. They treat historical and structural explanations rather as complementary, not contrarian, to mainstream economics. As two authoritative summaries - actually monographs - on these attempts (*Rutherford*, 1994, *Pejovich*, 1995) have highlighted, the marriage is quite fruitful. The cost of heterodoxy is a lower level of abstraction and less generally valid conclusions, which is alas the benefit as well, if one moves towards applied areas.

This line leads to several complementary areas. One is *evolutionary economics*, which some of the sources treated above as a branch of institutionalism. While the borderline is anything but clear, evolutionary approaches are much less concerned with institutions, setting the framework of allocation and the decisions of market actors, as with the process of *how* market *selection* of agents and rules take place. As the relatively recent but most authoritative survey of *Nelson* (1995) demonstrated the - biology-inspired - natural selection idea, the focus on the self-generating processes, as well as on forces promoting and hindering the *ability to adjust* to the basically hostile external environment, are the major concerns of this approach. In stressing the role of market for managers, as well as by the heavy emphasis on the natural selection process through trial and error against man-made optimal designs, this trend has a lot of commonality with the elder Austrian school.

Quite logically this approach extends to questioning the way decisions are taken and priorities set. Both processes tend to fall outside the scope of attention of formalised analyses, which tend to take these as externally given. However, *transaction costs approach*, could be successfully integrated in a framework of policy analysis by *Dixit* (1996) focusing on the planning and implementing policies in a multiple principal - agent framework. Here the multiplicity of principals is a source of inherently blurred system of incentives.

One step further economics can be, and indeed, is concerned with the *ways preferences are being formed*. In the social cost analysis of *Sen* (1995) rationality does not require the postulate of *homo oeconomicus*, but can follow multiple rationalities as well, like prudence or even generosity. This feature - equally highlighted in the monograph of *Bruno Frey* (1997) - helps a better understanding of the way preferences are being formed. The social cost approach, as Sen himself notes, may be particularly helpful in modelling and forming well-established decisions superseding microeconomic rationality, like those pertaining to the environment. The benefit of these approaches is, of course, in broadening our understanding of the *modus operandi*, whereas the cost is the loss of computability and of quantifyable evidence. As both approaches move abstract theories closer to down-to-earth issues, analysts may well want to choose, depending on the objective of their excercise, whether quantified testable evidence or more analytical insight is better suited to their purposes.

This far from exhaustive review of some of the more interesting trends in theoretical economics may well suffice for the present purpose. It is to highlight: it is not the international agencies which are the fountain of knowledge in economic theory. Furthermore it is not the oversimplified world of an introductory macroeconomics textbook which represent the state of art currently available in the profession. Thus the sometimes ritual references in the transition literature on the neoliberal strait-jacket, on the imperialism of formalised procedures, on the irrelevance of the one-dimensional economic man, searching for static equilibria and optimi, are fighting the strawman, indeed. It is a reflection of underinformation at best, and manipulation at worst. It is fairly evident, that there has been a rich menu of various approaches, formalised and less formalised, available to any policymaker or theorist. The international agencies, quite in line with their competences, normally fostered steps that improve the current account, which was only right. Whether the same steps serve a longer term interest of a development strategy, or whether they did reflect the best available professional knowledge at any point of time, has been up to the local élites to decide. And quite as in the third world, bad or simply unpopular policies are best sold as external dictates or, conversely, under IMF-bashing. But this would take us to an even more applied level, that earned the name political economy, and devoted to the *sustainability conditions* of normally unpopular sound economic policies, needed to bring about sustainingly improved well-being and wealth maximisation. Before addressing the related issues it seems expedient to revert the logic of our reasoning, and sum up what, if any, lessons of transformation policy can be inferred at the level of applied economic theory.

