Title

Digital and Physical Repression: Examining Internet Shutdowns, Protest Diffusion, and Electoral Violence in India and Beyond

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Digital and Physical Repression: Examining Internet Shutdowns, Protest Diffusion, and Electoral Violence in India and Beyond

By

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Abstract

This dissertation consists of three papers analyzing the dynamics of protest and repression in the digital age. It focuses on the interplay between government strategies such as internet shutdowns and traditional forms of physical repression, assessing their effectiveness and motivations across various contexts such as protests and elections. The first paper examines the strategic use of internet shutdowns by the Indian government to suppress dissent, particularly in regions with strong opposition presence. This study indicates that such digital repression tactics are not merely responses to violence, but also proactive measures to maintain political control. The second paper investigates the diffusion of protests, assessing the relative impact of geographic and social connections in the spread of movements. By combining an observational analysis of the India Farmers' Protest with an online experiment that isolates individual responsiveness to protest information, this study finds that although social networks facilitate protest diffusion, geographic proximity remains a significant factor. The third paper broadens the scope to global patterns of electoral violence, employing a close elections regression discontinuity design to explore the link between local incumbency and the perpetration of election-related violence. Collectively, this dissertation deepens our understanding of state control and repression tactics across diverse political landscapes and geographic regions, providing crucial insights into the evolving dynamics of technology, political power, and repression in our increasingly digital world, while also highlighting enduring patterns of state behavior and resistance strategies.

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Contents

CHAPTER 1

Introduction

This dissertation explores the dynamics of protest and repression in the digital age, scrutinizing how governments manipulate both physical and digital spaces to maintain control. Through three interlinked papers, this research investigates the strategies behind internet shutdowns, the mechanisms of protest diffusion, and the patterns of electoral violence. This exploration into how technology intersects with traditional methods of repression sheds new light on state power and resistance, highlighting the evolving tactics of control and the adaptability of protest movements in our increasingly digitally connected world.

The first paper explores the strategic deployment of internet shutdowns in India, challenging the notion that these are merely reactive measures for public safety. Focusing on the subnational execution of these shutdowns, this paper examines their timing, geographic distribution, and political motivations, particularly in response to partisan violence. The empirical findings reveal that shutdowns are most commonly enacted in areas with lower electoral support for the ruling party and following opposition-led violence. This selective use underscores that shutdowns serve as a politically motivated tool, distinct from traditional physical repression, aimed at curtailing threats to political power by targeting opposition strongholds.

The second paper shifts focus to the subnational diffusion of protests, illustrated through the case of the India Farmers' Protest. It integrates an observational analysis with a vignette experiment to explore how geographic, ethno-linguistic, and political ties influence protest spread. The observational study highlights the importance of geographic proximity in facilitating protest diffusion, while also demonstrating that shared religious and linguistic identities can significantly enhance the spread of protest. Complementarily, the vignette experiment isolates the effects of protest information on individual attitudes and willingness to participate in protest, distinguishing between mere exposure to protests and the specific appeal of their attributes. This combined approach reveals that while shared identities facilitate protest diffusion, geographic proximity remains a significant factor influencing the spread of protests.

The third paper, coauthored with Lauren Young, broadens the scope to a global analysis on electoral violence. This research uses a close elections regression discontinuity design to examine the effect of local incumbency on the perpetration of violence during elections. This study highlights the strategic use of violence as a tool to influence electoral outcomes, revealing patterns of state behavior that transcend national boundaries and contribute to our understanding of political stability and democratic processes.

Together, these papers illuminate the interplay between mechanisms of state control and citizen resistance. By examining the strategies of internet shutdowns, the contagion effects of protests, and the tactical use of electoral violence, this dissertation paints a comprehensive picture of the modern landscape of repression and resistance. It highlights the dual role of technology as both a facilitator of freedom and an instrument of repression, suggesting that the battle for control in digital spaces is a pivotal arena for contemporary political struggles.

Looking forward, my research agenda aims to further explore the themes of protest, repression, and the role of digital technology in political dynamics. Specifically, I plan to extend my fieldwork to Sri Lanka and India, focusing on how digital technologies are employed by both state and non-state actors in these regions. This future research will investigate the broader implications of digital repression strategies and protest dynamics, aiming to provide deeper empirical insights and theoretical advancements. By integrating field-based methodologies with comprehensive data analysis, I aim to contribute to a more nuanced understanding of the complex interactions between technology, political power, and citizen activism in diverse political landscapes.

CHAPTER 2

Strategic Disruptions: The Subnational Targeting of Internet Shutdowns in India

Since 2016, governments in 77 countries have implemented 1,118 internet shutdowns, revealing a growing trend in the use of "digital repression." Governments often justify these shutdowns as necessary for public safety and applied impartially in response to security threats. Focusing on India, the country that has enacted the most shutdowns globally, this paper explores whether shutdowns instead follow a logic of targeting political opponents, and how they compare with traditional forms of physical repression. The results align with a theory of strategic political targeting as shutdowns are predominantly executed in areas with lower ruling party support, following opposition violence, and prior to escalations in military violence. This pattern differs significantly from that of physical repression, revealing that internet shutdowns are a unique and politically motivated tool used by those in power to suppress opposition and facilitate state-sanctioned violence.

2.1. Introduction

Free and unfettered internet access has become an essential component of democratic processes. Modern citizens rely on the internet for access to information, economic activities, and personal communication. Moreover, the internet has become a vital tool for upholding and expanding political freedoms. Activists often leverage online platforms to voice their grievances, expose human rights abuses, and mobilize dissent (Enikolopov, Makarin and Petrova, 2020; Manacorda and Tesei, 2020; Pierskalla and Hollenbach, 2013). As a result, information and communication technology (ICT) presents a challenge to ruling authorities, enabling the spread of potentially destabilizing information and facilitating the coordination of opposition efforts. Therefore, governments might be motivated to strategically limit ICT access to fortify their rule.

State repression is not a new phenomenon. Autocratic regimes are often characterized by their use of repressive measures to raise the costs associated with dissent and maintain their authority (Danneman and Ritter, 2014; Davenport, 2007; Nordås and Davenport, 2013). Traditional physical repression includes direct actions against adversaries, such as targeted killings, imprisonment of key figures, and forceful suppression of protests. While often effective in the short term, these methods can provoke further resistance against the regime (Francisco, 1995; Moore, 2000), empower opposition groups (Kalyvas, 2006), and hinder the government's ability to collect information about its citizens (Wintrobe, 2000). Furthermore, implementing physical repression requires training, arming, and deploying violent actors who might ultimately become threats to the regime itself (Svolik, 2013). Therefore, despite its frequent use, physical repression can be costly, inefficient, and slow to enact.

