

## Three Outcomes of Contentious Elections<sup>1</sup>

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**Abstract:** Elections are contentious by design, but contentious election outcomes—from democratic backsliding to widespread protests and violence—occur regularly and represent significant challenges to both domestic and international stability. This review essay explores three specific outcomes (failed democratization, democracy protests, and election violence) using three recent, influential books on these topics. Several overlapping causal mechanisms are highlighted including those focusing on structure characteristics, actor behavior, and election cycle dynamics. Four cross-cutting themes (democratization, electoral history, structural constraints, and money in politics) are explored as are three areas (overlooked actors, election integrity, and international factors) for future research.

**Key words:** democratization, protest, election violence

### Books Reviewed

Dawn Brancati. *Democracy Protests: Origins, Features, and Significance*. New York: Cambridge University Press, 2016. 228 pp., \$28 paperback (ISBN: 9781316502754).

Jonas Claes, editor. *Electing Peace: Violence Prevention and Impact at the Polls*. Washington, DC: United States Institute of Peace Press, 2016. 241 pp., \$25 paperback (ISBN: 9781601275226).

Thomas E. Flores and Irfan Nooruddin. *Elections in Hard Times: Building Stronger Democracies in the 21<sup>st</sup> Century*. New York and Washington, DC: Cambridge University Press and Woodrow Wilson Center Press, 2016. 298 pp., \$30 paperback (ISBN: 9781107584631)

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<sup>1</sup> This article benefited from the constructive comments and suggestions of the editors and four anonymous reviewers. All remaining errors are my own.

National elections are the crossroads of democracy. Party competition, individual ambition, and policymaking intersect in an almost universally adopted political process, one that provides a unique legitimacy to the electoral victors. Public skepticism about elections' ability to effectively manage domestic conflict, however, is rising, and the fruits of electoral dissatisfaction—from democratic backsliding (e.g., Egypt in 2012) to public protests for greater democratic representation (e.g., Kyrgyzstan in 2005) to widespread political violence (e.g., Côte d'Ivoire in 2010)—have grown more frequent and destabilizing. Furthermore, a third autocratic wave is now underway (Lührmann and Lindberg 2019) that threatens the ubiquity of free-and-fair elections, a cornerstone of both minimal (Przeworski 1999; Cheibub et al. 2010) and maximalist (Gurr et al. 1990; Coppedge et al. 2020) definitions of democracy. The subsequent contested and contentious elections regularly lead to public dissatisfaction, disaffection, and violence (Norris et al. 2015).

This essay reviews three significant, recent books that explore three different contentious electoral outcomes, each the subject of their own research subfield—failed democratization (Flores and Nooruddin 2016), democracy protests (Brancati 2016), and election violence (Claes 2016). Each of these outcomes is important because it affects the perceived legitimacy of the electoral process in emerging democracies as well as the parties and leaders who take power. This essay reviews each contentious outcome and its associated research before highlighting overlapping causal mechanisms, cross-cutting themes, and gaps in existing research. What is to be gained by comparing works across contentious election outcomes (and indeed subfields)? I see three main benefits. First, each outcome's literature (typified by these three books) have infrequently engaged with the broader contentious politics literature (e.g., Tilly and Tarrow 2006; della Porta and Mattoni 2014) or used its generalizable theoretical frameworks. Second, significant overlapping but underexplored mechanisms drive these three outcomes. Mapping the underlying overlapping causal processes enables both theoretical growth (e.g., through

better understanding the substitutability of these outcomes) and crucial theoretical crosspollination (i.e., seeing if a mechanism behind one outcome and applies to another). Third, the democratic institutional design literature has, to date, generally overlooked variation in behavioral outcomes, while the protest and violence literatures have underexplored the broader contextual and institutional landscape. Moreover, a generation of literature advocating various policies to policymakers, donors, and the international community has advanced elections for peacebuilding and democratic consolidation (e.g., Lyons 2004), but the evidence is mixed (Brancati and Snyder 2013). Therefore, to really understand democracy's macro-level effects, and the variation in specific outcomes, this review essay highlights linkages across contentious election outcomes. Doing so gives us not only a clearer view of variations in outcomes but also a framework for understanding the causal processes and mechanisms that lead to those outcomes.<sup>1</sup> This promises to better inform the sorts of international and domestic policymaking that international studies scholars care about, including how to build peace in conflict-prone (Dunning 2011) or post-conflict settings (Walter 2002; Matanock 2017).

Why these three books? First, these books make significant contributions to their respective literatures and the broader comparative and international relations (IR) research on elections. There are, of course, other relevant books; however, because it is not possible to review all current works here, I chose a sub-sample of influential books with innovative arguments and research designs. Second, these books are comparable to one another, given their topical focus on contentious election outcomes, their theoretical focus on understanding recent political instability, and most importantly, their broader focus on international efforts to shape these outcomes. Finally, they address some of the most pressing questions in the field today. Why do some elections in developing countries not lead to democracy, and how can international

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<sup>1</sup> I thank an anonymous reviewer for helpful suggestions with framing here.

democracy promotion be more effective?<sup>2</sup> What drives democracy protests, and how effective are they at causing change? How can elections lead to violence, and what can be done to prevent it?

In the next section, I briefly describe several important aspects of the contentious elections research. In doing so, I group the underlying causal mechanisms into three main levels of analysis: background structural conditions (Level 1), elite actor behavior (Level 2), and the triggering of election cycle dynamics (Level 3). I then discuss how elections can (but often do not) lead to peaceful transfers of power and the strengthening of democratic institutions in developing states. Next, I explore how the electoral process and democratization pressure can come from both organized and spontaneous protests. At the extreme, this connects the study of contentious election outcomes to the use of violence. Finally, I highlight four cross-cutting themes (i.e., democratization, electoral history, structure constraints, and money in politics) and three areas for future research (i.e., overlooked actors, election integrity, and international dynamics).

### **Contentious election outcomes**

Contentious elections research, broadly defined, crosses several subfield boundaries. While different research agendas necessarily focus on a particular political outcome (e.g.,

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<sup>2</sup> While some of these causal mechanisms may also apply to developed countries—e.g., Europe and North America have seen democratic backsliding and protests in the last decade (Freedom House 2019)—I focus on developing, or emerging, democracies since the end of the Cold War in this study. Such a focus enables me to map recent shifts in the study of contentious election outcomes and their effects on governance more clearly.

democratization, protest, or violence), this research develops (implicitly or explicitly) from the contentious politics literature and involves similar political actors—masses and the elites—operating within similar structural constraints (Tilly and Tarrow 2006). Election outcomes shaped by these actors’ interactions range from stability-reinforcing transfers of power in consolidated democracies to complete institutional failure and war. One way to understand these outcomes is to see whether and how they affect a country’s political institutions (e.g., democratization or autocratization) or lead to specific types of events (e.g., protests and violence). Thus, this essay focuses on three interrelated, contentious election outcomes: failed democratization, pro-democracy protests, and election violence. Other election outcomes are, of course, substantively important, but the books I focus on in this review contribute to these three overlapping and rapidly growing literatures. Figure 1 summarizes publication growth in these areas from 2000 to 2019, both in absolute numbers, as well as relative to the amount of research produced in 2000.<sup>3</sup> It highlights how the work on contentious politics and contentious elections has grown significantly in the last decade. Put simply, the three outcomes discussed in this essay represent areas of enduring or growing research interest around elections.