Transformation: theory and evidence

The above sketched patchy summary of what is economic theory about, leaving aside the more technical aspects, has indicated an *agenda* quite *different* from the one cultivated in and on the transforming countries themselves. Many of the topics, having dominated the debates over the last decade or so, seem to have been mispercieved, lacking relevance, or having been purely ideological in nature, bearing little or no relationship to the broadly defined subject of economics, i.e. of *improving well-being by managing scarce resources* over activities and over time. The controversy over shock therapy versus gradualism, the idea that any country can opt for a path-independent (freely chosen) pattern of governance, the quest for optimality in privatisation technologies, the benefits of protectionist policies and many other subjects look rather queer today.

With the benefit of hindsight the controversy over the costs of stabilisation, the search for softer ways, or the debate over the uses of delaying privatisation and replacing it through state industrial policies, seem all to have missed the point, just as much as suggestions of exaggerated costs of transition in terms of lost output or welfare. I have not come across in the literature any modelling, quantitative or verbal, which would have positively offered a credibly concievable alternative path or optimal development pattern, against which actual costs in any country or time could be measured. True there is no shortage of claims and pronouncements, locally and internationally alike, qualifying transition costs as excessive. But this claim is a reflection of subjective value judgements, i.e. a political rather than a professional statement *par excellence,* should the speaker be unable to specify what costs had he been willing to accept *at the margin*, and how does he establishes his case. References to absolute magnitudes are obviously empty, while references to other countries beg the question of comparability. For instance the idea of deliberate industrial policy or thickly knit social safety belts presuppose a strong state that has simply been nonexistent, as the crisis of the Soviet empire has been at the very starting point of the entire process. Had there been a strong Soviet state, most of the former socialist camp would willy-nilly continue to experiment tinkering with various sorts of market socialism, instead of trying - and probably often erring - in various modes of transformation.

If transformation is the child of a crisis, traditional Sovietological plan-actual comparisons of various points and plans may hardly be the best measures to evaluate governmental success, let alone the progress of the entire process. All the less so, since much of the most relevant aspects emerged spontaneously, quite outside the framework of formal institutions and legislation. This is most striking in the case of the spread of the private sector. In this area, stagnation of the grand - and most contested - privatisation projects could neatly coincide with a blossoming of new startups, as well as with a creeping process of spontaneous privatisation. The speed of actual - not purely formal, bookkeeping - change in ownership patterns and restructuring did not prove to be among the independent, freely cariblated variables, whose size were just a matter of deliberation. Likewise the rate of growth - one of the important macroindicators - could not be set at will, as in modelling. 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 it is so, the mere idea of a growth generating policy, or the suggestion that would assign such a task to the governments, seems to be misplaced. These do not, even in theory, posess the *means* whereby they could be efficient, and thus be hold *accountable* for mastering the task, which is not only theirs. While many of those trained in observing the rituals of the planned economy would find the predominant spontaneity as a sign of crisis, or even chaotic, adherents to the evolutionary or Austrian schools would consider it as perfectly normal, as since the market economy is the only order which is the outcome of human action, not however, of human deliberation (*Hayek*).

There are, however, areas where public policy does shape the actual mold of the otherwise spontaneous order. One of these is *inflation*, especially in our case, when structural factors, liberalisation (to the extent of corrective element), productivity gains and overall uncertainty, coupled with low credibility of the government and the newly created financial institutions all point towards sustaining inflation. Meanwhile economic theory does extend a host of arguments proving the vices of price rises and the virtues of price stability.

Transformation stories have added two important empirical findings to the overall or common knowledge. First, the evidence of 27 countries, measured through various regressions (*Gelb - de-Melo*, 1997, *Fischer - Sahay - Végh*, 1997) have proven: there has not been a single case when softer policies would have resulted in *less* drop of output or *less* decline in living standards over the longer run. Furthermore *no* country could *enter* the path of sustaining growth without having brought down inflation, at least to moderate levels, whereas all countries that could sustainingly disinflate *did* enter the growth path. The later disinflation occured, the longer the transformational recession lasted.

There are two interesting related findings. First, the expansion of growth to, say, east Asian levels has nowhere materialised. Various models projecting very high growth trajectories for the long run, way ahead of the pre-crisis levels, never materialised. The reason for this is a subject of controversy.

