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[Book review] Guillermo Trejo: Popular Movements in Autocracies. Religion, Repression, and Indigenous Collective Action in Mexico

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Popular Movements in Autocracies. Religion, Repression, and Indigenous Collective Action in Mexico.

By Guillermo Trejo. New York: Cambridge University Press, 2012. 334p. \$109.99 cloth, \$27.99 paper. doi:10.1017/S1537592715001036

— Johannes Gerschewski, *Berlin Social Science Center (WZB)*

Guillermo Trejo's book is a valuable and important contribution to the studies of protest and democratization. Moreover, it is timely. Given the multiplication of ex post and ad hoc explanations for the uprisings during the so-called Arab Spring, Trejo's work is a theoretically sound and empirically abundant book that could "ripen" and develop over the years. It also points our attention to an often neglected group in the scholarly discourse on social movements and democratizations: the indigenous poor.

Trejo poses three central questions: 1) Why—despite similar conditions—do some rural villagers take collective action and protest while others are too apathetic to go to the streets? 2) How can we explain that some of these protests turn violent while others remain peaceful? And 3) why does the ethnicity of some groups sometimes become politicized while this remains only of secondary importance for other groups? His book is clearly structured along these three core questions. He states his research puzzle clearly, proposes a theory of collective action in autocracies and then subsequently deals with the emergence of protest, its nature and transformation to rebellion, and lastly the politicization of ethnicity.

The book stands out in terms of both its theoretical innovation and its empirical wealth. Beginning with the latter, the sheer amount of data that Trejo collected is truly impressive. Not only has he compiled a data set on protest activity that covers the period between 1970 and 2000 throughout the whole Mexican territory, but he also complements this quantitative data with ethnographic evidence and in-depth interviews with major actors, as well as with case studies. Methodologically, his study relies on large-n regressions, natural experiments, and life histories, which make his work almost a textbook case for carefully combining different methods in crafting causal inference.

Trejo masterfully combines insights from the social movement literature and democratization research. In contrast to the widespread assumption that neoliberal reforms were the main driver for protest, he argues that the breakup of the religious and political monopolies have provided Mexico with the organizational structure that was necessary for protest. The "competition for souls" when the Catholic Church was being challenged by U.S. Protestant missionaries and the "competition for votes" (pp. 7–8) when (semicompetitive) elections were being introduced

opened space for protest. While it might already be an innovative approach to analyze the twin erosions in the religious and political arena, the explanatory route Trejo takes is remarkable. He establishes an interesting connection to network theory and argues that the differences in the emerging religious and political structure account for both protest and the way in which protest is undertaken. While the Catholic Church response to its Protestant rivals resulted in decentralized regional networks with strong leading figures who are embedded in strong interregional associations, the admission of leftist opposition parties led to centralized associational networks with strong leaders but weak ties in between. This difference in the structural composition of the emerging networks sheds new light on one of the classic questions of comparative autocracy research: When are carrots (co-optation) and sticks (repression) successful? Trejo argues powerfully that these instruments were more likely to be successful when they faced centralized structures—as in the case of leftist unions—and have been more modest in demobilizing protest for decentralized networks—as in the case of religious actors. This is a highly interesting finding that future research should take up.

The nature of the networks serves also for the explanation of why some of the protest turned to rebellion. Trejo argues that the decentralized horizontal networks that have been created by the Catholic Church have been the primary reservoir for recruitment of rebels—in contrast to the political niches of the leftist opposition and its rather centralized structures. Moreover, he demonstrates that the politicization of ethnicity was also driven by religious competition. In areas where the Protestants have challenged Catholic dominance the most, the Catholic Church has turned out to be a longtime promoter of ethnic identities in order "to keep indigenous souls in the Catholic fold" (p. 229). And it was this politicized ethnic group, the Zapatista, that played a major role in Mexico's democratization. Newer research on East Asia by Dan Slater and Joseph Wong emphasizes the role of strong states and their reactions to signals (see "The Strength to Concede: Ruling Parties and Democratization in Developmental Asia," *Perspectives on Politics* 11 [no. 3, 2013]: 717–33), and Trejo highlights anticipation as well. Yet while the Mexican state seems to be strong, too, he argues that the Mexican government in fact feared the rise of a revolutionary coalition and made political concessions to the leftist opposition in order to absorb this (violent) movement. Paralleling the threat of workers to authoritarian elites in Europe's nineteenth century, Trejo underlines the importance of the indigenous social movement that drove the government's decision to introduce fair elections.

I would nonetheless offer two minor criticisms, one theoretical and one conceptual. Firstly, Trejo argues that his theory of collective action can be generalized. I do not doubt that it might be able to travel. However, the nexus

between religious and political competition that manifests in different network structures is observable in the Mexican case but needs to be proven in other contexts. Yet his claim to have a closer look at the emerging networks when explaining protest and the resilience of autocratic rule remains in force. Can we theoretically assume that a certain network structure emerges in a specific intergroup competition? Or are these interactions too complex and ultimately idiosyncratic? Furthermore, his theory needs to remain silent for explaining protest under monopolistic rule. Only when the autocratic regime is already “shaken” and on the path toward democracy—in other words, if at least some competition is allowed—does the theory find a grip. In this light, the theory applies only to the autocracies with a softening adjective. It would also have been helpful for generalization purposes if the author had stated more explicitly what Mexico’s case represents. Secondly, his measure of competition seems too vague

to me. Given the concept’s weight in his explanation, a more complex measurement of competition would have been desirable. Trejo measures competition by the mere effective number of religious groups or political parties (pp. 72–74). At least spatial approaches like ideological distance might have contributed to a more valid measurement.

To sum up, *Popular Movements in Autocracies* is an admirable work. Trejo’s methodical triangulation, his theoretical approach, and his collecting of empirical data are laudable. This well-written book offers many new empirical insights into the nature of indigenous mobilization. The theoretical synthesis of social movement and democratization literature is convincing, although further work would be needed to make it applicable beyond the studied case. I am hopeful that future studies will follow Trejo’s lead, and analyze emergent network structures to explain protest and rebellion, for this is a most promising line of inquiry.
