Towards a New Transition Economics

Katharina Pistor, Columbia Law School

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Comments on Konstantin Sonin’s “The End of Economic Transition”

Katharina Pistor

Abstract:
In these brief comments I take issue with the thesis expressed in Konstantin Sonin’s conference review that economic transition is a thing of the past. To the contrary, I argue that it has only just begun and so has the process of rethinking the economic theories and models that have framed the subfield of transition economics. Specifically, I point out that the many surprises economists have encountered when confronted with the actual results of the transition process reveal deeper flaws in the analytical framework that informs their analyses and policy prescriptions. If protecting theories and models is of first order importance, we should indeed call an end to economic transition and the subfield it has engendered. If, however, the issue is to understand real economies and their dynamic change over time and to better guide contemporary and future reform efforts there is a strong case for a New (Transition) Economics.
Konstantin Sonin has done a remarkable job summarizing the debates of a conference that honored 20 years of transition (Economies in Transition – 20 Years After) (Sonin (2012)). His summary captures reflections of academics and policy makers at the conference who were deeply involved in the process and have now offered lessons learnt. In my own comments I am reacting to this conference summary as an insider as well as an outsider: An insider insofar as I have closely followed and commented on the transition process myself; as an outsider in that I am not an economist, but a lawyer. From both perspectives the lessons learnt from 20 years in transition as presented in Sonin’s conference review are a great disappointment: It appears that little has been learnt from this unique social experiment and we should make an effort to understand why.

One of the most striking outcomes of the conference is that most participants seem to have agreed that transition is over (ibid). That might be true in a very narrow sense in that most (but not all) former socialist countries have left behind that particular model of central planning and political authoritarianism (EBRD (2011)). But this is a far cry from transforming these systems into vibrant market economies or democratic polities. Indeed, the very term “transition” is a misnomer in that it suggests that there is a single point on which all countries will converge. Complex systems hardly ever evolve along such pre-determined paths. The term “transformation” would have been much more appropriate (Polanyi (1944); Stark (1996)).

Economists resisted this term and the different conceptual approach that comes twenty years ago and they do so know, because it would require them to part with some of their analytical concepts. The first thing to go would have to be the notion of convergence and its close cousin, the debate about gradualism vs. radical reforms. There seems to have been a consensus at the conference that the latter debate has indeed been a waste of time without recognizing that the two are related. The debate about gradualism vs. radicalism makes sense only when the question is whether the same outcome -- convergence on some idealized version of the market economy -- could be achieved at all or in a superior fashion by either approach. Reading between the lines of the conference report there seem to have been less agreement on convergence than Sonin asserts. Convergence occurred with neighboring countries, not with the average market economy; EU accession was critical suggesting that convergence is not a natural outcome, but had to be pushed or pulled; and there was greater convergence on the economic than the political front. Several participants even offered explanations for the absence of convergence, such as the lack of civil societies or the problem of political capture. Note, however, that these explanations do not
explain the partial convergence where it did occur. Rather, they explain deviations from wholesale convergence they still assume as a feasible outcome contrary to the evidence provided.

Throwing out the concept of convergence would, of course, amount to a concession that the analytical framing of economic transition was flawed from the outset. It is apparently easier to conclude that transition is over and to return to the very same economic models that existed at the outset and that failed to explain what actually happened. That caused several major surprises, because existing economic models did not predict these outcomes and cannot explain them, which in turn has prompted economists to search for explanations outside economics.

The first big surprise Sonin identifies is that price liberalization did not result in the reallocation of scarce resources to the most efficient users. As it turned out, once prices were liberalized, not enough food made it into the cities. As city dwellers struggled with rising food prices they cut back on consumption of other goods, which depressed economic growth. This, of course, is a surprise only if one has never thought about the institutional and physical infrastructures that constitute markets. Economic theories that assume but don’t explain markets abstract from these factors and at most depict them as ‘constraints’ or add-ons to an imagined markets that exist outside any structures. Unfortunately, two recent strands in economics, the new institutional economics and law and finance, don’t do much better. The new institutional economics points to the importance of institutions, but in the end does not explain how they come about ultimately taking flight into the two black boxes of history and culture (North, Wallis and Weingast (2009)). And the law and finance literature concluded after ten years of intensive debate that its favored model of pro-market institutions associated with the common law would triumph (only) as long as financial markets continued to operate smoothly (La Porta, Lopez-de-Silanes and Shleifer (2008)) -- which unfortunately they ceased doing just as that paper went to press.

The second big surprise was the rise of the oligarchs especially in Russia, or more generally, the rapid divergence of wealth and opportunities in transition economies. Economic theory predicted that the liberalization of markets and the introduction of private property rights should result in something approximating equilibrium outcomes. It did not reckon with structural features of the socialist mode of economic and social organization that had concentrated power, resources and information at the center, which would prove difficult to dislodge. The obsession with liberalization as a strategy to free up markets that
were presumed to arise spontaneously and did not need to be built stood in the way of reforms aimed at reallocating power and redefining the modalities for exercising it. That would have been required a political and constitutional reform process at the outset of reforms, which was rejected, because it would have delayed the process of economic reforms (Sachs and Pistor (1997)).

