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Over the past few years, in a series of important articles,<sup>1</sup> Daron Acemoglu and Jim Robinson have developed an influential theory about the creation and consolidation of democracy. In this book they summarise and extend their analytical results within a unified, coherent and elegant framework, and illustrate their insights with historical examples. Acemoglu and Robinson seek to reach beyond economists and formal political scientists who are already familiar with their articles. Their ambition is to convince a broader audience of political scientists, sociologists, historians, and other scholars that the 'economic' approach, based on stylised and simplified analytical models, brings valuable insights and a deeper understanding of complex political phenomena. The authors want to convince a potentially sceptical audience that *heroic generalisations about democracy are good* - a challenging task in an area of study in which 'it seems that the "general propositions" are that there are no general propositions' (p. 81). As a card-carrying economist who shares their love for parsimonious and general models, I warmly endorse their methodological approach. In fact, I have been reading this book like a fan sitting on the sidelines and rooting for his favoured team, rejoicing when they score a spectacular goal, and suffering when they hit a pole or send the ball out. Now I am ready to engage in what American football commentators call 'Monday morning quarterbacking' ('il processo del lunedì', for Italian readers).

Their kick start is perfect: Occam's razor. 'Entities should not be multiplied beyond necessity' 'Be as simple as possible'. A more 'nerdy' version of Occam's razor would be: add complications to your model only as long as the marginal benefit from doing that, in terms of explanatory power, is larger than the marginal cost. Acemoglu and Robinson know that the marginal cost of complexity is high, especially in a book addressed to a broad audience and with ambitions to provide a unified understanding of a vast and diverse set of events. Hence they strive to provide a simple, unified and flexible framework, which is maintained, with gradual extensions, through the whole book. The main questions, ideas and facts are introduced in Part I. Part II provides a user-friendly

<sup>1</sup> Most notably, Acemoglu and Robinson (2000, 2001).

introduction to basic modelling, while the central results about the creation and consolidation of democracy are presented in Part III. Part IV covers a rich set of extensions and applications. Part V contains the authors' concluding remarks about areas for further research and the future of democracy.

In the simplest version of their model, there are only two groups: the elites and the majority of the citizens. The citizens prefer democracy because it allows them to implement their favoured redistributive policies. For that same reason, the elites prefer to keep dictatorial power. In the absence of democracy, social unrest and the threat of revolution may induce the elites to make concessions. But concessions may not be credible in a dictatorship because current policies can be modified by the elites themselves in future periods. In contrast, democracy credibly transfers power to the citizens. When the costs of repression are high enough and promises of concessions are not credible, the elites may be forced to democratise as the only way to forestall costly disorder and revolution. Democratic institutions consolidate when the elites face low incentives to overthrow democracy in a coup and revert to dictatorship.

This is a very crisp and insightful model, which clearly links preferences over economic policies (taxes) and political institutions (democracy) to fundamentals (material endowments). But, as a Monday-morning quarterback, I dare say that the authors could have used Occam's razor even more aggressively, in order to point out their different assumptions and insights to the reader. Their basic model collapses two logically distinct sets of assumptions. One set underpins the basic structure of the power game in which the elites choose current policies but can also choose state variables that affect the future distribution of power (institutional change). For example, at the beginning the authors could have presented a simpler, more general game with a group *A* that holds power dictatorially, and a (larger) group *B* that is excluded from power. Group *A* prefers policy *a*, and group *B* prefers policy *b*. By choosing more 'democracy', group *A* can increase group *B*'s probability of holding power in the future (for simplicity, the probability could go from 0 to 1, as in the book's basic model). All results about credibility, repression etc. could perhaps have been illustrated within this simpler, more general framework. Once the general logical structure of the argument had been presented, the authors could have moved to their second set of assumptions – that is, political conflict as being about *income redistribution across class lines*. This approach might have allowed the authors to separate their general insight about democratisation as a commitment technology (democratisation 'locks in' temporary *de facto* power – a neat extension of an idea originally developed by North and Weingast (1989) in a different context) from the important but more specific application to redistributive conflict between the 'rich' and the 'poor'.