[Figure 1 about here]

At the most basic level, elections both result from—and reinforce—a country’s political institutions and represent a cornerstone of a government’s legitimacy. This legitimacy derives from the election’s integrity, as well as retrospective judgements on incumbents’ achievements, prospective judgements about election challengers, and the rules and norms of democratic governance (Flores and Nooruddin 2016). An election victory (even without a transfer of power) provides crucial legitimacy to new governments—what Flores and Nooruddin (2016)

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<sup>3</sup> The figure includes only published, English-language articles. It excludes working papers, books, and book chapters. Scopus search conducted on February 18, 2020.

call procedural legitimacy—which explains why even established autocracies hold elections (Levitsky and Way 2010). Elections confer on the winner “a legitimacy that is denied to those who acquire power by other means” (Flores and Nooruddin 2016, 81).

When explaining electoral outcomes, scholars often (implicitly or explicitly) adopt a similar approach, grouping their causal mechanisms into (1) background structural factors, (2) elite actor behavior, and (3) election-specific triggers. I consistently and comparably group causal stories across outcomes along these same lines.<sup>4</sup> Level 1 includes background structural factors (e.g., historical, geographic, and socioeconomic characteristics). These characteristics (from colonial history, economic capacity, land size, and terrain to population size and governance experience) are unlikely to vary by much from election to election. Level 2 includes actors’ characteristics and behavior. The outcomes discussed here involve many actors, including incumbent and opposition groups, security and election management personnel, and citizen groups. While some actors play roles in more than one election, their actions and impact on any particular election can vary. Level 3 includes specific election dynamics that vary over time, such as allegations of fraud, voter intimidation, vote tampering, and international actors’ interventions. Taken as a whole, these three levels of causality help explain contentious election outcomes, including the three of interest here.<sup>5</sup> Although these three levels rarely receive equal attention, detail, or weight when explaining contentious election outcomes, grouping

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<sup>4</sup> The books reviewed here (and much of the literature) largely overlooks the possibility of endogenous or reciprocal effects (Pierson 2004). That topic lies outside this review’s scope.

<sup>5</sup> Although discussed separately here, these levels interact in important ways. For instance, background conditions affect which actors become incumbents or how they govern (e.g., Achemoglu and Robinson 2006), as well as how elites shape election cycle dynamics (Birch 2012). Such considerations lie beyond the scope of this review.

explanations under these three headings helps clarify what we gain and/or overlook when focusing on explanations in any particular one heading. In the next four sections, I discuss each contentious election outcome in turn before highlighting their overlapping causal mechanisms and areas for future research.

### **Failed democratization**

During the last three decades of the 20th century, a democratic wave washed over formerly autocratic states. At the time, some scholars argued that these states were inexorably developing towards more democratic representation. Growing evidence, however, suggests the democratic wave is receding. Over the last decade, elections in widely differing contexts (e.g., Egypt, Hungary, Myanmar, Nicaragua, the Philippines, South Sudan, Thailand, and Venezuela) were followed by democratic backsliding, crackdowns on opposition parties, and violence. For example, Egypt democratized after widespread protests led to President Mubarak's 2011 departure and the multi-party election of a former Muslim Brotherhood member, Mohammad Morsi. A 2013 military coup then placed Army General Abdel Fattah el-Sisi in power, a clear (and, to date, enduring) ebbing of Egypt's democratic wave. The global democratic tide has also turned with growing public distrust of government institutions and representative democracy (Armingeon and Guthmann 2014). As of 2020, Freedom House has recorded fourteen consecutive years of declining global political freedom, even titling its 2019 annual report "Democracy in Retreat." A rapidly growing literature (e.g., Bermeo 2016; Bogaards 2018; Levitsky and Ziblatt 2018; Lührmann and Lindberg 2019) explores how this autocratization process (moving backwards) is distinct from (but related to) democratization

(moving forwards) or failed democratization (not moving forward/staying in place).<sup>6</sup> This review focuses on failed democratization—defined here as a period of (attempted or actual) political liberalization, followed by either a stagnation or stabilization of an anocratic political system.<sup>7</sup>

Flores and Nooruddin's (2016) *Elections in Hard Times* is part of a growing effort to explain why some countries fail to democratize. Their main (Level 1 structural) argument is that “electoral seeds fail to bear democratic fruit not because they are poor quality, but because of the inhospitable terrain in which they are sown” (Flores and Nooruddin 2016, xvii). This inhospitable terrain includes the structural challenges developing countries face (e.g., historical, economic, and institutional). Flores and Nooruddin (2016) outline three types of legitimacy, the lack of which contribute to failed democratization: a government's *contingent legitimacy*, derived from an election victory; a state's *performance legitimacy*, accumulating from past governments' achievements; and *democratic-institutional legitimacy*, resulting from the (in)formal rules and norms of democratic governance. Unlike other election- or governance-focused works, Flores and Nooruddin theoretically link elections to governing through elections' effects on performance legitimacy (i.e., how governments provide public goods). In contrast, Coppedge et al. (2020) reconceptualize and measure democracy, rather than explaining how it drives contentious election outcomes, while Haggard and Kaufman (2016)

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<sup>6</sup> These different outcomes have their own literatures (e.g., Bermeo 2016; Bogaards 2018).

Addressing the conceptual differences between them, along with their distinct or interrelated effects on the outcomes of interest, lies outside the scope of this review.

<sup>7</sup> This conceptual definition builds on Waldner and Lust's (2018) conceptual definition of democratic backsliding and Wilson et al.'s (2020) definition of democratization.

focus on the divergent democratic preferences of the elite and masses, rather than how these preferences manifest themselves in election outcomes.

Despite an elections-outcome link, Flores and Nooruddin (2016) largely sidestep Level 2 and 3 explanations. Instead, they focus on three specific, Level 1 structural factors that shape the likelihood that elections lead to democratization. The first factor is democratic experience. Like Lindberg (2006), Flores and Nooruddin argue that (even flawed) elections provide institutional experience and opportunities for elites and masses to reconcile themselves to election outcomes. As a result, protests, riots, and violence are less likely in elections within countries that have more democratic experience, and this experience increases voter satisfaction with democracy.

