Chapters 5 and 6 investigate more deeply two of the core observations made in chapter 4: the decline in the number of coups over time and the correlation between term limits and democratic stability. Chapter 5 explores coups as a mechanism of leadership change, demonstrating both geographical and temporal variation in their occurrence. The authors explore what exactly these events are, why they seem to be more prevalent in West and Central Africa and before 1990, and what their effects have been. Chapter 6 does the same, but in relation to term limits.

In the second broad set of analyses, the authors turn to an investigation of the effects of leadership transitions on economic growth (chapter 7) and social welfare (chapter 8), as well as the effects of different types of leaders—identified in their dataset as transients, autocrats, hegemons, and democrats—on economic growth, social welfare, and democratic durability (chapter 9). The analyses here are typically time-series cross-sectional regressions with a variety of theoretically motivated control variables. In general, the authors find evidence that political leadership matters and that leaders who hold power based on winning a multiparty election are better at providing for their people. These insights may shed light on extant debates about the effect of democratization events on social and economic welfare on the continent.

Critics will likely note that the central strength of the book—its breadth—is also its primary weakness. Contra Harding's and Klaus's books, which have more specific geographic reach, Carbone and Pellegata study 60 years of history for each of 49 African states. As such, despite their admirable attention to historical detail, their theoretical claims and measurement strategies are almost by definition more systematic and general, and thus easier to find exceptions to. Likewise, their empirical tests are low resolution, with the unit of analysis being the country-year. The methodological difficulties of such cross-national statistical analyses are well known and will certainly cause some skeptics to question the strength of the findings presented in chapters 7–9. Yet, even if one views the tests of cause and effect in a skeptical light, the descriptive contributions certainly should not be missed.

Votes, Drugs, and Violence: The Political Logic of Criminal Wars in Mexico. By Guillermo Trejo and Sandra Ley. New York: Cambridge University Press, 2020. 349p. $105.00 cloth, $34.99 paper.

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Between 1990 and 2020, more than a half-million Mexicans were murdered. Of these killings, over 300,000 occurred since 2006 when the federal government, with support from governors and civil society, increased the participation of the armed forces in attempting to weaken and limit the activities of criminal organizations. Today, thousands of families search for their missing loved ones, who by official statistics number around 80,000—but the discoveries of mass graves across the country suggest the number of murdered Mexicans is much higher. The most recent victimization survey shows that 68.2% percent of Mexicans consider insecurity to be the main problem for the country. *Votes, Drugs, and Violence* is an important contribution to explaining why and how Mexico has become one of the most lethal countries in a region that has the highest levels of homicide rates in the world.

In its ambitious agenda, this book by Guillermo Trejo and Sandra Ley seeks to explain three interconnected phenomena: why cartels went to war as the country made the transition from authoritarian rule to multiparty democracy in the 1990s; why drug violence escalated after the Mexican federal government declared war on the cartels in the 2000s; and why, after two decades of inter-cartel and state–cartel wars, drug lords became political-territorial actors who developed subnational criminal governance regimes.

Important scholars in the emerging literature on the political foundations of crime and violence, Trejo and Ley advance our understanding of the links between organized crime and politics by showing that uncertainty generated through electoral competition or the presence of opposition parties cannot solely account for the onset and escalation of violence. Rather, we must consider the protection that criminal enterprises require to exist. This protection is not guaranteed and in fact can break down with "the actual *rotation of parties* in state gubernatorial power and the *removal* of top- and mid-level officials from the state attorney's office and the state judicial police" (p. 111; emphasis in original). This finding not only brings the state back in but also encourages us to carefully consider which actors within subnational politics are critical in enabling criminal operations to exist and survive. By extension, Trejo and Ley contribute to and build on existing research emphasizing subnational politics as a more appropriate unit of analysis for scholars focused on crime and violence.

In recent years, research on organized crime outside the field of criminology has produced or expanded important concepts, such as criminal governance, state-sponsored protection rackets, and violent specialists. Trejo and Ley also make an important theoretical contribution by introducing the concept of the "gray zone of criminality" defined as "the ecosystem in which criminals and state agents informally coexist and in which Organized Criminal Groups (OCGs) live, grow, and reproduce" (p. 40). As the name suggests, the gray zone is not defined by the existence or absence of a finite action, such as paying off a public official, but rather by the ongoing complex relationships between state and criminal actors. In this sense, the concept expands our understanding beyond notions of corruption, co-optation, and coercion that are often used to explain the interaction between state actors and criminal groups. The gray zone of criminality opens up the black box of informal practices and sets the table for refined systematic analysis. Furthermore, although the concept was generated in the context of the study of Mexico, it is not hard to imagine its application to other countries with high levels of violence and weak democratic rule.