Digital repression, which involves the use of modern technologies like the internet and social media for political control (Frantz, Kendall-Taylor and Wright, 2020), offers an efficient alternative to physical repression. These digital tactics are often more subtle, involving fewer unpredictable actors and lower risks of public backlash (King, Pan and Roberts, 2013; Qin, Strömberg and Wu, 2017; Roberts, 2018; Shadmehr and Bernhardt, 2015). The use of digital repression, however, does not exclude the simultaneous deployment of physical measures. In fact, governments often combine these tactics to enhance the effectiveness of their repressive efforts while curbing the risk of backlash. By controlling online communication, authorities can effectively limit the dissemination of information regarding acts of violence or human rights abuses, thus obscuring evidence that could lead to public outcry or international condemnation. For instance, during the Syrian civil war, network disruptions were linked to episodes of intensified state violence, suggesting a deliberate attempt to hinder the flow of information (Gohdes, 2015). A similar strategy was observed in Ethiopia, where the government enforced an internet blackout in anticipation of a military offensive in the Tigray region in November 2020 (Gohdes, 2020). These actions underscore the dual utility of ICT as both a means of suppression and a complement to physical force, offering a cost-effective and versatile tool for governments to maintain control and suppress dissent.

Internet shutdowns, defined as the deliberate disruption of network connectivity in specific areas or for particular groups (Esq and Dada, 2017; Rydzak, 2018), hinder political accountability by suppressing online communication and obstructing mobilization. These shutdowns restrict the dissemination of information, reducing awareness of grievances and protest activities, as well as instances of state repression. While there is a debate over their efficacy in suppressing protests, with some studies suggesting they might actually embolden radical factions and trigger violent conflict (Hassanpour, 2014; Rydzak, 2019), internet shutdowns are increasingly employed as a tool of control by both democratic and autocratic governments. Often justified under the guise of combating fake news, ensuring public safety, or protecting national security, there is growing evidence that governments use these shutdowns to oppress their populations, particularly during critical times like elections (Sutterlin, 2020), protests (Wagner, 2018), and periods of increased state violence (Gohdes, 2015).

In this study, I investigate the practice of internet shutdowns in India, a key case for understanding this phenomenon. India has recently become the world's most populous nation and ranks second in the number of internet users. However, India also stands out as the leading perpetrator of internet shutdowns. Unlike many countries that impose nationwide shutdowns,[1] the authority to shut down the internet in India rests with state officials, resulting in more geographically targeted shutdowns aimed at specific districts or cities. Leveraging India's localized approach to issuing shutdowns, I employ a time-series cross-sectional design with time and unit fixed effects to examine the local factors driving these shutdown decisions. The goal of this analysis is to uncover the underlying motives behind internet shutdowns in India and compare them with physical repression tactics, thereby shedding light on the distinct dynamics and strategic advantages of each approach.

The results reveal that internet shutdowns in India are deployed in a politically calculated manner. Districts where support for state opposition parties is high are more likely to experience internet shutdowns than those with strong ruling party support, even when accounting for concurrent levels of violence. The actors involved in these violent events are also crucial, with shutdowns occurring more frequently following violence perpetrated by opposition parties. Furthermore, government officials appear to use internet shutdowns preemptively, often implementing them prior to launching domestic military offensives. These patterns differ significantly from the use of physical repression, which is not precisely targeted towards opposition strongholds or in response to opposition-led violence. These findings suggest that internet shutdowns provide governments with

[1]Prominent examples of nation-wide shutdowns include Egypt during the Arab Spring in 2011, Myanmar during the 2020 coup, and Iran during the 2022 protests.

a more manageable and targeted means of repression than traditional physical tactics. This strategic and politically motivated use of internet shutdowns challenges optimistic views that ICT acts as a tool for empowering the oppressed. Instead, it indicates that digital technologies, when wielded by repressive regimes, can further tilt the balance of power in their favor.

This paper makes three key contributions to the literature on ICT and repression. First, it shifts focus from the consequences of internet shutdowns, which include adverse economic (Kathuria et al., 2018; West, 2016), social (Mawii et al., 2018; S.K. and Lakshané, 2018), and political effects (Hassanpour, 2014; Rydzak, Karanja and Opiyo, 2020; Stoycheff, Burgess and Martucci, 2020), to the determinants behind these shutdowns. Building on two recent studies (Collyer and Wright, 2021; Ruijgrok, 2022), this paper uses a district-level analysis to better understand how local heterogeneity in political support and contention influence the implementation of internet shutdowns. Second, the paper improves our understanding of how ICT alters the dynamic relationship between protest and repression. While ICT has been celebrated for empowering protest movements (Howard and Hussain, 2011; Little, 2016; Shirky, 2008; Steinert-Threlkeld, 2017), governments are increasingly using it to suppress dissent (Gunitsky, 2015; King, Pan and Roberts, 2017; Roberts, 2018). This suggests that ICT is a double-edged sword, serving as a technology of liberation while also being actively manipulated and curtailed by repressive governments, even in democratic settings. Lastly, this research offers a new perspective on the evolving strategies of state coercion. Governments have a diverse array of repressive tools at their disposal (Bagozzi, Berliner and Welch, 2021), and when selecting among these tools, officials must weigh potential benefits against the associated costs. While physical repression involves considerable logistical challenges, the control maintained by Indian authorities over national internet infrastructure allows them to swiftly and selectively shut down the internet. The paper's findings underscore how the strategic use of internet shutdowns align with political motivations, highlighting the move towards more technologically advanced and strategically sophisticated forms of state control.

2.2. Background on the Case

India, recognized as the world's largest democracy, operates under a federal parliamentary system that grants significant power to state governments. The country's political scene is primarily dominated by two major parties: the Bharatiya Janata Party (BJP) and the Indian National Congress (INC). Since the BJP came to power in 2014 under the leadership of Prime Minister Narendra Modi, it has remained the national ruling party and, as of 2024, governs 18 of the 28 states and administers most union territories.[2] During the BJP's tenure, India has shown signs of democratic erosion, including increased polarization, threats to minority rights, and limitations on the freedom of expression and assembly (Varshney, 2022). India has also witnessed a rise in communal violence, particularly between Hindus and Muslims, as well as several large-scale protests like the Farmer Protests and Citizenship Amendment Act (CAA) Protests. The Indian government's handling of these situations has drawn criticism, particularly regarding allegations of police brutality and internet shutdowns. These developments are reflected in Freedom House's 2021 reclassification of India as "partly free" and its designation as an "electoral autocracy" by the Regimes of the World index published by the Varieties of Democracy project.