Next, countries fail to democratize due to a lack of economic resources. Governments need a substantial economic capacity to deliver on election campaign promises. Flores and Nooruddin (2016, 122) focus on the implications of the lack of economic resources, which they refer to as fiscal space: “[L]imited fiscal space harms democracy via its effect on the electoral strategies adapted by incumbent leaders.” Limited economic resources constrain leaders’ ability to undertake new projects or initiatives. This then shapes leaders’ election strategies. Incumbents resort to clientelism, ethnic appeals, and repression because it is difficult to make credible campaign promises. Empirically, Flores and Nooruddin (2016) find that democracies have greater tax revenues than non-democracies and argue that higher tax revenues contribute to better, observable governance indicators. This makes logical sense, and coincides with other recent research on the topic. Nevertheless, it is surprising that they do not discuss where the tax revenue comes from (e.g., taxing citizens or resource rents), despite rents’ clear negative effects on political (Dunning 2008) or economic (Auty 2001) development. They do, however, consider how international aid may create more fiscal space if the aid flows to the government.

Avoiding the government may help reduce corruption, but it also does not help develop bureaucratic capacity and performance legitimacy.

Flores and Nooruddin's (2016) third causal story centers on the lasting legacy of political violence. Elections in countries with a current (or recent) ethnic or secessionist conflict lead to a "negative democratic dividend"—decreased levels of democracy five years after the election. This is unsurprising, given that 80% of elections held during civil wars are not competitive. Like previous scholars, they find that the first two years after conflict are the most unstable, and they highlight individual-level surveys in Guatemala, which suggest continued physical insecurity from these conflicts lead to less satisfaction with democracy.

Complimenting these three domestic mechanisms (i.e., democratic experience, fiscal space, and conflict) are three international mechanisms: election observation, democracy aid, and peacekeeping missions. The authors argue that (1) pro-democracy aid and international monitors increase the democratic dividend of the first two multiparty elections, (2) election monitors enhance the election process' legitimacy, and (3) peacekeeping missions increase the chance of sustainable democratization when peacekeepers have an election monitoring mandate. Although the authors consider neither whether election monitor quality or experience matter, nor why incumbents allow specific monitoring groups (Daxecker and Schneider 2014), they recognize that interventions are not random; international actors intervene more often in hard cases, likely understating the peacekeepers' actual democratizing effect.

*Elections in Hard Times* contributes significantly to the democratization literature. It helps explain why some elections lead to lasting democratic change, while others do not; it links structural dynamics to election outcomes; and it explains when and how international assistance can help sustain democratic transitions. That said, six elements of this book are less convincing. First, its top-down, elite-driven view of democratization fails to differentiate between the motives and actions of political elites, on the one hand, and those of the masses, on the other

(see Brancati 2016 below). Second, it focuses more attention on Level 1 independent variables than the dependent variable, sidestepping a discussion of what democratization means conceptually or empirically in developing countries. Third, Flores and Nooruddin (2016) repeatedly highlight founding elections. It is unclear, however, the extent to which founding elections remain relevant today. Given recent Timorese and South Sudanese independence, their position is possible, but are recent founding elections theoretically comparable to those held during the Cold War decolonization process? Fourth, the authors focus on international policymakers' actions and interests, rather than the domestic actors who run elections, compete in them, or govern after them. Actors in developing states therefore do not have much agency in the causal story, despite the fact that a number of incumbents can call elections when the political and economic winds are favorable and policy promises more credible. Fifth, they do not focus on any specific democratic political institutions (e.g., the balance of power between government branches or minority protections), but instead on shifts within aggregate democracy measures (e.g., V-Dem's latent Unified Democracy Score). Aggregate democracy scores amalgamate a wide variety of formal institutions. The authors do not disaggregate these scores to see whether one particular institutional characteristic (e.g., executive selection or legislative constraints) drives the results. Moreover, latent measures do not tell us about the relations between branches of government, a crucial element of democratic development (Schedler et al. 1999). Sixth, they overlook other measures of governance (e.g., increased education spending, literacy, or health) that can also represent democratic governance or accountability. Overall, however, Flores and Nooruddin (2016) provide credible Level 1 evidence for why some developing states fail to democratize and suggest three ways that international actors can provide resources to help this process.

## **Pro-democracy protests**

Flores and Nooruddin (2016) suggest that democratization through institutional change can often be a top-down process. Nonetheless, recent history (and a substantive body of research) suggests bottom-up efforts also play a role—either through formal electoral processes or through citizens pressuring their government via public pro-democracy protest (e.g., Beaulieu 2014; Burchard 2015; Chernykh and Svolik 2015). For example, large postelection, pro-democracy protests in Kyrgyzstan led to the removal of two consecutive presidents: long-serving Askar Akayev (i.e., the 2005 Tulip Revolution) and Kurmanbek Bakiyev (2010). More broadly, a substantial literature on political protests has evolved from the social movements literature and describes both the motives behind collective democratic protests and their effects on political change (della Porta and Tarrow 2005; Tilly and Tarrow 2006). Public protests are geared to influence government action or public opinion; indeed, research suggests such civil resistance more effectively brings about policy change than violent resistance (Chenoweth and Stephan 2011). Protests frequently target governments and their policies, springing from triggering events like food prices (Bush and Martiniello 2017), trade exports (Munro 2015), war (McAdam and Su 2002), meetings of international organizations (Levi and Murphy 2006), and national elections (Beaulieu 2014). Post-election protests have removed leaders in the color revolutions—either through leaders stepping down (e.g., Kyrgyzstan 2005 or 2010; Little et al. 2015) or coups d’etat (e.g., Algeria 1992; Wig and Rød 2016). Protest research either focuses on the underlying causes for protests in particular countries—like Brazil (Recuero et al. 2015), India (Jenkins et al. 2014), or Russia (Lankina and Watanabe (2017)—or takes a cross-national approach (Beaulieu 2014).

Regardless of the cases studied, recent research indicates that protesting can be effective, but difficult to sustain (e.g., see Beaulieu’s 2014 work on electoral protest, boycotts, and reform, or Boulding’s 2014 work on non-governmental organizations’ involvement in

contentious political behavior). I highlight Brancati's (2016) *Democracy Protests* here because it concentrates on protests aimed at increasing democratic representation and institutions, thereby connecting it to both the democratization literature generally and to Flores and Nooruddin (2016) specifically. Brancati (2016) argues that democracy protests can encourage governments to make democratic concessions, but these concessions depend on several Level 1 and Level 3 factors. Her analysis suggests that the third wave of democracy has been accompanied by a wave of opposition-initiated, election-related protests. Like Flores and Nooruddin (2016), she uses a mixed-methods approach, which leads her to conclude that protests are more likely to help democratization than impede it—an important finding that contributes significantly to both the democratization and protest literatures.