Some attribute this to inadequate savings and overextended welfare spending, which do not allow for a real takeoff above the 4-7 per cent level at the very maximum (*Gomulka*, 1998,pp. 27-28). Other, more theoretical writings (*Barro*, 1997) would indicate *deeper causes*, noting that a/ steady-state growth can not be meaningfully raised by governmental policies, and b/ the growth promoting effects of disinflation are limited, to about every 10 per cent disinflation accounting for an additional 0.3 per cent of growth. In this perspective countries like Poland, the Czech Republic and Hungary, having come down to the lower edge of moderate inflation, have not got this source at their disposal any longer. New, 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 more general welfare reasons as well. As *Young* (1998) demonstrated in a formal model, R+D outlays may well not translate into more output, still can be welfare-enhancing by improving the well-being of the average consumer even in the lack of delivering more.

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 could be shown to be expansionary, rather than as conventionally feared, contractionary (*Easterly*, 1996), the evidence on disinflation from moderate to single digit levels has previously been mixed. A recent compendium of papers, published by the IMF and the National Bank of Hungary presents theoretical and empirical evidence supportive of the uses of further disinflation. However, the same volume explains in detail how intricate it is technically, both for credibility and coordination reasons, to proceed in practice on this commendable way (*Cottarelli* and *Szapáry*, eds, 1998).

Here we have arrived to an important point, where quantitative empirical evidence helped overcome previously hotly debated theoretical issues, both in development and transition economics. While historically and institutionally oriented pieces help offer an analytical insight into the *mechanics* of how certain processes come about, formalised quantitative tests helped to come to generalisable conclusions, *integrating bits and pieces*, stemming from obviously varied country experience, around a general line or trend. One of the more reassuring elements of the ongoing controversy is that methodological differences and disputes notwithstanding - as to the specification of the individual models or to the explanatory power of individual factors - formalised methods did deliver *comparable cross-country evidence*, useful to settle at least some of those disputes. The latter stem, in part from differring assumptions, and in part from having advanced variously structured models, which could, at least on paper, aspire for equal treatment. *Empirical validity* of theirs is what is unlike, which is an important finding anyway, one which is not very frequently acquired in economic theory.

Another area where much of the controversy used to be was liberalisation. *Liberalisation* has several *dimensions*, as prices, wages, trade and financial markets and later asset markets have to be freed from entry and price setting barriers of various sorts. Whether or not this has to be done is not a matter of economic controversy any longer (through it remains a subject of political exchanges, of course). What remained highly controversial is the *speed*, the *scope* and not least the *sequencing* of liberalising steps. Those dissatisfied with the current state of affairs consider either the price as too high, or the sequence as mistaken, mostly premature, not taking account of the lower level of development and the heritage of decades-long seclusion. The ensuring distortions, rigidities and deformations in behaviour, all add up sizable cost elements to adjustment and re-establishing the market order in postsocialist countries.