Internet shutdowns, despite international criticism and calls for restraint from human rights organizations, have emerged as a prevalent strategy for governmental control. Such disruptions are not only common but have been recognized by the United Nations as breaches of international human rights laws when they deliberately obstruct the flow of online information. Paradoxically, India, with its rapid expansion of internet access - boasting the second highest number of internet users in the world - stands out as the leading perpetrator of internet shutdowns. As depicted in Figure 2.1, beginning in 2016, the frequency of internet shutdowns in India increased sharply, peaking in 2018 and maintaining high levels thereafter. The occurrence of these shutdowns has been widespread across India, affecting 26 of its 36 states and union territories, as shown in Figure 2.2. Unlike many countries that impose nationwide blackouts, shutdowns in India typically occur at the state or district level. This is a result of India's configuration of jurisdictional authority

[2]Union territories in India are usually administered by the national government - as is the case for six out of eight of these territories. However, Delhi and Puducherry are exceptions, having their own state legislative assemblies and chief ministers. The Aam Aadmi Party (AAP) governs Delhi, while Puducherry is under BJP rule.

over network disruptions which grants power to both central and state governments under certain circumstances.

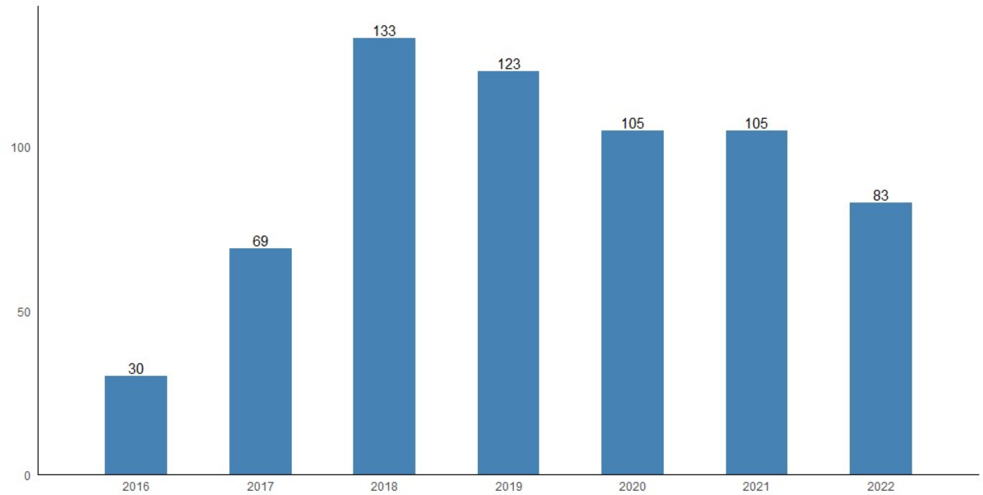


FIGURE 2.1. Number of Recorded Internet Shutdown in India, 2016-2022

Jammu and Kashmir has been particularly impacted, experiencing 423 internet shutdowns since 2016, the highest in the country. Officially, these shutdowns are often rationalized as necessary to counteract separatist insurgencies and violence. However, concerns are growing that they serve to conceal human rights violations perpetrated by state forces. Specifically, in Jammu and Kashmir, there have been alarming reports of human rights abuses, such as night raids, enforced disappearances, and the use of excessive force against protesters, coinciding with the internet blackouts (UN, 2019). Human rights advocates have strongly criticized these practices, urging the Indian government to cease the excessive restrictions on internet access in the region. Adding to the complexity, in 2019, the Indian Parliament redefined Jammu and Kashmir as a union territory directly administered by the central government. This shift granted the central government enhanced control over network disruptions in the territory, a power it has exercised numerous times since the reconstitution. This development has intensified concerns over the centralization of authority and the potential for abuses under the guise of maintaining public order in the region.

The most common official justification for executing an internet shutdown in India is to maintain public safety or national security during violent unrest and state security operations. Officials have also asserted that shutdowns were used to thwart the spread of fake news, hate speech, and illegal

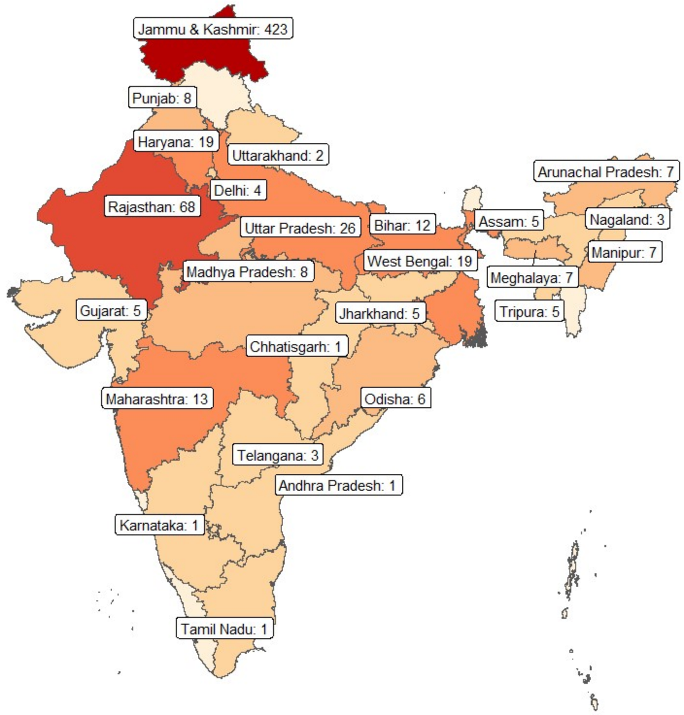


FIGURE 2.2. Number of Recorded Internet Shutdowns in India by State, 2016-2022

content and to prevent cheating on exams.[3] Despite official claims, there is reason to suspect that the true intentions behind many of these shutdown orders are not purely to uphold public safety, but instead are the result of strategic calculations made by officials to stifle the freedoms of political opposition groups. Shutdowns in India have increasingly been documented amid protest movements, such as the Indian Farmers' Protest and the protests against the Citizen Amendment Act. Furthermore, recent elections in India have also been marred by internet shutdowns. For instance, during the 2019 general election, network disruptions were reported in Rajasthan, West Bengal, and Kashmir, blocking voters from accessing important information as they prepared to cast their ballots.

2.2.1. How do Internet Shutdowns Occur?

[3]See Figure A.5 for the distribution of justifications given for internet shutdown orders in India.

Governments execute internet shutdowns by exerting influence over the domestic internet infrastructure through state-owned internet service providers (ISPs) or by pressuring privately-owned ISPs, threatening to revoke their operating licenses unless they comply with government demands. In India, internet shutdowns are primarily initiated through court orders issued to ISPs by State and Union Home Secretaries (Singh, Grover and Bansal, 2020). Originally, shutdowns were administered under Section 144 of the Code of Criminal Procedure, granting District Magistrates, the highest ranking officials at the district-level, broad discretionary powers during periods of heightened violence. However, the 2017 Temporary Suspension of Telecom Services (Public Emergency or Public Safety) Rules shifted this authority, enabling senior officials at both the national and state levels to issue shutdown orders during emergencies or to maintain public safety (Singh, Grover and Bansal, 2020). These orders are carried out by civil servants in the Indian Administrative Service (IAS). While originally conceived as a non-political entity, the IAS is significantly influenced by political forces, particularly through the manipulation of civil service appointments and transfers (Ruijgrok, 2022). As a result, these civil servants are incentivized to respond to the political priorities established by their respective state governments, including implementing targeted shutdowns.