This conclusion rests, in part, on the coding and analysis of 310 protests in 92 countries from 1989 to 2011. While other protest datasets exist, Brancati's (2016) data are collected to evaluate specific theoretical claims about democracy protests. Brancati (2016, 5) defines *democracy protests* as “mass public demonstrations in which the participants demand countries adopt or uphold democratic elections.” These demonstrations need to last for more than one day, and at least two protests—with the same organizers, targets, and demands—must occur within three months. Importantly, Brancati's (2016) definition includes not only anti-government protests, but also those against political institutions. The data prove necessary because broader datasets, like the National Elections Across Democracy and Autocracy (NELDA) dataset (Hyde and Marinov 2012) or those capturing electoral protests (Beaulieu 2014), do not specifically focus on pro-democracy protests.<sup>8</sup> In focusing on election-related events, Brancati (2016) embraces a minimal definition of democracy and excludes protests related to governance—a topic of keen interest to Flores and Nooruddin (2016).

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<sup>8</sup> Democracy protests frequently revolve around elections.

Brancati's (2016) dataset reveals several interesting trends. Notably, three-quarters of democracy protests relate to elections. Within these, allegations of electoral fraud motivate the most cases (43% of 310 cases), followed by a demand for open and competitive elections. Protest size also varied—an important consideration because larger protests threaten a government more than smaller protests. Nevertheless, smaller protests limited to capital cities capture the modal category. Finally, three-quarters of post-election protests occur after an incumbent wins an election, especially with narrow vote margins. Supporters of parties and candidates unhappy with an election result typically organize these protests, although non-governmental groups and diaspora groups coordinate a few others.

Like Flores and Nooruddin (2016), Brancati (2016) argues that economic factors help explain protests as a contentious election outcome. Brancati (2016), however, focuses on short-term economic change (Level 3) rather than economic capacity (Level 1). Economic crises make protests more likely because they encourage fraud, raise discontent with the (especially authoritarian) government, incentivize opposition candidates to organize protests, and help protesters overcome the collective action problem. They also shape governments' incentives to accommodate protests because incumbents are more likely to lose elections when the economy falters. For that to happen, though, the public must see the economic crisis and blame it on a lack of democracy—a tall order, given that voters seemingly care more about their economic situation (e.g., wages and housing), as opposed to democracy.

In operationalizing economic crises, Brancati (2016) uses three objective indicators (i.e., economic growth, inflation, and unemployment) and two subjective indicators (i.e., perceptions of standard of living and national economic conditions). Besides economic crises, protests originate via elections as well. This Level 3 triggering event has both direct effects on protests, as well as an interactive effect with economic crises; democracy protests are significantly more likely after an election in countries experiencing an economic crisis.

Governments respond to democracy protests strategically. They consider (non-violently and violently) repressing protests before accommodating them. Non-violent repression tactics include restrictions on public demonstrations, blocking media access and personal communications, and organizing counter-protests. Importantly, in four out of five cases when governments used non-violent repression, they also used violent repression. Violence is costly, as it requires more resources, but it can be more effective if it deters protest participation. Protestors, in turn, can also use violence. Indeed, almost half (45%) of democratic protests include at least some violence that harmed either people or property. Not only were these protests larger, which suggests that violence does not always deter participation, but protestor violence also precipitated repression three times more frequently than peaceful protests. If repression is ineffective, governments can then try (political, economic, or policy) accommodation. The most common concessions were increasing the openness and competitiveness of elections and reducing electoral malfeasance (e.g., holding election recounts, repeating elections, or sharing power).

Overall, Brancati (2016) paints a clear portrait of democracy protests and gathers new data to evaluate its causes, government responses to it, and its effectiveness. Her study is steeped in the narratives of particular protests, and specific case examples ground the comparative findings. The mechanisms put forward also help explain the contentious outcomes under consideration. Nevertheless, juxtaposing this study with Flores and Nooruddin (2016) highlights several significant limitations. Both books examine whether a state democratizes. Brancati (2016) finds that almost a third of protests led to democratization in the following year and concludes that democracy protests, in general, more likely help democracy than hurt it. The one-year timeframe, however, is markedly shorter than the five-year threshold Flores and Nooruddin use. This is potentially problematic; due to coders' tautological evaluation of democracy that immediately follows free and fair elections, "we should be skeptical of any

analysis that uses a one-year change in democracy as the dependent variable and is a primary reason [they] use a far more conservative five-year window” (Flores and Nooruddin 2016, 91). Brancati neither addresses such skepticism nor includes any robustness checks that vary the timeframe under consideration.

Brancati’s approach differs in three additional ways. First, her argument appears more inductively formulated than Flores and Nooruddin’s (2016). This, in part, may derive from deep knowledge about the cases, developed from coding protests, but it also misses the opportunity to establish a generalizable model of democracy protests. Second, Brancati makes scant reference to the importance of a history of democracy protests, while Flores and Nooruddin’s (2016) explore the historical legacies of political violence. The citizens of some countries are systematically more interested in using protests as a tool of democratic influence than others; over half of all democracy protests in Brancati’s (2016) data occur in only 11% (21 of 192) of the coded countries. Third, Brancati focuses less on the international drivers or effects of protests, which makes it difficult (1) to contextualize the peaks (e.g., 1989 and 2011) and valleys (e.g., 1999 and 2001) of these protests’ popularity over time, and (2) to gauge the Level 2 and Level 3 impacts of international actors (e.g., election monitors and democracy aid). Despite these issues, Brancati’s theoretical and empirical research represents a significant contribution both to the election protest and democratization literatures. It also touches on the strategic usefulness of violence—the subject of the final book under review.

### **Election violence**

After failed democratization and democracy protests, election violence is the third contentious election response, and the focus of Claes’s (2016) *Electing Peace*. Failed democratization and democratic protests can result from elections held in countries with limited Level 1 structural capacity. Election violence is no less stabilizing (or strategic) than governments’ decisions to

accommodate or repress protests, and the use of violence is crucial to the perceived procedural and governing legitimacy of the electoral winners. Election violence is both a contentious election outcome and a form of political violence with a unique set of motivated perpetrators, targets, and time focus (Höglund 2009; Birch et al. 2020). It is also an enduring threat in contentious elections. Roughly 20% of all elections see some form of election violence (Hyde and Marinov 2012). The form and intensity of this violence varies substantially. Some elections escalate rapidly to widespread post-election violence (e.g., Kenya's 2007 and Côte d'Ivoire's 2010 presidential elections). In general, however, smaller scale election violence is more common (Daxecker et al. 2019), occasionally occurring even before election day. Like both the democratization and civil conflict literatures, recent sub-national and cross-national quantitative research on election violence takes an election-focused or conflict-focused approach (e.g., Birch and Muchlinski 2017; Daxecker et al. 2019).