With the benefit of hindsight it is not legitimate to question any more, whether or not export-oriented growth is *feasible*. Exports of central European countries have expanded at staggering pace in the 1990s. According to the *Economic Survey of Europe*, 1998/1. p. 207. for instance the Czech Republic nearly trebled its exports in 1992-97 from 8.7 to 22.5 bn$, Poland starting from 14.7 bn$ in 1989 reaching 26 bn$, or Hungary, starting from 9.6 bn$ in 1989 reaching 19 bn$ by end-1997, while having reoriented their trade from soft Comecon markets to OECD markets - the EU accounting for a larger share of their trade, than in the case of Portugal or Spain for instance. Opening up the economies helped orient structural change, i.e. *create the supply response*, overcome bottlenecks and previous shortages, thereby contributing directly to the growth of marketable output. Quite in line with Latin American experience (surveyed in *Nogues - Gulati*, 1994) *sustainability of trade liberalisation proved to be the key*. While more gradual opening strategies reverted to protectionism, sustained liberalisation helped *unlock other factors of economic growth*, provided it is part of a comprehensive policy, so that it could induce mutually reinforcing adjustment processes. This finding was supported by the experience of transforming countries: those who could sustain by the large *coherent macropolicies across the decade*, like Poland and the Czech Republic or Hungary, did benefit from the same improvements. Meanwhile those, like Romania, Bulgaria, Albania and Russia, where policies proved unsustainable, or those, like Ukraine or Serbia-Montenegro, where such policies have not even been launched, only contemplated, are obviously lagging behind. This is, of course the macro view, which is little comfort for those having been crowded out of business or laid off, but for *theory* it is the *macro* which really matters. This finding is also in line with an even broader overview of experience encompassing 73 countries. This has found that the *lower* is the level of initial development and the *higher* is the average level of *schooling* of the labour force, the *higher* is the *additional growth* generated by trade liberalisation. The latter does follow a J-curve pattern, with the first year 1, the second 1.5, the third and fourth years yielding 2-2 per cent additional growth on average (*Greenaway - Morgan - Wright*, 1998, p. 1556). This finding is all the more remarkable for a/ the authors have controlled for a series of alternative measures and possible errors and biases their estimates; and b/ one of them has published a much-quoted essay on „liberalisation seen through rose-tinted glasses" some years back, cautioning of the overestimation of the role of this factor. Quantitative methods and the sophisticated econometric methodology help clarify much of the issues such as biases and multicollinarity.

Last but not least *privatisation*, the third pillar of SLIP needs to be addressed. In the volumes quoted at the very outset we recurringly find the assessment critical of an alleged undue emphasis on the problem of ownership change vis-a-vis other issues. With the hindsight of roughly a decade, it is hard to share this view. The best proof is *indirect*: *none* of the countries having experimented with various third road solutions, or having opted for higher rather than lower share of public ownership could deliver *higher* growth rates, *lower* unemployment or *more welfare*, let alone the issue of *capital accumulation* (both human and physical capital included). The comparisons 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 is also being reverted, i.e. those advocating slower pace of privatisation, more state guidance, more public policies in areas other than those accepted in standard OECD market economies have to argue, more convincingly than before, for their position.

This fact-finding does *not* mean to legitimate the fairly ideological posturing of the early transition period, or that part of the literature which mistook progress in *ownership* change for progress in economic *well-being*, especially in the short run. I still adhere to my earlier position (*Csaba*,1995) on the natural rate of privatisation which is hard to supersede through direct governmental intervention, since the process is an outcome of a series of factors' and agents' interplay, domestically and internationally. Those e.g. who want to conclude major deals at the low ebb of the emerging market crisis may well be disappointed or simply lose a lot of assets for peanuts. But the theoretical issue, i.e. whether a relatively fast emergence of a system based on private property is feasible, whether this vouches warranty against backslide to statist totalitarian practices, has indeed been delivered. The comparison of Russia to Belarus, or Slovenia to Serbia-Montenegro clearly shows, that even in countries, where structural reforms progressed with a degree of delay, have made demonstrable gains in systemic terms against those, who still have been tinkering with improving the socialist heritage, manipulating bequeathed institutions via traditional techniques.

Privatisation in these terms ceases to be a primarily quantitative issue, a plan-target to be attained for a prefixed point of time. Rather, and quite like in market economies, it develops into a quantitative issue, namely of delineating business and politics, business and the state administration. Under this angle it is not the number of commodity exchanges or the number of private shareholders which 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 or not we see Korean or Italian type of intertwinings, whether or not we see *politically unbiased allocational decisions* being taken in growing numbers, whether or not we witness the emergence of capital market and banking arrangements which put *institutional barriers* to politically leveraged allocational decisions *to reoccure in the future*. Under this angle the *scope rather than the size of the capital market*, which really matters. It is the presence of strategic investors in the financial sector and the size of foreign - and thus by definition hard to leverage - owners which may serve as points of orientation. With the decrease of the share of industry in GDP - in case of Hungary it is only 27 p.c. in 1997 - generalisations based on the progress of ownership change in the industrial sector or even only in manufacturing, of what happened to the dinosaurs, seem to be of historical value at best. Meanwhile *progress or lack of it in the financial sector*, transparent or intransparent ways of conducting business, availability or lack of prudential arrangements and disclosure requirements according to international standads are the real criteria of evaluation, rather than any arbitrarily set quantitative plan-target.

