

Scott Mainwaring and Aníbal S. Pérez-Liñán. (2013). *Democracies and Dictatorships in Latin America: Emergence, Survival, and Fall*. Cambridge: Cambridge University Press, 368 pp.

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Recensiones

CIENCIA
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Why do some countries democratize and others do not? Why do some democracies become long-lasting and others quickly reverse to authoritarianism? In the other pole, why are some authoritarian regimes stable while others collapse? These are classical questions in comparative politics and we have managed to answer them in diverse manners in the last decades. And yet somehow we seem to be far from a dominant paradigm, or a fairly undisputed theory of regime change. In *Democracies and Dictatorships in Latin America: Emergence, Survival, and Fall*, Mainwaring and Pérez-Liñán (2013) offer yet another answer to these core questions. Therefore, the natural judgment for readers to make is whether their theory is in any fashion superior to previous ones. Mainwaring and Pérez-Liñán make a valid point about the limitations of the theories of democratization so far, including contemporary and often celebrated economic accounts such as those of Acemoglu and Robinson (2005) and Boix (2003). They correctly assess the need for broader theoretical frameworks and of more grounded and agent based measurement. Nevertheless, they run into measurement and design problems, which account for weak evidence against well-established theories. Regarding theory alone, the book highlights the causal role of normative preferences and external pressures in processes of regime change. While these are valid points, the authors do not offer strong theoretical arguments about who's normative preferences should matter and why.

The focus on Latin America makes sense. Something happened in the region for countries to abandon authoritarianism (hegemonic by the early 1900s) and embrace democracy. Moreover, democracies became fairly stable in the region, previously characterized by instability and a constant back and forward of regimes types. As the authors argue, something took place in Latin America than explains how democracy becomes the only game in town. According to Mainwaring and Pérez-Liñán, there is a great deal of learning during authoritarianism and political actors often change their positions and develop strong preferences for democracy. Normative predisposition of political actors change according to past traumas and examples coming from abroad. In my view, the notion that political actors learn throughout the process, thus changing strategies and beliefs, is a powerful one.

In this review, I will first address the book's theory and then its research design. As will be argued, the natural antagonists of Mainwaring and Pérez-Liñán's theory are the economic accounts of regime change, which so far have the blessings of many in

the field. The book also invests against modernization theory and, implicitly, against Przeworski *et al.* (2000), by now a classical account of regime change. Nevertheless, the authors miss good opportunities to empirically defend their own theory against these and others in the literature. As I will argue, the main problem regarding research design is the option for a correlational and observational study, which gives us limited evidence on causation.

The most popular models of regime change today overemphasize the role of maximization strategies, assuming well-informed actors motivated solely by material interests. Against that, Mainwaring and Pérez-Liñán argue that political actors often value the procedure as much, or even more so, than goals. This means that normative preferences play a significant role in the processes of regime change. Also, the authors are sensitive to a wide range of motivations that lead political actors to embrace democracy or authoritarianism. In that sense, the book tries to be loyal to the complexity of the phenomenon under study.

THE THEORY

Mainwaring and Pérez-Liñán's theory of democratization for Latin America is based on three levels of motivation for political action: (i) policy preferences, (ii) normative preferences and (iii) international environment. The first and last levels are contemplated in our current understanding of democratization, i.e. democracy is frequently attributed to distributional preferences (Acemoglu and Robinson 2005, Boix 2003) and to transnational phenomena (think of Huntington's waves, for instance. Huntington 1993). Therefore, the book mostly focuses on the second causal dimension: normative preferences. But let's not forget that intersubjective causes of regimes are also not new in comparative politics, they are just not fashionable these days, and one could certainly question the merits of their abandonment. Inglehart and Welzel (2005) would argue that it is all about values for instance. Others theorists of democracy, such as Diamond (2008) also highlight the relevance of values and loyalties to democracy.

That aside, the authors argue that strong normative preferences for democracy or autocracy can lead political actors to support regimes, even if this means to jeopardize their immediate material interest. Political actors, they say, often value "procedures" as much as they value ends. If supplemented with solid evidence, this argument would be a knockout against current economic models of regime change, in which political actors act solely based in the maximization of resources, whatever their nature. I will soon discuss whether evidence is sufficient or not.

Without making much use of the term "elites", the authors emulate elite theory by claiming that a theory of regime change should focus on the actors capable of influencing political outcomes. Such actors then cluster into political coalitions, both pro and against established regimes. The odds of regime change are therefore linked to the strength of such coalitions. A regime is likely to fall if (i) new political actors emerge and join an opposition coalition, (ii) the distribution of power switches in favor of the opposition coalition, and (iii) sufficient political actors switch sides. As said, whether actors will

remain on a coalition or switch to the rival one will depend on both policy and normative preferences.