As with democratic protests, typologies are crucial in defining election violence, its perpetrators, their motivations, and their targets. A lively debate in the election violence literature centers on what election violence is and how important motivation and timing are to it (Fischer 2002; Höglund 2009; Staniland 2014). Some see motivation as less important than timing, while others see motive as differentiating election violence from other forms of political violence (Daxecker et al. 2019). Regardless of definitional approach, explaining election violence involves substantial overlap with the explanations for failed democratization and democratic protests. Risk factors for election violence also cluster around Level 1 factors, including centralized power structures, the process of consolidating democratic institutions, horizontal inequality and societal diversity, electoral system design, uncertainty about election outcome, and a history of violence.

While early efforts deductively typologized election violence (Höglund 2009) and studied global trends (Fischer 2002), a significant proportion of election violence research derives from

African cases (e.g. Collier and Vincente 2012; Bekoe 2012; Straus and Taylor 2012; Burchard 2015) and the myriad causes of violence in this region. Like the other two outcomes, several recent book-length works on the topic exist. Burchard (2015), for example, focuses on the causes of violence in Kenya, Liberia, and Senegal; Becher (2015) studies ethnic violence short of civil war in Kenya and Malawi; and Matanock (2017) explores peace settlement provisions and civil war. Nevertheless, there has been much less work on violence prevention.<sup>9</sup> Therefore, it is of both theoretical and policymaking importance that Claes (2016) examines the efficacy of violence prevention efforts. In particular, *Electing Peace* identifies eight forms of election violence prevention efforts: (1) security sector engagement, (2) election management and administration, (3) preventative diplomacy, (4) peace messaging, (5) civic and voter education, (6) monitoring and mapping, (7) voter consultations, and (8) youth programming. Claes, like Flores and Nooruddin (2016), adopts an international focus and an appreciation for the role and importance of Level 2's international actors. Furthermore, the eight interventions derive, in practice, more from what international actors do to prevent violence than from what might best address the underlying causal mechanisms identified in the literature. This disconnect is unfortunate, for connecting interventions' effectiveness to the underlying causal mechanisms would have strengthened its argument.

*Electing Peace* offers a qualitative, structured-focused comparison of violence prevention efforts in five countries with recent national elections—Bangladesh, Honduras, Malawi, Thailand, and Moldova—that had significant variation in both election violence and prevention

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<sup>9</sup> That is gradually starting to change, however. For instance, see von Borzyskowski (2019) on democracy aid and international election observers' effects on the likelihood of election violence.

efforts.<sup>10</sup> Election violence falls along a five-category scale, as do prevention efforts, with substantial variation across the five cases. This organization supplies a desperately needed contribution to the field. In it, Claes (2016) adopts an opposite approach to Bekoe (2012), which examines nine specific elections to see whether general findings (see Straus and Taylor 2012) can explain specific elections. *Electing Peace* is one of the rare edited books with a clear ex ante structure that contributors apply consistently across cases.

While the first two reviewed books explicitly highlight their main causal mechanisms, Claes (2016) only implicitly recognizes Level 1 causal factors.<sup>11</sup> For example, in the discussion of election violence in Bangladesh (Chapter 2), Level 1 “contextual vulnerabilities”—such as power centralization, religious division, first-past-the-post election rules, and single member districts—take center stage. Similarly, violence surrounding Thailand’s 2014 snap election (Chapter 3) link to incomplete democratization—a structural factor also relevant to the cases of Malawi (Chapter 4), Moldova (Chapter 5), and Honduras (Chapter 6). Level 1 structural factors common to all five cases include an unconsolidated democracy, a history of election violence, centralized power structures, horizontal inequality, social diversity, a majoritarian electoral system, and uncertainty about electoral outcome. The focus on Level 1 historical legacies sounds similar to Flores and Nooruddin (2016); but unlike the other two books, *Electing Peace* largely ignores economic issues. This is surprising, not least because violence prevention programs are often costly and (domestic or international) economic capacity necessarily shapes the sustainability of such programs.

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<sup>10</sup> Claes (2016) includes two explicit selection criteria for these five case studies: (1) some history of election violence and (2) governments considered partly free by Freedom House.

<sup>11</sup> Claes (2016, 215-6) waves a quick hand at Level 2 actor dynamics and Level 3 triggering events.

Claes (2016, 195-209) reaches three conclusions about the effectiveness of election prevention mechanisms. First, prevention mechanisms can work, but an early needs assessment must select the appropriate ones. Importantly, the relationship between violence and prevention can be endogenous; violence often inhibits prevention efforts due to safety concerns. Notably, though, diplomatic interventions to prevent violence often increase in frequency when stakes are high and violence is a real possibility. Second, state actors are crucial, but international engagement with these actors varies and often fails to address what Claes calls the ‘paradox of prevention.’ “[S]tate-led prevention is required to steer the perpetrators and enablers of violence away from violence; [while] at the same time political leaders are commonly responsible for the violence,” Claes (2016, 197). Here, the security sector and election management bodies are particularly important for shaping the risk of violence. Third, civil society engagement yields mixed results, largely because of its overly ambitious goals.

Overall, the five case studies suggest that prevention works, provided that prevention efforts are not implemented out of habit and are relate to specific Level 1 structural issues. What works? Four of the eight prevention mechanisms exert a significant, pacifying effect: security sector engagement, election administration efforts, civic and voter education, and election monitoring and mapping. The remaining four—voter consultation efforts, peace messaging, youth programming, and preventative diplomacy—prove less effective.

In general, *Electing Peace* does several things well. To an extent not seen elsewhere, it describes and evaluates a diverse menu of election violence prevention mechanisms. The case studies then evaluate the extent to which actors implemented these prevention mechanisms effectively in particular elections. That said, two main shortcomings remain. First, it is unclear which specific Level 2 political actors hold responsibility for election violence and therefore what related prevention measures will successfully prevent it. The case studies largely operate under the assumption that elite actors (e.g., government representatives, the security sector, and

election management) are crucial to preventing violence. They also suggest that the risk of violence increases when elites prioritize their own partisan interests over that of the country. How do prevention efforts shape these partisan interests, and which types of partisans are more likely to be perpetrators? *Electing Peace* does not answer this crucial question. Furthermore, as Flores and Nooruddin (2016) suggest, previous civil wars lead to contentious elections and a smaller democratic dividend. Moreover, current or former rebels, terrorists, and insurgent groups often use violence during the election cycle. Claes's (2016) case studies recognize electoral actors' history of violence as a risk factor for future violence. However, previous research suggests that elections are also opportunities for other actors (besides the incumbent and political opposition groups) use violence during the election cycle as a means of receiving added attention or shaping an anti-incumbent electoral outcome (Bali and Park 2014; Harish and Little 2017). Several recent studies illustrate this, particularly those looking at how insurgent violence relates to elections in Afghanistan (Condra et al. 2018), Peru (Birnie and Gohdes 2018), and Colombia (Weintraub et al. 2015). Additionally, international actors can also decrease the risk of electoral violence in ways Claes's (2016) eight prevention mechanisms do not capture (e.g., peacekeepers or foreign aid; Flores and Nooruddin 2016; von Borzyskowski 2019).