Privatization also produced a series of ‘surprises’. Taking the cues from Sonin’s summary of the lessons learnt from privatization it appears that small-scale privatization worked pretty much irrespective of the privatization method chosen, but large-scale privatization was more difficult, and loans-for-shares was a disaster. Closer inspection suggests that large-scale privatization succeeded when designed to produce owners that could contribute capital to the restructuring of enterprises (Frydman, Pistor and Rapaczynski (1996)) and when done in a context of ‘good’ rather than ‘bad’ governance so that the new owners would invest in rather than loot their companies (Black, Kraakman and Tarassova (2000)). That does not seem to be so surprising given what we know about the difficulties of effectuating corporate governance in developed economies (Hopt, Kanda, Roe, Wymeersch and Prigge (1998)), but leaving it there would be unfair to the designers of mass privatization programs. They did not neglect the importance of capital; they only assumed that capital markets would arise spontaneously once shares had been widely distributed (Boycko, Shleifer and Vishny (1993)). Neither, did they ignore the relevance of good governance; they were only guided by Marxist ideas that changing the economic foundations would inevitably result in the reorganization of the political and legal superstructure. Privatization was supposed to do just that --- to change control over economic assets and thereby create a new class of owners that would demand the institutions they needed to defend their rights (Boycko, Shleifer and Vishny (1995)). Max Weber would have cautioned against this prediction. He pointed out that economics rarely if ever determines political or legal systems (Weber (1978)). Some advocates of mass privatization appear to have conceded this point by later arguing that law is determinative of markets (La Porta, Lopez-de-Silanes, Shleifer and Vishny (1998)), arguably pushing Weber’s argument to the extreme.

A final surprise discussed at the conference was the fact that the population in transition economies has been less enthralled with the economic freedoms transition economists bestowed on them than they expected. Polls suggest that people place greater trust in the ability of strong leaders to set things right than the powers of the market place. While it may well be the case that trust and civil society play an important role in explaining this outcome the flight into
explanations outside economics may by a little too rushed. Given the surprise factor among economists about the outcomes their policies produced one might surmise that the real markets people experienced were quite different from the ideal textbook markets that had inspired reformers and policy advisors in the first place. Exposure to real markets may have helped erode trust rather than build it and data on the rise of corruption in several countries points in this direction (Kaufmann and Siegelbaum (1996)). It may not be possible to ever establish a clear causal relation between trust and reform strategies. Still, using civil society or rather its absence for explaining deviations from the predictions of economic models does not prove these models right.

In short, I agree with Konstantin Sonin that it is time to call and end to transition economics as we know it. This, however, is not the same as saying that economic transition is over. Perhaps he is right in calling for political economy as the answer, but I am less sure what he means by the “unified framework” it is supposed to provide. However one may want to label this new field -- which for purposes of convenience I will call the “New Transition Economics” -- here are some thoughts as to why it is important and what it should accomplish.

Transition is far from over. The Arab Spring has triggered change and created opportunities for further change in a part of the world that only recently many had viewed as stable and settled, though in a ‘low equilibrium’ trap. The problems these countries face are in many respects different from those of the former socialist countries, but they also share similarities. More importantly, they are in need of real, policy relevant lessons from other transition processes so as not to repeat their mistakes. Perhaps even more importantly, the very countries that served as the ‘outside anchor’ (Berglöf and Bolton (2002)) or benchmark for the former socialist countries on which they were supposed to converge currently find themselves in the midst of their own transformation. The global financial crisis has shown that the institutional structures of these countries are far from stable and may indeed not be sustainable. Economies are dynamic systems; they change all the time even though the pace of change may vary. Perhaps it is time to focus on processes of change rather than points of convergence or equilibrium outcomes.

The key question then is how to do better next time around than spending 20 years of testing existing models only to find oneself in disbelief about the empirical results they produced. The above comments leads me to conclude that the answer lies in rethinking existing models and theories. There are at least two strategies for theory development (Viskovatoff (1999)) – a deductive one that
starts with a coherent theoretical framework, generates hypotheses and tests them empirically; and an inductive one that builds theory from observable facts. The models thus generated are then tested against new data. Contemporary economics has pursued for the most part the former approach, but given the disappointments incurred when applied to transition economies perhaps greater efforts should now be spent on the latter.

The data presented and discussed at the conference on “Economic Transition – 20 Years After”, and the evidence presented already offers some building blocks for such a new theory. First, politics is not exogenous to economics. Political institutions, political choice, and the selection of implementing strategies have shaped the course of transition and its outcomes and thus need to be endogenized. Second, there seem to be strong path dependencies in social and economic development. Yet, some economies/societies do change, while others don’t; some countries experience spurts of change that fizzle out (Rodrik (2000)), while other countries undergo deeper transformations. Existing theories of change put a lot of hope on ‘exogenous shocks’ for ushering in change (Olson (1982)); yet real world experience suggests that they may only reshuffle the deck-chairs rather than result in deeper change. We therefore need theories that help exchange the processes of change and their variation. Third, context matters. If neighbors have a substantial influence on the direction of transition we need to understand why and how and what that tells us about the social learning processes that accompany the building of markets. Last but not least, detail matters. Whether firms are big or small, whether new owners have effective control rights or not, and how to design such control rights in light of existing institutions and their propensity to change may determine success or failure of privatization programs or similar reform strategies (Kornai (1990)).

Generating new theories from these insights is still a tall order. It will be messy and often difficult to reconcile with existing models and research strategies. And yet, an empirically grounded theory of economics may offer better guidance for the real world, and perhaps leave us with fewer surprises when looking back in another twenty years.

References


