Daron Acemoglu and James Robinson, *Economic Origins of Dictatorship and Democracy*. New York, NY: Cambridge University Press, 2005. Pp. xv, 416.

Economic Origins of Dictatorship and Democracy is impressive not only in the analytic argument provided, but also for what it represents for research in modern economics and political economy. Before the professionalization of economics as a scientific discipline in the 20th century, political economist sought to offer broad arguments that explained the differences in the pattern of wealth across countries. Why didn't China develop faster than Europe? Why did the Islamic crescent at one time the pinnacle of civilization ceased to continue its dominance in science, art and commerce? However fundamental these questions were, they seemed to defy clear answers that could be expressed in the language and tools that were increasingly accepted as the demarcation devices between science and non-science. As economics became more professionalized, these questions of a less "scientific" age were pushed aside in favor of more technical questions of microeconomic efficiency and macroeconomic balancing.

But the empirical reality that inspired the earlier comparative questions did not go away. Instead, they actually grew more acute. Rather than the convergence in economic growth that was predicted by the mainstream model, we witnessed divergence. And, in addition to the differences in economic performance, it became increasingly obvious that differences in political systems seemed to persist. There was no upward and onward climb to stable democracy that coincided with modernity. If professional economics wanted to remain relevant, then it better be able to answer questions concerning the comparative performance of economic systems and the comparative structure of political institutions, and if it wanted to be really relevant it would be useful if it could explain the connection between both sets of questions in a coherent political economy framework. In other words, research at the beginning of the 21st century starts where it left off at the end of the 19th century. Political economy is back in vogue among economists and political scientists, and comparative institutional and comparative historical analysis is the mode of inquiry that the best work in modern political economy pursues.

Another way to think about this, is that economic inquiry takes on an hour-glass shape. The end of the 19th century represents the top of the hour-glass, the mid-20th century of an institutionally antiseptic theory of economic behavior represents the narrow middle of the hour-glass, and the period from late 20th century to today is represented by the bottom part of the hour-glass as the questions asked and the model of analysis broadens to tackle the "big" questions of social systems of exchange and production, and alternative political structures. The history of political economy over the past century cannot be adequately told without thinking through this metaphorical image of the hour-glass, and tracing out the cultural reasons for this trajectory of economic inquiry.

Daron Acemoglu is one of the main intellectual agents of change in this transformation of modern economics and political economy. He inherited a grand tradition in political economy (though he appears to be ignorant of it, which I will talk about later), but there should be little doubt that his work (often with James Robinson, but not exclusively) has captured the intellectual attention of the academic world in a way that those in that previous generation of political economists never did. In Acemoglu's hands, comparative political economy is something that is respected by the faculty and students at MIT, and gets published in the *AER*, *QJE*, and *JPE*. For young scholars in the field of political economy, the awarding of the John Bates Clark Award to Acemoglu in 2005 is as big a professional event as the awarding of the Nobel Prize in 1986 to James

Buchanan was to my generation. But we have to admit (no matter how much we want to resist the lure of fad and fashion in science) that the Clark Award is a better barometer of what is currently fashionable than the Nobel. To win the Clark Award means you are "in" the club, to win the Nobel means the club recognizes that you did something of value whether they consider it to part of the in-club now or not is a case by case matter. With Acemoglu's Clark Award it is evident that political economy is once more firmly established in the mainstream of the profession of academic researchers in economics and political science.

In essential character, Buchanan's work in political economy was analytical and tackled puzzles that arose against a background of the failure of fiscal restraint and monetary responsibility in the western democracies during the post-WWII period. It was not historical, and it was not explicitly comparative. Gordon Tullock tackled issues of non-democratic states and the paradox of revolution, but his pioneering analysis of these issues has had a limited influence among mainstream economists and political scientists. Moreover, his work while historically informed, was not primarily historically narrated. It was instead, primarily analytical with some use of history to illustrate. It was not what could be termed deeply historical.