From the above said there are at least two conclusions to be inferred. First, that upon the way the above discussed issues are being settled will depend the *type of market economy* that emerges in individual transforming countries. Here the path-dependency argument as well as a closer scrutiny of the new institutions is warranted. Second, privatisation is going to have efficiency enhancing effects on two levels. One is the immediate one - *static efficiency* - when overcoming obvious deformations or flawed incentives may produce miracles, both in terms of profitability and productivity, via making better use of available assets. *Dynamically* seen, however, the element of investment will come in. *Depending on the way financing new investments is being secured*, and the related *incentives* operate - towards efficiency or policy-determined choices - *the potential* for catching up, inherent in the Solow-model, *is going to* *materialise.* It is important to underline, that the neoclassical catchup model *presupposes* the availability of precisely those institutions and incentives for appropriate resource allocation, which are *only emerging* - to varying degrees - in the respective transforming countries. Thus their progress in financial sector reform will likely to determine the efficiency gains from privatisation in the short and medium run, as well as and the *growth-generating* effects of ownership change *in the longer run*.

From this it follows, that a strong rather than a minimal, a competent rather than a cheap, state administration which is required as a side-condition for transformational policies to succeed. A *strong* state is *not* necessarily a *much intervening* one, since its success indicator is output, i.e. effects brought about its intervention, not input, i.e. the number and variety of measures instituted in order to ward off responsibility for outcomes. And conversely, those who can be confident in the efficacy of their potential action, may not feel the urge to step in every now and then.

On the political economy of second generation tasks

What has been said both at the theoretical and on the transition policy level point towards what is increasingly described as political economy, where procedures and institutions *translating theories into policy matter a lot*. Currently available evidence has clearly shown that no country can save the tasks and pains of the SLIP plus financial sector reform, provided wealth is to increase and development sustained. The ongoing differentiation of the group of transforming economies implies, among others, the *delineation of subject* in the research area. For those studying the laggards a lot of policy analyses and sectoral/technical issues remain open, addressing basically the question of *how to graduate* from the present stage to a more mature one. This is interesting *per se* as area study, not however as an open theoretical issue, requiring new approaches. For those studying the more advanced or frontrunner cases, a so-called second generation transformation agenda emerges, which by and large presupposes that the first generation tasks are already successfully mastered. This agenda comprises tasks like creating a *sustainingly financable welfare system*, while retaining the competitive edge of the respective country in the competition among localities, in finding *new models for rural development* in densely populated regions, in addressing the problem of *environmental* decay, and generally in redifining the *role of government* in an increasingly comprehensive society which is an integral part of *globalisation*, where intertwinings put severe limits on traditionally postulated sovereign choices.

What has been said also implies that what *Williamson* (1994, pp. 26-28) termed as the Washington consensus is indeed, a shared vision of good policies for advanced and less advanced countries alike. As far as the most advanced countries are concerned, the critical essay of *Lányi* (1997) has neatly demonstrated the philosophical as well as the technical similarities between this and the deliberations of the Emu, the latter of course being more detailed as well as more restrictive on several bits and pieces.