The regulation of shutdowns in India has faced legal challenges. In 2019, the Gauhati High Court contested a shutdown in Assam, demanding the restoration of internet services. In 2020, the Supreme Court upheld the constitutional protection of internet freedom (Rautray, 2020), mandating public disclosure of shutdown orders and reviewing the suspension in Jammu and Kashmir (Jain, 2020). Furthermore, the Parliament Standing Committee on Communications and Information Technology criticized routine shutdowns for non-emergency situations in 2021, recommending a review of relevant laws and monitoring of shutdown orders (Singh, 2022). Despite legal advancements, the persistent and impactful nature of shutdowns in India underscores the importance of understanding how they are used and their effects on politics in India.

2.2.2. Political Violence in India

In Indian politics, characterized by its fervent and often contentious nature, partisan violence is not an uncommon occurrence as political parties compete for power. This form of violence, which is distinct from communal or insurgent violence, involves political parties engaging in aggressive tactics like electoral clashes and intimidation. While communal violence, fueled by religious or ethnic differences, can be influenced by electoral dynamics and shaped by political actors, it is important to draw a distinction between religious or ethnic riots and violence perpetrated by political parties. Partisan violence must also be distinguished from insurgent violence, which is prevalent in regions in India with a history of separatist movements. Insurgent groups can overlap with political parties, however, they often pursue goals that challenge the very foundations of the political system, such as disrupting or boycotting elections rather than influencing their outcomes. This paper focuses on partisan violence perpetrated by opposition parties to assess the political motivations behind internet shutdowns, distinguishing them from public safety or national security concerns.

The study's emphasis on partisan violence stems from its unique relevance to the phenomenon of internet shutdowns. By focusing on this type of violence, we can more accurately discern the political motivations behind the implementation of these shutdowns. When a government enacts an internet shutdown in response to violence from opposition parties, it is often a strategic decision aimed at suppressing dissent, preventing mobilization, or controlling the political narrative. This is different from shutdowns used to maintain public safety or national security in response to communal or insurgent violence. Moreover, examining partisan violence allows for a clearer understanding of the interplay between government actions and political opposition. In regions where opposition parties are strong, the government may resort to internet shutdowns as a means to curb the influence and reach of these parties, especially during times of heightened political tension or electoral competition. The decision to shut down internet access in such scenarios can be interpreted as an attempt to stifle opposition voices and hamper their ability to organize and protest, rather than as a measure purely for public safety.

2.3. Theoretical Framework

2.3.1. The Logic of Internet Shutdowns

Repression is defined as "the actual or threatened use of physical sanctions for the purpose of imposing a cost on the target as well as deterring specific activities and/or beliefs perceived to be challenging to the government" (Davenport, 2007). In this context, internet shutdowns are a form of state repression, allowing governments to suppress dissent and disrupt collective action by cutting off essential information and communication channels. Such tactics are used to impede protest movements and prevent opposition groups from mobilizing against the ruling party. In India, the decentralized execution of internet shutdowns gives local parties the power to selectively direct these actions towards specific populations in certain areas. Targeted shutdowns not only provide strategic political benefits but also align with national legal principles advocating for the "the least intrusive measure" to ensure public safety. Moreover, these shutdowns can be coordinated with other forms of repression, like physical violence, allowing the state to conduct operations under reduced public scrutiny by isolating affected communities and limiting their ability to share information about their situation.

Internet shutdowns offer government officials a means to stifle dissent and assert their authority. Nevertheless, they come with significant economic, functional, and credibility costs. The internet's crucial role in everyday life means that disruptions can lead to substantial economic losses (Howard, Agarwal and Hussain, 2011). For instance, a study by Kathuria et al. (2018) indicated that internet shutdowns in India from 2012 to 2017 resulted in an estimated economic loss of $3.04 billion. Additionally, these shutdowns limit the government's ability to utilize digital platforms for purposes like manipulating online discourse against opponents (Gunitsky, 2015) or diverting attention to less threatening topics (King, Pan and Roberts, 2017). They also hinder the government's capability to gather intelligence on public grievances and planned dissent, which could be used for targeted repression (Galperin, Marquis-Boire and Scott-Railton, 2013; Gohdes, 2020). Furthermore, restrictions on internet access might drive individuals to find alternative ways to access information, like using virtual private networks (VPNs), and potentially increase interest in political issues and critical discourse (Hobbs and Roberts, 2018). Importantly, shutdowns can undermine state legitimacy, as heavy-handed government interventions can backfire, damaging public perception and intensifying grievances against the ruling authorities (Huang, 2018).

In general, we should expect repression to be used in a way that maximizes its effectiveness while minimizing negative repercussions. However, the costs and benefits of repression vary across different contexts and types of repressive actions. Therefore, we should anticipate diverse approaches to repression, depending on the circumstances, timing, and specific methods governments choose to employ. While repression is more likely in situations where authorities perceive increased threats, the local environment and the nature of these threats significantly influence the choice of repressive tactics. For example, physical repression can signal the state's commitment to countering a threat, which might either reinforce the ruling party by showing their capacity to maintain order or damage their reputation by triggering public backlash against forceful measures. In the following section, I present my theoretical expectations about how local political support and the nature of violent incidents should affect government officials' propensity to repress, and their strategic considerations in selecting specific repressive methods.

2.3.2. Theoretical Expectations

The local political landscape shapes incentives to use different repressive strategies. In regions where the ruling party is popular and faces little electoral risk, officials might be less prone to use repression general. However, when repression is deemed necessary, it is likely to be through physical means. This approach, while overt can serve to entrench local divisions and solidify party allegiances by demonstrating the ruling party's commitment to maintaining order and protecting its constituents. In these strongholds, heavy-handed responses can actually reinforce loyalty among the ruling party's base, enhancing its grip on power while minimizing the risk of alienating swing voters, who are already less influential in such areas. Conversely, in districts where the ruling party's support is tenuous, the political calculus shifts significantly. These areas, characterized by a stronger presence of opposition support, present a direct electoral threat to state officials. The risk here is twofold: firstly, heavy-handed repression could galvanize opposition support, turning undecided voters against the ruling party; secondly, it could attract national or international condemnation, further eroding the ruling party's legitimacy. Consequently, officials in these regions may opt for more subtle forms of repression to manage dissent and mitigate threats. Internet shutdowns emerge as a preferred tactic in these contexts for several reasons.