Second, this book lacks clear, systematic, and comparable links between cause and effect. The cases implicitly highlight some of the factors that emerge from the comparative election violence literature (e.g., a legacy of election violence or weak institutions); yet the focus lies with prevention, rather than causal relationships. This contrasts starkly with the broader election violence literature, which focuses almost exclusively on causal processes and

overlooks prevention. Indeed, Claes stands out as the only book-length treatment of prevention mechanisms—one that will hopefully trigger more research in this area.<sup>12</sup>

### **Cross-cutting themes and areas for future research**

The three books reviewed here focus on three distinct, but interrelated, contentious election outcomes. A synthesis brings four cross-cutting themes to the forefront, highlighting a number of overlapping causal mechanisms. First, each work focuses on democratization, electoral legitimacy, and contention. The global advance (and recent ebb) of democratic institutions has changed both elite and mass opinion about the process of selecting political leaders and how they govern. Because elections have become the most common way to select leaders, the electoral process is crucial to leaders' legitimacy. This raises the stakes and makes elections—and the design of electoral institutions—even more contentious.

Second, electoral history matters. As competitive elections repeat within a country, parties, candidates, and the public accept them more. The democratization process then strengthens, leading to norms and patterns of electoral behavior that shape how the electoral process unfolds in the future, including the possibility of protest or violence. Once violence becomes part of the electoral process, it is harder to prevent in subsequent elections, as actors develop the skills or motivations to use it. Every national election in Bangladesh since 1986, for example, has produced some form of election violence (Hyde and Marinov 2012).

Third, background structural conditions receive the lion's share of theoretical and empirical attention—at the expense of Level 2 actor behavior and Level 3 triggering events, including

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<sup>12</sup> Some recent election violence research exists (e.g., Birch and Muchlinski 2017). Flores and Nooruddin (2016) also show how underlying causal processes link to the effectiveness of particular prevention mechanisms (see above).

election cycle dynamics. This is both surprising and less directly helpful for prevention or policy efforts. In the short term, governments or concerned international actors cannot significantly reshape Level 1 structural characteristics (e.g., economic capacity or horizontal inequality). Moreover, one cannot blame the works reviewed here for such a focus; other work in this research area also heavily stresses Level 1 factors, whether because of a cross-national (rather than cross-temporal) theoretical focus or data availability. Still, it represents a missed opportunity.

Fourth, money matters. States need economic resources both to hold elections and to govern after them. In politics, money also shapes political participation and outcomes (Scarrow 2007). Two of the reviewed books recognize this. Flores and Nooruddin (2016) concentrate on economic levels (capacity), while Brancati (2016) looks at economic change (specifically, crises). Nevertheless, economic factors often receive scant explicit theoretical attention, relegated empirically to control variable status (e.g., Hafner-Burton et al. 2014).

Given these themes, three issues merit further discussion. First, work heavily stresses elite, state-level actors and their motivations. Considering a broader set of actors (e.g., the security sector and women), as well as their interests and actions, would allow for a more complete understanding of the outcomes. Second, work underexplores election integrity's effects on contentious elections, despite the fact that this integrity establishes a government's procedural legitimacy. Third, indirect international diffusion effects on contentious election outcomes, as well as direct international interventions into the electoral process, deserve greater attention. To understand these effects, we need greater consideration of an election's international context when explaining contentious election outcomes.

### *Expand the set of relevant actors*

These books (and the research more broadly) mainly highlight a few Level 2 actors. Flores and Nooruddin (2016), for example, concentrate on state actors, including state-based international, democratic interventions. Brancati (2016) spotlights government-protest group pairs, while largely ignoring international actors. Finally, Claes (2016) considers domestic political elites, along with domestic and international actors' interventions. In short, a set of established actors receive significant attention at the expense of other potentially important ones. To highlight the potential costs of this approach, I discuss two largely overlooked actors: the security sector and women.

The security sector (e.g., military, police, or intelligence groups) plays an important role in Brancati's (2016) story; it decides whether to support the government or the opposition. It also features prominently in Claes's (2016) understanding of elite decision-making and enforcement and less so in Flores and Nooruddin's (2016) path to democratization. The security sector often fulfills crucial functions in democratization, repression, and violence processes. Consider, for instance, the Tatmadaw, Myanmar's armed forces. In 1990, the ruling junta annulled the first multi-party election since 1960; it then actively repressed democratic protests in 2007, before initiating political reforms in 2011. The Burmese population approved a new constitution, leading to multi-party elections in 2015. The Tatmadaw maintained a veto-wielding minority in the legislature, but Myanmar's Polity score changed from -3 in 2014 to 8 in 2016—a twelve-point swing. Expressions of religious nationalism and the Rohingya crises have more recently led to increasing concerns about democratic backsliding (Fink 2019). This type of pendulum swing to and from multiparty elections to protests, violence, and military involvement also appears in Egypt, Thailand, and Turkey.

The security sector also represses or accommodates pro-democracy protests. In China, for instance, security actors first stayed out of Tiananmen Square, the focal point of democratic

protests in 1989, before dramatically intervening to remove protestors. Their actions led to the deaths of hundreds if not thousands of people (Lusher 2017). In contrast, Sudanese soldiers protected pro-democracy protestors outside the Defense Ministry in Khartoum in 2019—only days before President Omar al Bashir stepped down (Reuters 2019). Finally, security actors frequently repress opposition supporters and coerce supporter voting (Hafner-Burton et al. 2014).

Like the security sector, a gendered analysis would show women’s fundamental importance in shaping the nature of contested electoral outcomes, especially election violence. For instance, Konte and Klasen (2016) find that women in Africa are less likely than men to support democratization. That Flores and Nooruddin (2016) analyze citizens’ support for democracy in a post-conflict environment without considering the possible gendered differences in this support is therefore surprising. Doing so requires a noteworthy assumption and, perhaps, an erroneous one (e.g. Kornberg and Clarke 1994). Indeed, a closer look at the Guatemalan survey data Flores and Nooruddin (2016) use reveals gender differences. Figure 2 suggests that, from 1996 to 2015, Guatemalan women consistently preferred democracy less than men—a trend that also applies in other Latin American countries and confirms Konte and Klasen’s (2016) findings in African countries.