An additional question is whether the best way to model redistributive conflict within a society is by using a simplified Meltzer and Richard (1981) setting. Certainly, the approach has pedagogical advantages because of the popularity of this framework in political-economy articles and textbooks. But the authors are well aware that the empirical record provides little support for the standard Meltzer-Richard predictions. In particular, extensive empirical investigations have not found the implied positive relationship between inequality and redistribution in democracies, therefore casting a shadow over the mechanisms underpinning the book's results about the relationship

between inequality and democratisation.<sup>2</sup> Acemoglu and Robinson do provide extensions that partially 'insulate' their model from some of those counterfactual implications. For example, in subsections about 'alternative political identities' (e.g., races or ethnic groups), they extend the basic framework to allow for conflict over income redistribution between two different groups which are *not* defined along class lines (e.g., each group includes some rich and some poor).<sup>3</sup> Such extension is conceptually useful and realistic, but uncouples the relevant concept of inequality from standard inequality measures, making the basic theory difficult to test, at least with currently available data.

As Acemoglu and Robinson recognise, a fundamental issue that complicates the empirical study of the relations among democracy, inequality and redistribution is that all three are endogenous in the long run, and affect each other in complex ways. Still, considering the well-known creativity and industriousness of the authors, a loyal fan might have expected a more consistent effort to bring the model to the data, and clearly show the explanatory power of the economic approach to the sceptical audience mentioned above. However, the authors do present a broad review of stylised facts about democracy in chapter 3, and argue that these facts are loosely supportive of their main insights.<sup>4</sup> Their fascinating discussion of case studies is limited to a handful of well chosen examples (mainly Britain, Argentina, Singapore and South Africa). An interesting question is the extent to which these examples are representative of a larger group of countries, and illustrate the main mechanisms in their framework. For instance, to what extent is Singapore representative of Taiwan or South Korea? And, in explaining Singapore's stable non-democracy, how paramount is the relatively low level of income inequality, rather than other characteristics (say, huge rates of economic growth) that may set Singapore apart from different countries with similar levels of inequality?

When analysing non-democratic regimes, the authors mainly focus on 'right-wing' dictatorships, as they did in their original articles.<sup>5</sup> In the simplest version of the framework, revolutions against the rich are 'off the equilibrium path', and even when they take place, they do not lead to 'post-revolutionary' dictatorships. In this respect, this book is quite different from its illustrious inspiration, *Social Origins of Dictatorship and Democracy* by Barrington Moore (1966), which included a detailed analysis of communism. The lack of an explicit role for communist parties and regimes may miss a big chunk of the political mechanisms and conflicts at work during the 20<sup>th</sup> century. For instance, a disproportionate number of dictatorships in the database provided by Beck *et al.* (2001) are classified as 'left-wing'. It is almost paradoxical that a book so

<sup>2</sup> Specifically, in their setting the relationship can be U-shaped, with democratisation occurring at intermediate levels of inequality, while low-inequality societies and high-inequality societies stay non-democratic (pp. 189–93).

<sup>3</sup> An interesting issue for the theory of democratization, which is not explored in the book, is the possibility that the main conflict between these political groups might not be over income distribution but over other policies (language, religion etc.).

<sup>4</sup> A systematic attempt to study the empirical relations between democracy and redistribution is in Boix (2003), who focuses on wealth distribution and factor mobility as key determinants of democratisation.

<sup>5</sup> For instance, in a footnote to their article on transitions, they wrote 'Obviously, in practice, there are dictatorships that are against the interests of the richer segments of society, such as socialist dictatorships or some African regimes, but they fall outside the scope of our model' (Acemoglu and Robinson, 2001)

closely inspired by a 'Marxian' focus on conflict over class lines should ignore socialist and communist regimes. The authors acknowledge this gap in the conclusions (p. 357) and suggest this as a promising area for future research.