First, internet shutdowns allow state officials to disrupt the opposition's ability to organize, communicate, and mobilize without the overt violence that could provoke public backlash or international scrutiny. By curtailing access to online platforms, state officials can effectively silence dissent, hinder the coordination of protests, and block the dissemination of information that could mobilize opposition support. This strategy not only neutralizes immediate threats but also undermines the opposition's longer-term electoral prospects by restricting their ability to campaign and engage with the electorate online. Second, internet shutdowns can be framed as responses to security concerns, allowing state officials to justify their actions under the guise of maintaining public order and national security. This framing can help mitigate potential backlash from the international community and from segments of the local population that might otherwise be sympathetic to the opposition's cause. In essence, the decision to employ internet shutdowns in opposition-dominated areas is a calculated strategy that reflects a deep understanding of the local political landscape and the broader electoral implications. It represents an attempt by state officials to maintain power and control in the face of significant political challenges, leveraging the internet as a battleground for political dominance. This leads to my first prediction that districts with lower ruling party support should be more prone to internet shutdowns (Prediction 1).

Furthermore, the identity of the actors involved in violent events significantly influences the decision-making of state authorities regarding the use of repressive tactics. Specifically, when violence is perpetrated by political opposition groups, it not only poses a direct threat to the physical safety and stability of the state but also challenges the ruling party's political authority and electoral security. These threats compel state officials to adopt measures that are both effective and expedient in curtailing the opposition's capacity to disrupt the political landscape. Internet shutdowns may be a favored strategy in these cases because violence perpetrated by opposition groups carries with it the risk of undermining the ruling party's image as a guarantor of public safety and order. In such scenarios, the ruling party faces the dual challenge of addressing the immediate physical threats posed by the violence and countering the broader political implications of the unrest. Inaction or inadequate responses can lead to increased dissatisfaction among the ruling party's supporters, who may perceive the government's inability to control the situation as a sign of weakness. This dissatisfaction can erode the party's base of support and diminish its legitimacy, both of which are vital for maintaining power. While physical repression can be used to counter these challenges, it is often seen as a less desirable option due to its visibility, potential for escalating violence, and higher economic and political costs. Physical repression can also provoke international condemnation and further solidify opposition resolve, making it a risky and sometimes counterproductive approach. Internet shutdowns, by contrast, offer a number of strategic advantages. They are swift to implement and can immediately disrupt the opposition's ability to communicate, organize, and mobilize, effectively blunting the impact of their actions without the direct use of force. By cutting off access to critical communication platforms, state authorities can limit the spread of information about the violence, control the narrative, and prevent the opposition from rallying further support. This approach not only addresses the immediate security concern but also mitigates the risk of further political destabilization by silencing opposition voices and reducing their visibility within the public discourse. Thus, my second prediction posits that internet shutdowns should be more common following violence linked to opposition parties (Prediction 2).

Finally, the effects of internet shutdowns extend well beyond mere disruption of communication networks. These shutdowns serve as a strategic tool for state authorities, significantly affecting the dynamics of conflict and civil unrest. By severing online access, authorities not only impair the organizational and mobilizational capabilities of opposition or civilian groups but also strategically manage the flow of information, which plays a critical role in the modern battlefield of perceptions and narratives. Internet shutdowns create significant barriers for affected communities in terms of coordinating responses to state-led military actions. The absence of online platforms, which are crucial for real-time communication and organization, disrupts the ability of these groups to effectively organize resistance. This fragmentation and isolation of opposition efforts can significantly diminish their capacity to respond to or counteract state actions. Furthermore, these shutdowns play a pivotal role in controlling the narrative surrounding state violence. In the digital age, the rapid dissemination of information can galvanize public opinion, both domestically and internationally, against state actions. By restricting access to the internet, authorities can effectively delay or minimize the spread of information regarding military offensives or instances of state-led violence. This reduction in the visibility of such actions decreases the likelihood of immediate public outrage, potentially mitigating the intensity of both domestic backlash. The strategic manipulation of information through internet shutdowns also aids in obscuring events from global view, thereby limiting the scope for international scrutiny and criticism. In contexts where authorities anticipate or engage in military offensives that could be deemed controversial or unjustifiable by international standards, maintaining a degree of opacity through internet blackouts can provide a tactical advantage. This controlled flow of information allows authorities to proceed with their military objectives with a lower risk of external intervention or condemnation. These dynamics lead to my third prediction that in the wake of internet shutdowns, there should be an increase in military-led violence (Prediction 3).

2.3.3. Alternate Expectations

In this study, I test whether there is a disjuncture between the official justifications for internet shutdowns given by Indian government authorities and the observed patterns in where shutdowns are implemented. The central question is whether these shutdowns are genuinely aimed at curbing violent unrest and maintaining public safety, or if they are politically motivated, designed to disrupt the activities of groups that pose a threat to the ruling party. If the official narrative holds true, we should expect to see a consistent pattern of internet shutdowns across India, not influenced by the political dynamics of different regions but closely linked to instances of violence. In such a scenario, areas experiencing unrest, regardless of their allegiance to ruling or opposition parties, would face similar chances of internet disruptions. Conversely, if internet shutdowns are being employed strategically for political gain, a different pattern should emerge. We would likely observe a higher frequency of shutdowns in regions with stronger opposition support and following violent incidents attributed to opposition-affiliated groups. Such a pattern would indicate that internet shutdowns are being leveraged to undermine political opposition.

Additionally, if the primary purpose of internet shutdowns is to manage to violent unrest and ensure public safety, without serving as a cover for military-led violence, then these events should be unrelated. In this scenario, shutdowns might coincide with non-military state responses to unrest or be implemented as preventive measures in anticipation of potential disturbances, but should not be directly linked to military operations. This pattern would suggest that while shutdowns are not systematically employed as a smokescreen for military violence. The absence of a temporal alignment between shutdowns and military actions would challenge the notion that these measures are being used to conceal aggressive military maneuvers, pointing towards a more complex interplay between internet control and state responses to perceived threats.

2.4. Research Design and Data

To empirically investigate how internet shutdowns are targeted in India, I employ a time-series cross-sectional design, incorporating time and unit fixed effects. This approach takes advantage of subnational variation in the implementation of internet shutdowns across India, a result of the country's decentralized approach to internet regulation which enables state officials to initiate network disruptions at the local level. The primary variable of interest is the onset of an internet shutdown in a specific district during a given week. To identify these events, I use data from the Shutdown Tracker Optimization Project (STOP) by Access Now,[4] which provides key information about each disruption, including its timing,[5] duration, and geographic location.[6]

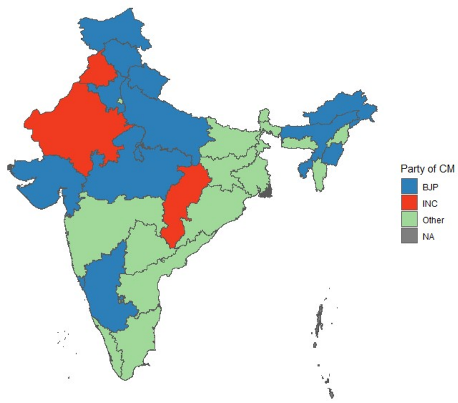
The analysis first examines two primary predictors of internet shutdowns: (1) political support for the ruling state party and (2) violence perpetrated by political opposition groups. Additionally, I explore the relationship between internet shutdowns and the initiation of military-led violence.

