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Symposium on

Institutions and economic performance

Abhijit V. Banerjee and Maitreesh Ghatak
Department of Economics, MIT. banerjee@mit.edu
Department of Economics, LSE. M.Ghatak@lse.ac.uk

Introduction

One of the most welcome developments in economics over the last twenty-five years is the resurgence of interest in institutions. The conceptual work of North (1981) and Williamson (1985) on why institutions are important, along with theoretical developments in the economics of contracts and organizations, set the stage for a large empirical literature that documents the enormous differences in institutions across countries, and goes on to argue that these can explain a large part of cross-country differences in output per capita.

A large part of the debate today about institutions centres around this last claim. The problem, as the literature well recognizes, is that institutions are at least partly endogenous: they are as likely to be the product of things that are going on in the economy, as the cause. The seminal paper by Acemoglu, Johnson and Robinson (2001) (henceforth AJR), tries to address this issue by focusing on the parts of the variation in the institutional environment that are the result of one specific incident in their history – the experience of European settlers when they first arrived in the country. In their story, if the early European settlers experienced high mortality the colony was never heavily settled by Europeans. The institutional structure in the colony then evolved in a way that would make it possible for a small number of Europeans to rule over an overwhelming majority of non-Europeans. Where early settler survival was easier, Europeans settled in larger numbers and the institutions evolved very differently.

Sokoloff and Engerman (2000), in their equally seminal contribution, emphasize the variation in institutions that resulted from the fact that in the 17th and 18th centuries, different colonies represented different economic opportunities to the

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colonists. In particular, colonies whose geography favoured labour-intensive products that happened to be in demand at that time (such as sugar-cane plantations and mines), ended up with a large underclass of slaves and almost-slaves, and institutions aimed at keeping them in their places.

The important series of papers by Shleifer and his collaborators (see, for example, La Porta, Lopez-de-Silanes, Shleifer, and Vishny, 1998; Djankov, La Porta, Lopez-de-Silanes and Shleifer, 2003; Glaeser and Shleifer, 2002) on legal institutions emphasize the influence of the colonizing country. The presumption is that the identity of the colonizer was an accident that imprinted the legal system of the colonizer onto the colonized country.

In each of these papers and their many follow-ups, the primary source of variation is something that happened a long time ago, that, at least apparently, seems to have very little to do with economic fortunes of the country today. In this sense, it has the potential to be an ideal instrument for institutions. On the other hand, it is not easy to see how it would pass the exclusion restriction test – that would require us to believe that the only reason why the colonial encounter remains salient today is because it shaped institutions, and not because it influenced, for example, language, culture or the nature of the elite. So it remains possible that institutions are taking the blame for other, less easily measured, manifestations of the colonial experience.

The literature, of course, is entirely aware of this possibility. Acemoglu and Johnson (2004) argue that their interpretation of the reduced form relation between historical accidents and economic performance today, as the effect of certain specific institutions, works better than other interpretations that the data allows. Glaeser and Shleifer (2004) disagree and suggest a different interpretation. They argue that human capital is a more basic source of growth than are institutions and that it is economic development that leads to the improvement of political institutions rather than the other way round. In the end however this is not a question that the data can entirely resolve.

It is worth asking, however, what we would do if, by some miracle, we did learn the truth about this question and it turned out that it was indeed certain specific economic institutions that made the difference. Should we expect things to take off as soon as we set up these new institutions? Or do we think it could be a long wait before we see an impact? The evidence cited above, does not help us here, since it deliberately focuses on institutions that were the result of a historical process that lasted hundreds of years. However when this process is played in reverse, in other words when an institution is abolished, the data seem to suggest that the effects do not go away immediately. Banerjee and Iyer (2004) find that the effects of British land tenure institutions in India persist (and indeed seem to grow) long after the British have left India and these institutions have formally been abolished. Banerjee and Iyer argue that these effects are not driven by the endogeneity of these institutions or omitted variables but by the fact that these institutions and the inequities that they created shaped subsequent policies and also generated a conflictual environment that negatively affected resolution of collective action problems. The paper by Hoff and Pandey in this issue confirms that past inequities and antagonisms may continue to be relevant even though the formal institutions supporting them may no longer be salient through the collective memory of the people in these areas. They show that while the caste system in modern day India has no formal institutional legitimacy, it continues to have an effect because people have internalized the stereotypes it created.