[Figure 2 about here]

In Sudan and Togo, women have also recently taken the lead in organizing democracy protests (Mohamed 2019; Agence France-Presse 2018). Brancati (2016, 7) refers briefly to Togo— noting only that opposition parties have called for women to withhold sex to protest the removal of term limits. Nevertheless, work on democratic protests remains largely ungendered. Finally, previous research suggests that men and women often face different forms of electoral violence. Men more likely experience physical and public acts of violence, while women experience private and online psychological violence. The latter can have important electoral

effects, including lowering voter turnout (Bardall 2013) and decreasing women's satisfaction with the election process. The three books sidestep this gender effect as well. Future research might therefore investigate more thoroughly how the genders (or other possible domestic cleavages) experience contentious elections and their aftermath differently, as well as how the security sector can both exacerbate and alleviate pressures for contentious election outcomes.

### ***Consider election integrity***

An election's (Level 3-focused) integrity also needs greater theoretical and empirical attention, especially given its importance to all three outcomes. Election integrity is "the agreed upon international conventions and universal standards about elections reflecting global norms" across the election cycle (Norris 2013, 21). Leader incentives to violate electoral integrity, including through manipulation and fraud, can affect these norms and standards. And while these Level 3 dynamics can be rooted in both Level 1 (e.g., economic capacity) and Level 2 (e.g., leader incentive) issues, they change more year-over-year than the Level 1 and 2 factors. It is therefore conceptually important to disentangle levels that often interact or affect one another. For instance, political scientists often assume that leaders want to maintain power (Bueno de Mesquita et al. 2003). The books reviewed here, however, rarely address specific, predictable parts of the election cycle or their integrity. Flores and Nooruddin (2016) suggest that incumbents' limited, credible campaigning options, due to constrained fiscal space, drive electoral fraud and manipulation. They also recognize election integrity's effects on democratization and find election integrity instrumentally benefits democratization in the short run, with diminishing effects. For Brancati (2016), democracy protests respond to election integrity shortcomings, but these shortcomings pale in comparison to the importance of economic crises. For Claes (2016), electoral processes factor into the election violence case studies, but with little focus on Level 3 factors, except for violence prevention efforts.

Nevertheless, Level 3 triggers frequently spring from failures of electoral integrity (Norris 2015; Birch 2012; von Borzyskowski 2019).<sup>13</sup> Some of these failures result from technical issues, including ballot design (Wand et al. 2001), aggregating votes (Cheeseman et al. 2017), the independence or experience of the election management body (Hartlyn et al. 2008; Onapajo 2015), and foreign election-focused aid (Uberti and Jackson 2020). Other failures arise from an incumbent's ability to tilt the playing field in her favor, which decreases an election's freeness and fairness and helps explain short-term failures to democratize. Relatedly, election fraud often seemingly motivates post-election protests, but it is unclear exactly how. Does the motivation spring from who gets to contest elections, or something later in the election cycle (e.g., stuffing ballot boxes or obscuring and manipulating the ballot counting process)? Finally, election malpractice also drives the use of violence during the election cycle, whether the targeting of candidates or voters before the election or protestors afterwards (Hafner-Burton et al. 2014). In short, Level 3 election integrity can shape all three contentious election outcomes, but the reviewed books largely overlook such a possibility.

### ***Include international dynamics***

Finally, scholars often ignore the vital international context of contentious election outcomes. As highlighted above, each reviewed book touches on international actors and their interventions to varying degrees, but the focus sits squarely on domestic structures, elite actors, and domestic policy formation at the expense of the (often crucial) international context.

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<sup>13</sup> The three sorts of contentious election outcomes discussed here also affect perceived election integrity. Reciprocal relationships between these factors over longer time frames would be an important area for future research to consider.

International actors, however, have both indirect and direct effects on contentious election outcomes.

(Testable) indirect effects include the diffusion of democratic and electoral norms from abroad (della Porta and Mattoni 2014). Norm diffusion can encompass governments learning about the costs and benefits of cutting off the internet before elections (Freyburg and Garbe 2018); protestors adopting the most effective means of avoiding government repression, getting people out in the streets, or attracting media coverage (della Porta and Tarrow 2005); or management bodies learning administrative best practices and prevention mechanisms, including civic and voter education efforts.

Direct effects highlight visible international interventions—something more likely to be considered in the reviewed books. These interventions include military, economic, diplomatic, and informational assets deployed by international state, interstate, or non-state actors. The most overtly intrusive interventions in contentious elections would be the presence of international military personnel on the ground. Such personnel threaten to weaken an election's (and government's) legitimacy if others see it as preferring a particular side or can enhance legitimacy if they provide electoral security without a visible bias. For instance, the United States' substantial military personnel in Iraq arguably hurt the independence and legitimacy of Iraq's 2005 parliamentary election (Burns 2005), while United Nations peacekeepers helped provide stability and security in Timor Leste's pre-independence 2001 and 2002 elections (Pushkina and Maier 2012). Economic interventions can supply aid for enhancing long-term economic development or short-term election-related capacity building. Diplomatic interventions—like those Claes (2016) discusses as a conflict-prevention mechanism—and informational interventions would monitor and map initiatives that international organizations support or deploy to gather and disseminate information about the electoral process.

## **Conclusion**

In this review essay I examine three recent books, highlight their contributions to their respective literatures, and explain the ways they enhance our understanding of contentious election outcomes. I also highlight four crosscutting themes. First, there is a common focus on democratization, electoral legitimacy, and electoral contention. Second, history casts a shadow. Established norms and patterns of behavior shape the electoral process including whether a peaceful transfer of power or violence are seen as part of the electoral landscape. Third, background structural conditions receive the lion's share of theoretical and empirical attention at the cost of an in-depth consideration of Level 2 actor behavior and Level 3 triggering events including important (and often unanticipated) election dynamics. Fourth, money matters. Economic resources are crucial for both the effective holding of elections as well as the governance of a functioning society.

I also suggest three areas for future research. First, the literature would benefit by broadening its set of relevant actors, and I describe how security services and women help shape contentious election outcomes. Second, election integrity also affects these outcomes in often unappreciated ways that help explain otherwise unexplained within-country variation. Third, global political winds directly and indirectly help shape electoral dynamics through norm diffusion, learning, and international interventions.