My first predictor is the level of political support for the ruling party in each district-year. In states and union territories, the ruling party is that of the Chief Minister, the position that holds de facto executive authority over the area. The primary opposition party is identified as the party that received that most votes in each district when excluding the party of the Chief Minister. I measure political support as the vote margin of the ruling party in the most recent prior state legislative assembly (Vidhan Sabha) election using data from the Trivedi Centre for Political Data. This is calculated by taking the difference between the vote share received by the

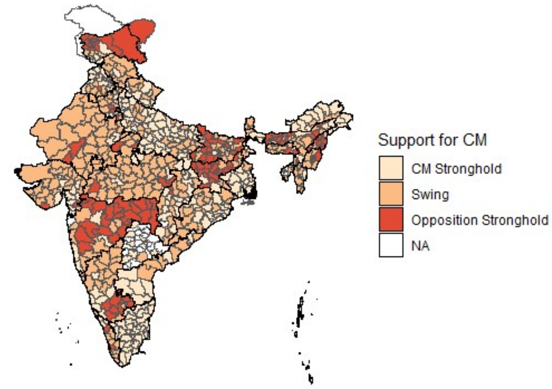
[4]STOP records network disruption events, encompassing full network shutdowns, bandwidth throttling, and service-based blocking for two-way communication platforms. This tracker gathers information from multiple sources including IODA, OONI, Google and Facebook transparency reports, Censored Planet, Internet Society Pulse, and several others.

[5]STOP provides the start and end date for most network disruption events. For the few cases where the end date is not specified, I assume that the network disruption lasted for a single day.

[6]STOP includes the name of area affected by the internet shutdown, which may range from an entire state to a city or town. To ensure precision in examining how shutdowns are targeted at the district level, I identified the corresponding district or districts that match, contain, or are contained within the specified affected area. I excluded a few instances of state-wide shutdowns to focus on district-level variations.



(a) Party of the Chief Minister



(b) District-Level Ruling State Party Support

FIGURE 2.3. These figures depict the Chief Minister's party in each state (a) and its district-level of political support (b) in 2021.

Chief Minister's party and that of the primary opposition party in each district. The distribution of the ruling party vote margin, presented for each state in Appendix Table A.1, ranges from -100 to 100, where -100 signifies that the opposition party received 100% of the votes and 100 means the Chief Minister's party received 100% of the votes. Administrative districts were chosen as the unit of analysis rather than state legislative assembly constituencies in order to match the units used in the data on internet shutdown locations. As a result, district political support is a coarse measure that reflects the aggregated results from multiple assembly constituencies that make up each administrative district. The party of the Chief Minister and their level of political support in each district is depicted in Figure 2.3 for the year 2021. According to my first prediction, I expect to find a negative relationship between ruling party support and the onset of an internet shutdown.

The second predictor is the number of violent events perpetrated by opposition parties in each district-week. I construct this variable using data on violent events from The Armed Conflict Location & Event Data Project (ACLED) which I classify according to their characteristics and the actors involved.[7] The perpetrators of violent events are defined using the coding rules detailed in Appendix A.2.2, which assign one or both of the event actors supplied by ACLED to be perpetrators depending on the event type. I then identify opposition-perpetrated violence as events in which the perpetrating actor matches the names of the opposition parties and their affiliates that are active in each state-year. The set of opposition parties and affiliates - which often include the party's youth

[7]This coding procedure, detailed in Appendix A.2, was adapted from a working paper by Lauren Young and Marika Miner titled "A Global Analysis of the Targeting and Effects of Election Violence."

wing and women's branch - consist of all district-level opposition parties in that state during the year the event took place. I aggregate these events to produce a count of the number of opposition-perpetrated violent events that occurred in each district-week. More details about this variable and coding procedure can be found in Appendix A.2.3. Following my second prediction, which posits that internet shutdowns are used in response to partisan opposition violence, I anticipate a positive relationship between these violence incidents and the onset of internet shutdowns.

Third, I investigate the connection between internet shutdowns and instances of military-led violence. Here, the onset of a shutdown is the independent variable and military-led violence is the dependent variable. As detailed in Appendix A.2.4, I identify these incidents as particular types of violent events perpetrated by the Indian government and state security services. Consistent with my third prediction that internet shutdowns are used as cover for domestic military offensives, I expect a positive correlation between internet shutdowns and subsequent instances of military-led violence.

Finally, to understand how physical repression is targeted relative to digital repression strategies like internet shutdowns, I examine the relationship between state-led physical repression and my primary predictors, using the coding rules in Appendix A.2.5, which consider physical repression events as those consisting of particular characteristics which are perpetrated by the government or state security services.

2.4.1. Estimation Strategy

To test my predictions regarding the determinants of internet shutdowns in India, I employ a time-series cross-sectional design with time and unit fixed effects. Given that official explanations for internet shutdowns often cite the need to prevent the outbreak or spread of violence, it is important to consider that regions prone to violence might naturally see more shutdowns, independent of political dynamics or the affiliations of the actors involved in violence. To account for these factors, I include a measure for the number of violent incidents in each district-week. I also account for the level of violence in neighboring districts, recognizing that shutdowns may be a preemptive response to potential spillover violence. When analyzing the link between opposition perpetrated violence and internet shutdowns, I further control for non-partisan violence to disentangle actions taken due to political motivations from those that are a general response to violence. Additionally, I include week fixed effects[8] to mitigate any bias arising from temporal trends that might impact the decision to order an internet shutdown - such as the occurrence of religious holidays or the enactment of divisive national-level policies.[9]

My analysis also recognizes that Indian states differ in administrative practices and internet shutdown regulations, which could affect shutdown patterns. Furthermore, variations in political party control over the state may influence the frequency of internet shutdowns, especially if certain parties, particularly those aligned with the central government, are more inclined to employ shutdowns. Consequently, I include fixed effects to capture these regional variations. In models estimating the influence of political support on shutdowns, I use state fixed effects, while in models examining violence-related shutdowns, I use district fixed effects. This distinction is due to the different data granularity in measuring political support (based on yearly state assembly elections)[10] versus violent events (recorded at the district-week level). While this decision may not allow me to control for potential confounding factors that differ among districts within the same state, it is justified for several reasons. First, I account for violent events and exposure at the district level, which are among the primary confounding factors. Second, my primary objective is to examine how political support influences the targeting of shutdowns against specific districts within a state. Since shutdowns are typically imposed by state governments on specific districts within their jurisdiction, employing district-level controls in this context would not be suitable because such controls would capture the very variation I am seeking to explain.