As far as less developed countries are concerned, as the seminal paper of *Waelbroeck* (1998) has demonstrated, the particular branch of economics devoted to their study has undergone no less than *four paradigmatic changes* in the last half a century. It evolved from the Indian Congress consensus, focusing on import-substitution and industrialisation generated and managed by an activist state, to accepting the need for outward-looking policies and in the nineties also the Washington consensus. As *Waelbroeck* (op. cit. pp. 341-2.) rightly stresses, the original term is misleading in so far as it suggests an almighty global government located at the IMF headquarters, which is imposing its deliberations over a large number of undisciplined agents. If nothing else, the failure of the IMF to manage the contagion of the east Asian crises on emerging markets in 1997/98 has overwhelmingly demonstrated the *lack of* such a *supranational coordination mechanism*, operated consciously by a bunch of technocrats. On the contrary, what we witness here is *voluntary compliance* of a growing number of countries, not only with the liberal economic principles, but also agreeing to the need to fight corruption, make governmental and business operations more transparent, enforcing more prudential regulations. They are supporting democratic values not only for efficiency considerations, but also for their own sake. Therefore it is legitimate to follow his suggestion to talk of the *one world consensus*, implying that the proximity of rules and values have, to a large extent, been related to the intervowen world, to the informational revolution, to financial market globalisation as well as the resultant flow of ideas. These form norms and expectations, approximating priorities and expectations of a growing number of opinion molders and citizens alike.

This finding has two intermediate repercussions. 1. At the level of theory, *gone is 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 such phenomena as lack of credibility rob policies of their effectivness. 2. Policy advice, traditionally levelled towards a benevolent dictator, should not be directed towards a group which is but one among those shaping outcomes. Moreover - following the public choice school - policy-makers are not seen as selfless heroes any longer, but humans maximising their gains. Furthermore, many other actors, from households to foreign invetment funds, have to be convinced of the virtues of legislative and policy actions. Therefore the *target audience is the educated public at large*, and the job is to improve governance, i.e. to elaborate procedures and institutions able to *traslate* proper theoretical findings into the *interaction* of state, private agencies and groups of individuals.

This state of affairs has further implications. First there is a *growing similarity of the agenda of policy reform in the various country groups* under discussion. This, however is not making the lives of analysts and policy advisers easier, for two reasons. 1. There is *no „proven way"* in the sense of disinflation; the goals to be attained need to be discussed and elaborated. Normative concepts, as of *Kornai* [1998] are helpful only in specifying the subjects to be deliberated and discussed; there is no such thing as the single optimal outcome set by technical desiderata or consideratons. 2. As a consequence the lengthy *democratic decisionmaking process can not be saved at all*. Such considerations as speed, radicality, quantitative targets for a given period of time, the similarity of option A to any other currently available option /say, those in the EU/ may *never serve as a point of reference* any longer. This is a major difference from the early transformation period.

As a consequence, the role of feasibility studies, of coalition building, of compensating or buying out losers are gaining relevance against such traditional considerations as coherence or analytical simplicity. Sustainability considerations though do allow for *heterodoxy, not, however, any of its sort*. Dynamic modelling may be one of the more important analytical tools in forecasting what kind of compromise is *self-defeating* and which might be *self-sustaining*. Quantitative modelling does play a crucial role in specifying financability conditions as well as conditions under which these can be met. The longer term life cycle a reform has - like in the pension system - the higher is the relevance of these approaches, as well as their integration to 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 interest* (*Rodrik*, 1996, pp 22-4). The thinner is 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 a most serious impediment to change for the better. This might prove a particular challange to postcommunist societies, where the traditionally extended irregular economy and a decade of most ineffective legislation has made the norm-breaching behaviour more accepted than in most advanced countries.

The increased role of bargaining, while clear-cut - i.e. quantitative - measures of progress are often nonexistent, is likely to *enhance the scope for governmental failure*. Also the fact that incremental change is more likely to produce Pareto improvements, since the scope for large-scale experimentation and redesign is diminishing, can be a trap for the government.

More common *grounds* for governmental failure in the second generation tasks include: 1. inability to make committment; 2. coalition formation and bargaining dilutes the agenda; 3. destructive competition, inherent in party politics thwart the implementation of generally approved measures; 4. uncertainty about the consequences of change; 5. the simplicity constraint /not always the best professional arguments sell well/; 6. secrecy of bureaucratic decisionmaking, leaving the door open for special interest legislation (*Stiglitz*, 1998, pp 8-16).