The causes of policy preferences are well established in the economic accounts of democratization, but what about normative preferences? Here Mainwaring and Pérez-Liñán highlight the role of international pressures, incentives and examples (e.g. US sanctions, international aid, nearby transitions or breakdowns). External events are especially important because they offer evidence to political actors about the feasibility of regime change. Traumatic episodes can also change normative preferences. Mainwaring and Pérez-Liñán explore this in their two case studies, Argentina and El Salvador, offering insights for why regimes become stable in adverse conditions. They argue that the traumas related to previous authoritarian rule account for the normative attachment to democracy in both cases. Again, the idea that political actors learn and change positions throughout the process is powerful, and probably the highpoint of their theory.

In sum, at the macro-level (using their own terminology) regimes survive or fall as the outcome of competing coalitions. In the micro-level, those coalitions are supported by individual and collective actors perusing their policy and normative preferences. All under strong external influence, coming from both interested actors and compelling experiences.

THE EVIDENCE

As seen, the theory proposed is quite ambitious. It accounts for influential actors –which could be just about any person or organization–, the coalitions they form and external pressures, which come in several forms and shapes. In theoretical terms, the authors privilege complexity, which creates challenges for their research design. Their strong reliance on history and context-specific combination of such factors suggests a more classical small and comparative design, such as comparative historical analysis. Their causal claims suggest a design closer to experimental. What they offer is neither one or the other, nor a combination or both. Instead, they opt for a correlational framework.

Mainwaring and Pérez-Liñán build a large year-country dataset (from 1900 to 2010) in which they code political regimes, their main political actors, as well as the policy and normative preferences of such actors. All informed by the historiographic and political science literature, revised by a team of coders. One neglected problem is that historians rely a lot on official documents and archives, which are produced by organizations. Therefore, it is not surprising that formal organizations (the church, the army, and so on) appear as relevant actors, as well as the president for instance. Key actors in other theoretical accounts, such as the middle classes, state bureaucrats, the poor and the rich, are not taken into account. Their exclusion of the analysis is not due to theory, but due to the limitations of their data and coding procedure. In fact, their theory is open regarding the relevant political actors in each case.

One of the book's ambitions is to offer evidence against well-established theories. As the authors correctly assert, the most popular theories of regime change rely mostly on

structural variables (e.g. development, inequality, resource dependence, and so on). The authors constantly claim that their models offer evidence against structural variables, but this is not entirely true. For instance, they include per capita GDP and Inequality (measured by Gini coefficients) in their models and are glad that such variables do not show “statistically significant” results. Concordantly, the authors are glad to find that their coded “normative preferences” are statistically significant.

Now let’s see why this is not the best they could do. First, there are not many reasons to be concerned with P values in a non-randomized dataset. P values, thus statistical significance, give us information about what is likely to occur if we were to repeat an experiment, or remake a random selection of cases. Since their cases are not randomly selected or part of an experimental design, P values are meaningless. Moreover, the authors base their argument against “structural variables” by running a model where “normative regime preference” is the outcome variable, and not likelihood of regime change. Modernization theory and economic accounts of regime change do not claim that structural variables cause regime preferences, they claim that they cause regimes. The announced evidence against such models is, therefore, not compelling to engaged readers.

WHY THE BOOK SHOULD BE READ

I have argued that Mainwaring and Pérez-Liñán’s theory of democratization for Latin America poses problems, and yet I also mentioned that it is worth reading. Why? As the authors argue, we are now paying tribute to economic theories of democratization which build on unrealistic assumptions, and offer near-to-naïf causal mechanisms. The most popular models of regime change assume that political actors act consistently with their distributional expectations, which are always well-informed and well-served by either authoritarianism or democracy. Moreover, they assume that causes of regime change are constant in time. Mainwaring and Pérez-Liñán are correct in highlighting the need of theoretical models that account for the complexity of real choices made by political actors in quite different historical contexts.

Furthermore, they have the merit of not discrediting the variables proposed by the mentioned economic accounts of democratization. Instead of disputing causal arguments from other theories, the authors highlight how they might interrelate. This simple move is already an improvement in our current understanding of democratization. The problem is that the mechanisms that connect such different causal arguments are often unclear throughout the book.

It is also not clear, for instance, why this is a theory of regime change for Latin America, and not a theory for late developers or a general theory for that manner. There are no derivations or premises in the theory that imply a geographical limitation of scope to Latin America. The authors do start by claiming that many of current theories of regime change don’t fit in Latin America, but they also do not fit elsewhere. Mainwaring and Pérez-Liñán have a good sense for why that is: such theories are much likely to be wrong. They are not wrong for Latin America, they are just wrong.

The end message of the book goes in that direction. In it, Mainwaring and Pérez-Liñán revisit modernization theory, political culture theories and economic theories of regime change (which they oddly call “class theories”) and make good arguments about why we should move in a different direction. Other recent publications have also made the effort of including a more historically oriented, but quantitative account of regime change, such as Ansell and Samuels’ recent book (2014). In a way, these new scholarship moves away from “pure” economic modeling and introduces a more sociological account of regime change, relying on complex elite networks and on context-specific incentives for political actors. In my view, this turn in the field is desirable, necessary and feasible. It is a means toward better theory, but one with greater measurement challenges.

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