To evaluate the relationship between political support and internet shutdowns, conditional on other relevant characteristics, I estimate the following specification:

$$\text{logit}\left(\Pr(Shutdown_{i,t} = 1)\right) = \alpha + \beta Support_{i,t-1} + \lambda_1 Violence_{i,t-1} + \xi ViolentExposure_{i,t-1} + \gamma_i + \delta_t + \varepsilon_{i,t} \tag{2.1}$$

$Shutdown_{i,t}$ indicates if a shutdown occurred in a district during a specific week, while $Support_{i,t-1}$ measures political support in that district from the most recent prior state legislative election.

[8]Week fixed effects refer to each week-year combination in my data.

[9]Notably, the period covered by my data (2016-2021) coincided with widespread protests in India following the passage of the India Farm Bills and the Citizenship Amendment Act.

[10]This measure relies on the outcomes of the most recent state assembly elections, which occur at five-year intervals. Given that my dataset spans from 2016 to 2021, most districts only have two distinct observations for political support during this period, resulting in limited district-level variation.

$Violence_{i,t-1}$ and $ViolentExposure_{i,t-1}$ capture the district's internal violence level and its exposure to violence from neighboring districts in the previous week, respectively. γ_i represent state fixed effects, capturing individual time-invariant characteristics, and δ_t are week fixed effects, measuring temporal trends common to all units.

For assessing the likelihood of shutdowns following opposition-led violence, I use:

$$\text{logit}\left(\Pr(Shutdown_{i,t} = 1)\right) = \alpha + \beta OppositionViolence_{i,t-1} + \lambda_1 OtherViolence_{i,t-1} + \gamma_i + \delta_t + \varepsilon_{i,t} \tag{2.2}$$

$OppositionViolence_{i,t-1}$ denotes violent events perpetrated by opposition groups, and $OtherViolence_{i,t-1}$ are violent incidents in the district carried out by groups who are unaffiliated with the opposition party. Both are lagged to represent violence in the prior week. γ_i are district fixed effects, while δ_t captures week-specific effects. I also control for district-level exposure to violence from neighboring districts in the previous week.

To explore the relation between internet shutdowns and subsequent military violence, I estimate:

$$MilitaryViolence_{i,t} = \alpha + \beta Shutdown_{i,t-1} + \lambda_1 Violence_{i,t-1} + \gamma_i + \delta_t + \varepsilon_{i,t} \tag{2.3}$$

$MilitaryViolence_{i,t}$ measures military-led violent events in a district, with $Shutdown_{i,t-1}$ indicating whether a shutdown occurred in the previous week. $Violence_{i,t-1}$ captures the overall level of violence in a district during the previous week. Again, γ_i and δ_t represent district and week-specific effects and I also control for district-level exposure to violence from neighboring districts in the previous week.

Finally, I apply a similar method to investigate patterns of physical repression, substituting internet shutdowns with instances of state-led physical repression.

2.5. Results

Consistent with my first prediction, the analysis reveals that districts demonstrating stronger support for the ruling state party are less prone to internet shutdowns. The results, presented in Table 2.1, indicate that a 10% increase in the vote margin for the ruling state party corresponds to a 23%

decrease in the odds of experiencing an internet shutdown. Notably, the interaction between political support and violence indicates that while politically supportive districts are less susceptible to internet shutdowns, this negative effect diminishes as the level of violence in the district rises. In other words, when violence is rampant, state officials are more inclined to implement internet shutdowns, even in regions where they enjoy substantial political support.

The patterns for physical repression differ significantly. Districts with higher support for the ruling party are actually more likely to experience physical repression. This finding implies that physical repression is more frequent in areas supporting the ruling party, indicating distinct strategic uses for digital versus physical repression.

TABLE 2.1. Political Support for State Ruling Party

	Internet Shutdown			Physical Repression		
	(1)	(2)	(3)	(4)	(5)	(6)
Political Support$_{t-1}$	-0.0246***	-0.0231***	-0.0252***	0.0084***	0.0091***	0.0079***
	(0.0046)	(0.0042)	(0.0045)	(0.0028)	(0.0028)	(0.0029)
Violent Events$_{t-1}$		0.1119***	0.1450***		0.3554***	0.3526***
		(0.0146)	(0.0106)		(0.0835)	(0.0732)
Violent Exposure$_{t-1}$		0.0599***	0.0592***		0.0498**	0.0507**
		(0.0102)	(0.0097)		(0.0250)	(0.0250)
Political Support$_{t-1}$*Violent Events$_{t-1}$			0.0018***			0.0018
			(0.0004)			(0.0014)
Fixed-effects						
State	✓	✓	✓	✓	✓	✓
Week	✓	✓	✓	✓	✓	✓
Observations	155,805	155,805	155,805	237,765	237,765	237,765
Pseudo R^2	0.39529	0.40832	0.40886	0.10035	0.12367	0.12395

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 2.2 presents the results examining the likelihood of internet shutdowns following violence led by opposition groups. The results align with my prediction that internet shutdowns are more frequently implemented following acts of partisan violence perpetrated by opposition parties. For each additional event of opposition-linked violence, the odds of a shutdown increase by 30%. This pattern points to targeted shutdowns in areas experiencing opposition-related violence, rather than a general response to all forms of violence as violent incidents carried out by groups unaffiliated with the opposition party do not appear to provoke shutdowns. Conversely, physical repression doesn't show a significant correlation with opposition-instigated violence when controlling for other forms of violence, suggesting that unlike internet shutdowns, physical repression is not specifically responsive to opposition violence.

Table 2.2. Opposition Perpetrated Violence

	Internet Shutdown		Physical Repression	
	(1)	(2)	(3)	(4)
Opposition Violence$_{t-1}$	0.2696***	0.2574***	0.1450***	-0.0062
	(0.0600)	(0.0564)	(0.0342)	(0.0552)
Unaffiliated Violence$_{t-1}$		0.0334		0.1809***
		(0.0278)		(0.0140)
Violent Exposure$_{t-1}$		0.0126		0.0682***
		(0.0122)		(0.0108)
Fixed-effects				
District	✓	✓	✓	✓
Week	✓	✓	✓	✓
Observations	45,390	45,390	229,779	229,779
Pseudo R^2	0.39453	0.39513	0.22104	0.22931

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 2.3 presents the relationship between internet shutdowns and military-led violence. The findings reveal a significant link, showing that the initiation of shutdowns often precedes increased military activities, independent of pre-existing violence levels. In districts where internet shutdowns occur, there is, on average, a 0.05 rise in the frequency of military violent events in the subsequent week. This increase is in comparison to a baseline prediction of 0.03 military violent events in the absence of a shutdown, escalating to 0.08 in the presence of a shutdown. This contrasts with physical repression, which does not show a significant preemptive link to military-led violence. These distinct patterns highlight the different strategies in digital and physical repression, underscoring the unique targeting approach of internet shutdowns compared to traditional physical repression methods.