The relationship between economic structure and democracy is covered in Chapter 9, which contains an interesting discussion of how industrialisation and urbanisation may have made repression more costly and democracy less costly for the elites, hence spurring democratisation and redistribution.<sup>6</sup> Perhaps the chapter could have included a more extensive discussion of alternative theories – for example, the hypothesis that industrialisation might have led to more education and less inequality as the optimal choice of capitalists who directly benefit from the accumulation of human capital by the masses (Galor and Moav, 2006). Future investigations may provide 'horse races' among competing theories to assess their relative empirical relevance.

A key open question in the book is: *Why* are democratic institutions more persistent – and hence more credible – than mere 'policy promises'? What is special about giving *de jure* power to the citizens through democratic institutions? In their formal models, the authors deal with this issue through an extreme application of Occam's razor: by assumption. That is, they *assume* that the introduction of democratic institutions will change the distribution of power.<sup>7</sup> This is an acceptable simplification at this level of abstraction, but it sidesteps the important issue of how institutional change (namely, democratisation) actually affects state variables that determine the distribution of power in the future. Through what mechanisms and under what conditions does *de jure* democratisation actually work? After all, history is full of 'paper constitutions' with little or no effects. For example, as the authors recall (pp. 140–2), after the Potemkin mutiny in 1905 and other major social unrest in Russia, Tsar Nicholas II granted freedom of speech and association, and established that no new laws could be introduced without the agreement of an elected body (the Duma). However, the Tsar repealed most of those reforms later on (with sad long-term consequences for himself and his family). The authors argue that the ineffectiveness of these Tsarist reforms were due to the fact that Nicholas II did not go far enough in extending *de jure* power to his citizens. But how would more or less *de jure* power map into actual *de facto* power? How would the effectiveness of *de jure* democratisation depend on the willingness of people with *de facto* power (the military, the police, the bureaucracy) to obey elected officials and follow democratic procedures? Why do people with *de facto* power (say, the military) accept to obey civilian rule in some circumstances but not in others? Does a full-fledged analysis of these concepts require a still-to-be-developed economic theory of 'legitimacy'? Acemoglu and Robinson informally discuss some of these issues in their book but an explicit formalisation is left for further research.

Another topic that may deserve future attention is the study of the diffusion of democratic institutions across different countries and societies. Acemoglu and Robinson's approach focuses on domestic conditions within individual countries, but it has

<sup>6</sup> In particular, the redistributive costs of democracy may be larger for landowners than for the owners of mobile capital, as also stressed in Boix (2003).

<sup>7</sup> In a recent extension of the framework, Acemoglu and Robinson (2006) consider the possibility that increases in the citizens' *de jure* power be offset by additional investment in *de facto* power by the elites, therefore leaving the relative balance of power unchanged, or even more tilted towards the elites. However, this mechanism still assumes that, *ceteris paribus*, democratisation increases the power of the citizens.

often been noticed that democratisations tend to proceed in ‘waves’ (Huntington, 1991). In chapter 10 the authors consider the joint effects of globalisation forces as a possible explanation for the simultaneous adoption of democracy in several societies.<sup>8</sup> Alternative channels could depend directly on the forces affecting the diffusion of democratic institutions themselves, as ‘institutional innovations’ that are imitated, with changes and adaptations, across different societies and cultures.

In conclusion, I should point out, once again, that my remarks are ‘Monday morning quarterbacking’ – and from a fan who, at the end of the day, is happy that his team has scored and won the game. Overall, this book is very successful at illustrating the intellectual benefits from approaching complex issues with simplified but insightful analytical models. And a major benefit from this approach is, indeed, that it stimulates so many questions! I strongly recommend this fascinating and ambitious book, and expect that it will pave the way for further advances in the growing field of political economy.

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Daron Acemoglu and James Robinson’s magisterial book *Economic Origins of Dictatorship and Democracy* forcefully makes the case for using economic theory to understand democratisation. The book fuses two different strands and styles of research. Their use of formal models to understand democratisation has its antecedents in the positive

<sup>8</sup> A different set of international mechanisms linking the process of democratisation across countries may emerge under the alternative hypothesis, briefly mentioned in the book, that democracy is introduced to increase governments’ ability to carry external wars (Ticchi and Vindigni, 2003).