From an empirical standpoint, purists of ethnographic work may take issue with the quantitative tests used by Trejo and Ley. Choosing Michoacán and Guerrero for their synthetic control models is a valiant choice considering the uniquely bellicose pasts of these states. However, even if one is skeptical of the explanatory power of such methods, the authors have undeniably made a conscious effort to engage with complex phenomena through an innovative mixed-methods strategy incorporating multivariate regression models, quasi-experimental techniques, process tracing, and interviews with high-level government officials. More importantly, even with the combination of these methodologies that seek to establish causal identification, the authors are not claiming a monocausal explanation of criminal wars but rather advocate for the "development of a theoretical and empirical corpus that will lead to the systematic testing of the likely impact of political regimes, political change, and electoral politics on large-scale criminal violence" (p. 292).

The robustness of the argument, however, is constrained by Trejo and Ley's explanation of the evolution and fragmentation of drug trafficking organizations. Although they encourage us to think about the intricacies of state actors operating at the subnational level, they treat drug trafficking organizations as unitary, homogeneous actors pursuing identical goals. This is problematic because it overemphasizes rationality in decision-making processes within criminal groups and neglects explanations for violence related to principal–agent problems. That is, sometimes the violence we observe is not the result of a calculated decision by the criminal group but rather the result of shirking. As fragmentation has taken place in Mexico's criminal landscape, we cannot discount this as an outlier explanation for violence.

Furthermore, in using a narrative that emphasizes the leadership of one criminal group (the Guadalajara Cartel) that purportedly created three new cartels in the late 1980s (Tijuana, Juárez, and Sinaloa) as their starting point for subsequent turf wars, the authors assume that all drug trafficking groups want and can expand. Rather than focusing on the alleged *pax mafiosa*, the argument would be improved by considering the international and national developments within drug markets that created different incentives for the use of violence during the period of study.

For example, although they take into account the demise of the Caribbean route and the subsequent change of drug routes to Mexico, the authors do not mention the opium production ban imposed on Turkey in the early 1970s that was crucial for enabling traffickers in Mexico to supply heroin to US consumers. Given Sinaloa's longstanding ties with the United States, the ban also made that state a key international player in the illicit drug markets, in contrast to other opium-growing regions in Mexico. Similarly, although the authors briefly mention changes in consumer preferences, the increased demand for synthetic drugs in the mid-1990s allowed groups to manufacture illicit drugs locally without requiring partners in production centers as was necessary with the importation of cocaine. In line with their argument, this change would translate into different gray zones of criminality being contingent on different production models.

Beyond drug markets, the authors fail to consider that some criminal groups may diversify their criminal activities not because they are expanding drug trafficking routes but because they are moving to a different business model. In this sense, violence against civilians is not a result of turf wars, but occurs because violence needs to be public for extortion threats to be credible. This does not refute the authors' argument of criminal groups as political actors but adds nuance to the idea that violence is bad for business.

My assessment of the blind spots of the book as they relate to the business models of criminal groups does not take away from its substantial contributions. In the preface, Trejo and Ley declare that "during the long years of research and writing of this book we have always kept in mind the hundreds of thousands of civilians and their families who have become victims of criminal wars." Ultimately, beyond academic debates, the arguments and findings of this book remind us that, when it comes to violence and criminality, carefully constructed research is not exclusively an intellectual pursuit but one of the building blocks for more peaceful societies.

Trust and the Islamic Advantage: Religious-Based Movements in Turkey and the Muslim World. By Avital Livny. New York: Cambridge University Press, 2020. $99.99 cloth.
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The Islamic political revival in the Muslim world in the last several decades has been a central topic of inquiry in the social sciences. Even if their electoral performances and ability to capture power exhibit significant variation, Islamist movements continue to be the most formidable and resilient political forces in many countries. Avital Livny's *Trust and Islamic Advantage* argues that Islamists do have a competitive advantage when it comes to mobilizing support and inducing individuals to engage in both political and economic collective action. That advantage