Acemoglu and Robinson are not "deeply historical" either, but they are more rooted in historical narrative than the work of previous political economists. And they are explicitly comparative. They study four case studies: Britain, Argentina, Singapore, and South Africa. Each of the cases is selected to represent a exemplar of the alternative political pathways that can be followed. The consolidated democracy that does not experience reversals (Britain), unconsolidated democracy that eventually reverts back to non-democracy (Argentina), stable non-democracy that can persist without significant repression (Singapore), and non-democracy that requires repression to survive (South Africa). The historical time period examined in these cases is the lengthy consolidation process in Britain from the Glorious Revolution (1688) to the establishment of full universal suffrage for men and women (1928): to the oscilation between democracy and non-democracy in Argentine from the constitution of 1853 through military coups, Peronism, radicals, and liberal reformers in the 20th century; to the experience with non-democracy in the 20th century in Singapore and South Africa.

But Acemoglu and Robinson's book is not a work in political history. Instead, it is a work in the analytic narrative tradition of scholarship in political economy (I reviewed the Bates, et. al., *Analytic Narratives* for this journal). The fundamental contribution of the book is to be judged on the *analytical* level, with the narrative employed to illustrate the interpretive power of the core model. The main idea is to expose the underlying causal mechanism that is at work in political-economic reality.

Acemoglu and Robinson have two basic ideas they are attempting to model: Conflict and Commitment. The first idea is that political life is full of conflicting interest groups. They pair these group downs to two --- the ruling elite and citizens (disenfranchised social classes at the start). The ruling elite will make concessions to democracy only in situations where the disenfranchised can form a coalition and threaten the ruling power of the elite. But the disenfranchised citizens have good reasons to believe that the ruling elite will renege on their promise of concessions in times of political tension once order is restored. Thus, we have the basic game structure. Ruling elite are offering the promise of concessions to the citizens, but they have trouble making those concessions in a credible manner. Policies, Acemoglu and Robinson reason, are easier to reverse than fundamental political structures, so the social classes agitate for change will cease the pressure for overturning the ruling elite in situations where the elite agree to concessions in the form not of policy but of fundamental political structure. Movements toward democratic rule emerge as a commitment device by rulers to appease the conflict with competing social classes, which would have overturned the regime had no concessions been made.

With this theory in hand, Acemoglu and Robinson go out to explain how Britain resolved social tensions through consolidated democracy, how Argentina was unable to credibly commit to fundamental political change and so its movements toward democracy were unconsolidated, how Singapore was able through industrialization in the post-colonial period to ease social tensions and thus eliminate the need for democratic consolidation and also the need for repression, and how South Africa was unable to ease social tensions and unable to make the necessary credible commitments, so its non-democratic regime had to resort to repression to survive in power. The narratives are different, but the general story is the same and it is about political survival as coping with conflict, and commitment devices are essential for that coping.

This is a parsimonious and powerful theory for constructing the comparative analytic narratives that Acemoglu and Robinson provide. However, Jon Elster raised a very important issue in his critical essay "Rational Choice History: A Case of Excessive Ambition," *American Political Science Review*, when he argued that in constructing analytic narratives, a disregard for facts cannot be excused because of the beautiful simplicity of the analytic model constructed. To put it another way, when novelists construct narratives we do not hold them to any factual standard, but the best novels do ring “true” to us. Novels are true, but not factual. This, however, is an unacceptable outcome for scholarship. When we write political-economic histories, we must be factual and if we are lucky they will also be true.

So do the stories that Acemoglu and Robinson construct fit the facts and ring true? Here I think a reasonable debate should be engaged. Political and economic historians have documented that revolutionary challenges to elites do not come from the destitute. They are too poor, too weak, too busy just trying to survive to be politically outraged. No, revolutionary challenges come from competing elites. Claims of “dual power” drive revolutionary moments. How would the historical narrative be different if Acemoglu and Johnson looked at the way that ruling elites must negotiate with challenging elites, in order to maintain order over the masses? The masses must be addresses because they fuel the economy and thus the tax system, but the challenges come from competing elites.