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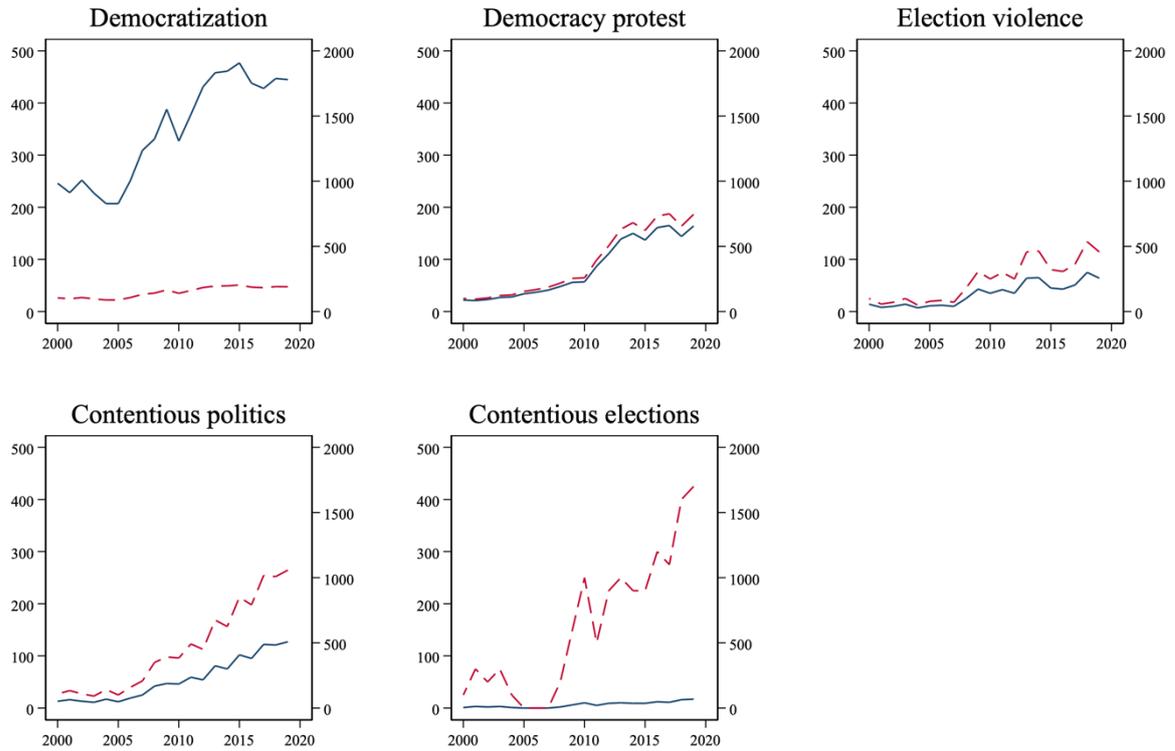
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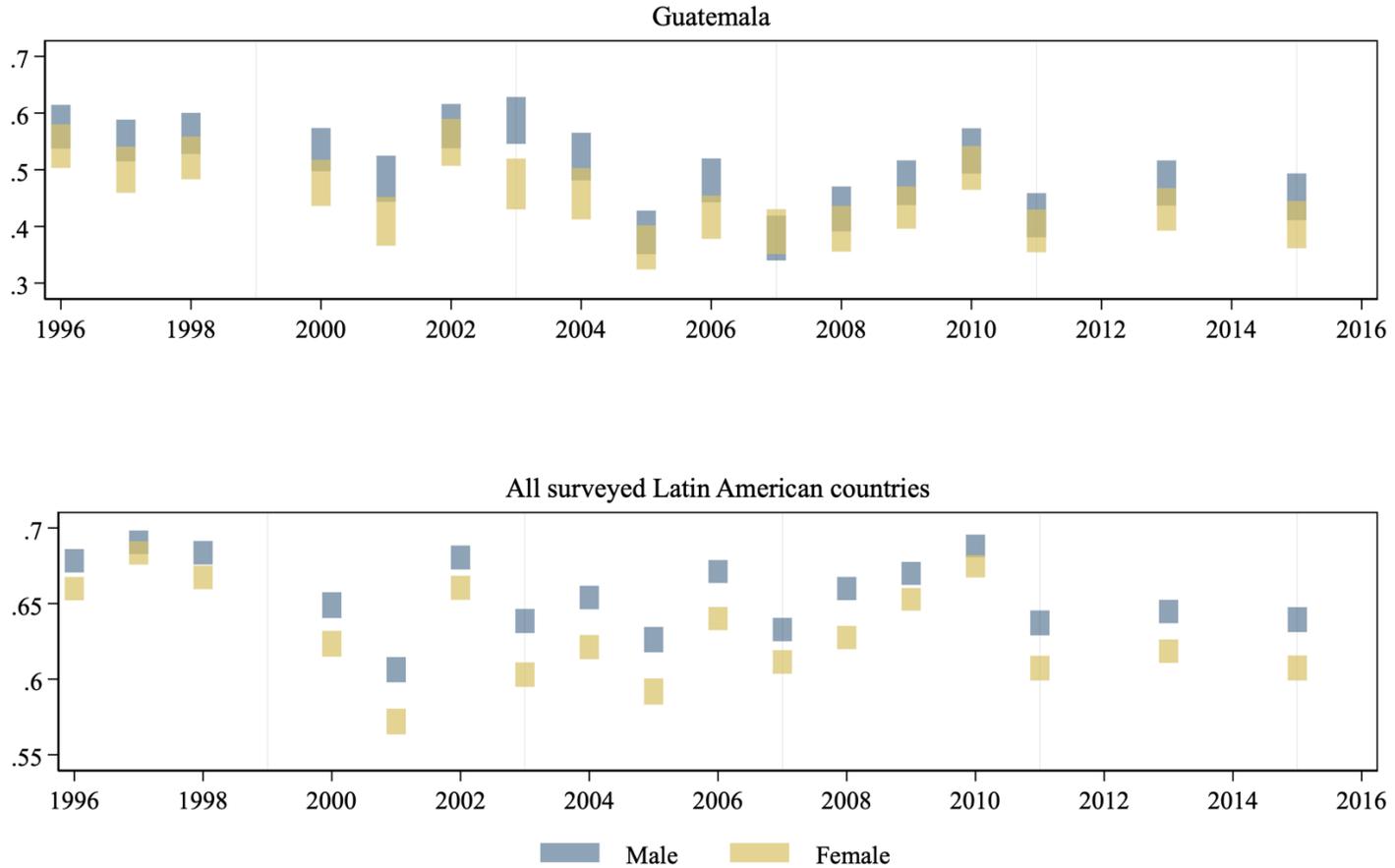
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Figure 1. Article frequency in Scopus database, 2000-2019



Note: Left axis and **solid lines** measure absolute frequency of articles mentioning topics in title or abstract. Right axis and **dashed line** measure frequency of articles mentioning a topic as a percentage of the number in 2000. Search terms used: “democratization”, “democra\* AND protest”, “election AND violence”, “contentious AND politics”; and “contentious AND election\*”.

Figure 2. Gender differences in preferring democracy



Note: Bars represent 90% confidence intervals around the mean value for the (0/1) question “Democracy is preferable to any other kind of government”. Data from Latinobarometer (2020). Two-tailed t-tests suggest that men have a statistically significant ( $p < 0.05$ , two-tailed) higher preference for democracy than women in all years except 1997.