It is possible, however, that this evidence is also misleading, since the underlying institutions in both these examples were notoriously associated with a long history of oppression. One might imagine that the average institution does not come with such a burden, but in making such distinctions between different types of institutions we are relying on some theory of institutions that has not been properly articulated. When we talk about instutions, we have the broad notion that they impose a certain necessary orderliness and predictability on economic transactions, but we usually do not say exactly how this is achieved. Is a creditor-protection clause important because of its direct instrumental benefits to creditors, in which case the effect might be immediate? Or does it primarily provide a focal point or a default, around which private citizens build their much more complex private rules (as Hoff and Pandey suggest), in which case there may be a complex and drawn-out process of learning and coordinating before there is any impact? Or is it mainly a signal of the broader intentions of the state towards the business sector, in which case the effect will depend on what other laws are being instituted?

This is one place where a more micro approach would clearly help. By talking about one specific institution, it would make it easier to have a richer theory of how exactly the institution functions as well as to look at evidence on how exactly and on what time-scale it has an impact. Consider, for example, Goldstein and Udry's (2004) study of communal land tenure in Ghana. Under this system, village politicians allocate land based on perceived need and political influence. Under the prevailing norms, land cannot be taken away while being cultivated but it can be taken away while it is being fallowed. Those who are politically less well-connected, therefore try not to keep land fallow, and this hurts productivity. On the other hand, it is not clear how we are supposed to think about the impact of policy reforms here, since the institution of communal tenure is a part of a broader set of traditional institutions that govern a very large part of life in rural Ghana.

In a sense, the above discussion points to a more general problem for the literature on institutions: how should we think about where institutions come from? Clearly the whole question of what kind of institutions we should aim for would be of little relevance if we took the so-called positive economics view (following Friedman, 1953) which says that economists should always assume that the world knows better and hence should stay away from criticizing the institutions we observe. Fortunately over the last twenty-five years, the economics profession has moved away from this position. The political-economy view that has supplanted it as the dominant view, argues that we only get new institutions when they serve the interests of those who are in power (see, for example, Acemoglu, 2003). This need not mean that they never do anything that would help anyone but themselves: but if they do so, it is to pre-empt a threat to their future pre-eminence (see Acemoglu and Robinson, 2000 for an interesting example of this kind of reasoning).

An awkward implication of this kind of model is there is not much one can do to improve institutions, unless one is prepared to take on the elite. The important work of Greif (1994) suggests a way out: in his framework, changing institutions requires coordination, with the implication that institutions can persist long after they have outlived their utility. There is, therefore, the possibility of improving institutions. However, Greif does not offer a theory of how the coordinated move to a new institution is supposed to happen. Indeed it is not clear how one would even go about building such a theory: for the case where the institution just consists of an informal collaboration of many individuals one can perhaps take a decentralized network-formation approach (along the lines suggested in the paper by Munshi and Rosenzweig in this issue). However, where there are many detailed rules (as in most formal institutions and many informal ones), the theory would have to be built around the ideas of leadership and agenda-setting.

The role of leadership and social entrepreneurship more generally, becomes even more salient when we drop the assumption that we always know what the institutions ought to be. Banerjee (2002) makes the case that it is much more realistic to think of institutional innovation in more or less the same frame as most other innovations: somebody has to come up with a new idea, or at least a new way of combining existing ideas. At this point we have no vocabulary for talking about what would make that task easier or harder, about how the innovator gets rewarded, about the process by which the innovation spreads, or about the politics of the process that allows institutional innovations to happen.

This symposium was organized with the aim of bringing together a sample of interesting recent research on these questions. The five papers that were selected look at various aspects of the problem. The paper by Hoff and Pandey looks at how institutions affect beliefs and can have persistent effects even though the formal structures are removed. The paper by Munshi and Rosenzweig studies how traditional institutions such as caste-based social insurance networks respond to economic development. The paper by Bandiera, Barankay and Rasul studies how institutions affect the resolution of collective action problems. The papers by Bardhan, and Rodrik and Rigobon carry forward the empirical research agenda of identifying the effect of institutions in cross-country data. We hope this symposium offers a key point of transition: institutions have been given the central place they deserve in economic analysis. Now we need to analyze them.

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