There is no trivial textbook solution to these challanges. *Misalignment of incentives* is a near-inevitable by-product of the type of democratic decisionmaking processes that were shown above to be inevitable to try to figure out best and feasible solutins for redeeming entire macrosystems. Making use of sociology and organisational science in *devising the appropriate task force* might be a tremendous challenge. For instance in Hungary public finance reform tended to founder more than once on the composition of the task force, drawn basically from the lines of vested interest. Inability to delineate professional preparatory discussion with social harmonisaton, needed to secure public acceptance at a later stage of implementation, could not be successfully married. The outcome has been a situation when *only crises and coups,* deplored by most analysts, *could offset low-level equilibira*. Improvised solutions on the other hand, having been born by these exigiencies, rarely prove sustainable and deliver only part of the technically achievable at any point of time.

One of the further problems of consensual decisionmaking has to do with the tendency of *diluting concepts by way of overusing them*. The more a concept gains universal acceptance the higher is the likelihood of its being broadened to the point where its meaning loses significance. This happened to such core concepts as the social market economy. As the excellent summary of *Cassel* and *Rauhut* (1998, pp 17-24) has demonstrated, practical compromises as well as the feedback from practice have clearly indicated the conceptual limits to what used to produce the *Wirtschaftswunder,* the envy of Europe. The authors particularly caution against mixing up the overextended redistributory practices in the Federal Republic with those of the original theoretical construct, which itself is not free from unresolved contradictions. Therefore it seems that this concept may well be used only as one denoting an approach, an emphasis on *Ordnungspolitik*, the principle of competitive order trying to strike a balance with social justice, rather than any concrete policy prescription.

And here we have come to an important conclusion. As the example would be easy to replicate on a series of widely shared concepts, from subsidiarity to optimal regulatory areas, there is no quick fix. Subscribing to that particular interpretation of what economic theory has to say, which was unfolded in this article, *does not impose a straight-jacket* on further research of various sorts. While setting some of the frames for conducting productive discussions it is unlikely to settle disputes over methodology or even less on aplications of various sorts. Quite in line with the original Hayekian idea, what is likely to emerge is a competition among systems, not in the sense of capitalism versus communism, but in the sense of variants of capitalism. This competition is already very real with the emergence of such dissimilar models as the Swedish and the American, as well as owing to the unintended but very real spontaneous decentralisation processes in China, Russia and on the African continent. The more we remain at the study of real world systems and are thus forced to take account of the relevance of *empirical evidence* and the *feedback provided by this to theory*, the more inclined we might turn out to be for following the call of *Hodgeson* (1998, p.190) to attempt to *redefine economics as the study of systems*. In so doing we are unlikely to be forced to give up the traditions and methods of the discipline. On the contrary, the latter may provide us with the analytical frame by which this particular view of affairs might contribute to the different angles - and maybe worldviews - provided by other branches of social sciences, like social anthropology. In so doing we may go back to Marshall seeing economics not as a body of concrete truth but an *engine for discovering* the former. 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.

An if it is so, it is only right that with cross-cultural transmissions economic ideas also mutate as *Barber* (1995, p. 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 only a common languague and other forms of communication, which make individual country-, sector-, firm- and person-specific experiences transmittable to others. Appreciating the limits of the paradigm may be a way of becoming more forthcoming to concurring approaches. And it is going to be the task of the policy advisor and -maker to set up that policy mix, which is best suited to attain specific goals under the specific circumstances. The latter, quite laboriuos, task can not be saved by any amount of compromise on the side of researchers. On the contrary: it might be a good idea, if public policy figures and laymen could be sure *what kind of questions economists*, rather than magicians or historians *are more likely to answer in a satisfactory manner*, and which are the ones, for which we can not even attempt to provide an insight. Thus issues pertaining to wealth creation and maximisation may be more properly addressed to this guild than are moral issues, legitimating principles, justice and many other facts of life, which are taken as given in this particular approach. 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.

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