But reasonable people can debate these issues. Acemoglu and Robinson also argue that the relationship between economic growth and democracy is weak, and that it is other historical factors that determine the transition to democracy other than economic growth. Again reasonable people can disagree, but there are two empirical issues which their perspective cloud out of view right from the start. First, the observation that the countries that experience economic development prior to the transition to democracy tend to adopt democratic institutions that constrain the confiscatory power of the ruling elite, whereas when countries pursue democracy prior to economic development the democratic institutions adopted maximize the redistributive powers of the state. Second, the observation that many of the recent transitions to democracy have in fact been transitions to illiberal democracies precisely because they were transitions to democratic forms which maximized the redistributive powers of the state, rather than constraining the confiscatory powers of the state. The idea of an illiberal democracy doesn't fit neatly into Acemoglu and Robinson's analytical framework.

These complaints are empirical in nature. But there are also analytical sins of omission in Acemoglu and Robinson's ambitious book. The most egregious omission is the failure to cite and discuss the work of James M. Buchanan on the time consistency problem and credible commitment at the core of the analysis of government. Geoff Brennan and James Buchanan's *The Power to Tax* and *The Reason of Rules* address this foundational issue. And Buchanan's *Limits of Liberty* is focused on the constitutional contract that negotiates our way out of the conflict equilibrium. Buchanan shows how it is through constitutional-democratic contract, that we move from politics as games of conflict, to politics as games of exchange. Buchanan's overriding puzzle is how in this transition to democracy, the protective state (police, courts and national defense) and the productive state (public goods) can be empowered without also unleashing the redistributive state (the churning of special interests). Buchanan's concern with the redistributive state undermining the democratic order is in direct contrast to the argumentative line that Acemoglu and Robinson put forth in their work where redistribution is the key tool for constructing sustainable democracies. In short, a charge of intellectual irresponsibility can be leveled at *Economic Origins of Dictatorship and Democracy* in Acemoglu and Robinson's failure to at least engage Buchanan's contributions to this field even if ultimately the judgment by Acemoglu and Robinson would be that Buchanan didn't solve the problem effectively.

The sins of omission are not limited to Buchanan's foundational contribution to the field he created --- constitutional political economy --- but also to other more recent work in the field: Kuran's work on preference falsification and revolutions; Weingast's work on the paradox of governance and on how market-preserving federalism as an institutional solution to the commitment problem; Rodrik's work on promises, promises and the idea of policy overshooting to signal commitment; Olson's work on the intricate institutional mix that is required for countries to realize the extensive benefits of the gains from specialization and trade; and Shleifer's work on predation and institutional possibilities in coping with the grabbing hand of the state on the one hand, and the opportunism of private actors on the other. In each of these contributions to political economy, the authors wrestle with the core idea of coping with games of conflict by establishing binding and credible commitments so that these games can be transformed into ones of cooperation and exchange. Not that I would expect a citation, but my own work in *Why Perestroika Failed* and several subsequent articles examined Soviet and post-Soviet economic and political reforms in this credible commitment framework as well. My point is rather the simple one that the claim to originality in Acemoglu and Robinson on the analytical front is "thin" and the historical narrative offered is a bit "forced".

Despite these concerns, *Economic Origins of Dictatorship and Democracy* is a welcomed addition to the literature and will undoubtedly attract attention to the field of political economy and spur much research on the nature and history of democratic transitions. They do conclude that the future of democracy is bright, though their prediction ultimately is that mature democracies will be less redistributive than what might be expected and desired. This conclusion is not reached because of an ideological revolution protecting property and prosperity, or even a new found respect for the scientific argument for constitutional constraints on democratic proclivities to take from some and give to others in exchange for support, but because of the organizational logic of mature interest groups and the abilities of the elites to use that organization. Unfortunately, this conclusion, like several other aspects of the book, runs into the problems of being factual and being true. A narrative of the economic analysis of dictatorship and democracy cannot really proceed without a discussion of the attempts to constrain the powers of the state; without dealing with the problem of predation; without looking at how through constitutional contract we can transform games of conflict into games of cooperation. The analysis of concessions and redistribution doesn't quite get the story right.

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