Table 2.3. Military Led Violence

	(1)	(2)	(3)	(4)
Shutdown_{t-1}	0.1240**	0.0470***		
	(0.0502)	(0.0117)		
$\text{Physical Repression}_{t-1}$			0.0247	-0.1206
			(0.0154)	(0.0803)
$\text{Violent Events}_{t-1}$		0.0751		0.0935*
		(0.0461)		(0.0545)
$\text{Violent Exposure}_{t-1}$		0.0024***		0.0025***
		(0.0006)		(0.0007)
Fixed-effects				
District	✓	✓	✓	✓
Week	✓	✓	✓	✓
Observations	251,196	251,196	251,196	251,196
R^2	0.25689	0.27778	0.25655	0.28160

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

2.6. Robustness Checks

2.6.1. Alternative Measures of Political Support

To ensure that my findings are not solely dependent on my initial measure of political support, I incorporate two alternative metrics. The first is a binary indicator denoting whether the Chief Minister's party secured the majority of votes in each district. This metric offers a clear-cut distinction, categorizing districts into those where the ruling party has a clear majority versus those where it does not. This approach simplifies the understanding of political support, focusing on whether the ruling party holds a dominant position in a district. The second is a categorical index classifying districts as ruling party strongholds, swing districts, or opposition strongholds based on their vote share.[11] The results from this analysis, reported in Appendix A.4, affirm my

[11]Districts are considered ruling party strongholds if their vote share for the Chief Minister is at least 10% higher than that of the main district-level opposition party. They are considered swing districts if the vote margin for the party of the Chief Minister is within 10 percentage points of the opposition party. Opposition strongholds are identified

primary findings, showing a lower likelihood of shutdowns in districts where the Chief Minister's party is dominant. Specifically, shutdowns are less likely in districts where the Chief Minister's party won a majority of the votes and are more likely in opposition strongholds. This pattern reinforces the argument that political support for the ruling party plays a significant role in the decision to impose internet shutdowns. Furthermore, these alternative measures also corroborate the findings related to physical repression. The trend remains that physical repression is more prevalent in areas where the ruling state party is dominant.

2.6.2. Looking Beyond Violent Events

While this paper primarily focuses on violent events, non-violent activities like peaceful protests or strikes may also prompt internet shutdowns. In order to provide a more comprehensive understanding of the triggers behind internet shutdowns, I extend my analysis to include a broader range of events. The findings, presented in Appendix A.5, provide interesting insights into the contextual importance of violence for shutdown decisions. The results are consistent with my primary finding that shutdowns are less likely in districts where the ruling state party enjoys higher levels of political support. However, when evaluating all types of events, those led by opposition parties no longer appear to significantly influence the use of internet shutdowns. Physical repression also displays different patterns of responsiveness to this broader set of events. In contrast to my main findings, physical repression does not appear significantly related to political support when controlling for all events occurring in a district-week. Moreover, physical repression is positively related to opposition-led events, although events that are unaffiliated with opposition parties still exert a stronger, positive effect on the use of physical repression. The divergence in the use of internet shutdowns and physical repression in response to a broader set of events suggests that state responses depend on the local violence dynamics. Specifically, while violent events led by opposition parties appear to spark shutdowns but not physical repression, this does not hold when also considering non-violent political activities. This indicates that shutdowns are more selectively

as districts in which the vote share for the opposition party is at least 10% higher than that of the Chief Minister's party.

deployed in the face of threatening political violence, rather than as a blanket measure against all forms of political expression.

2.6.3. ACLED Bias

The analysis in this paper relies on the ACLED dataset, which has some limitations regarding the underreporting of events in less accessible or sparsely populated regions (Clarke, 2023). To counteract this, I refine my analysis to focus exclusively on urban districts. This targeted approach aims to mitigate the influence of reporting biases that may skew the data, especially in rural or remote areas where media coverage and event documentation might be less thorough. I identify urban districts as those with a population density that is above the national median using gridded population density data in 2020 from WorldPop. This criterion for selecting urban districts is grounded in the assumption that higher population densities correlate with better media coverage and more comprehensive event reporting. Urban areas, being more accessible and usually having more resources and infrastructure, are likely to have a greater presence of media and other reporting agencies, leading to more reliable and complete data. The central objective of this analysis is to assess whether the patterns observed in the broader study hold true even when considering only urban districts, where the accuracy and completeness of the data should be higher. The results of this analysis, presented in Appendix A.6, align with the findings from the main models. This consistency in outcomes between the overall study and the urban-focused analysis serves as a robust validation of the original findings. By showing that the trends and patterns identified are not merely a result of uneven data reporting across different types of districts, this approach strengthens the argument that the observed relationships between political support, violence, and the use of internet shutdowns and physical repression are not merely artifacts of data limitations but reflect genuine underlying dynamics.

2.6.4. Regional and Temporal Heterogeneity

To comprehensively assess the factors influencing internet shutdowns in India, I conducted a series of stratified analyses. These analyses are crucial in understanding the regional and temporal nuances that might affect the relationship between political support and the implementation of internet shutdowns.

A critical component of this stratified analysis is the exclusion of Jammu and Kashmir, a region with a unique political environment and distinct dynamics of violence and internet shutdowns. I reestimated my models without Jammu and Kashmir to determine if the trends observed are consistent across all states or vary significantly when this region is removed. The results, shown in Appendix A.7.1, show some regional-specific influences, especially concerning the relationship between political support and internet shutdowns. Although the overall trend remains the same, the relationship is not statistically significant without Jammu and Kashmir. This is not very surprising as Jammu and Kashmir is the most frequent target of internet shutdowns in India and thus contributes the most observations and variation in the use of this repressive tool. Additionally, in regions other than Jammu and Kashmir, shutdowns appear to be negatively associated with military offensives, suggesting that this form of dual-pronged repression is not common outside of Jammu and Kashmir.

Next, I explore the role of partisan dynamics on the implementation internet shutdowns, particularly the influence of the Bharatiya Janata Party (BJP), which is the national ruling party in India. I re-estimate my models on a stratified set of the data, subset according to whether states are governed by the BJP or another political party, to investigate whether the patterns of internet shutdowns found in the main analysis are more pronounced in states aligned with the national government. The findings, presented in Appendix A.7.2 reveal that in BJP-governed states, there is a strong negative correlation between political support for the ruling party and the occurrence of internet shutdowns. This contrasts with states governed by other parties, where this relationship is not significant. This demonstrates that the political affiliation of state governments plays a crucial role in the use internet shutdowns. Specifically, shutdowns appear to be a tool that can be wielded against political opposition areas, but only when the ruling state party is politically aligned with the national government. While states controlled by other parties may have similar incentives to order shutdowns, they may have less control over issuing and directing shutdowns or face push-back from the national government, diminishing their capacity to use shutdowns strategically. This could also imply that, despite being ordered and executed locally, shutdowns are essentially reserved for the national government's strategic interests, guiding local officials to enact shutdowns in areas deemed of national importance.

Finally, I examine how electoral cycles impact internet shutdown patterns. This is particularly important for understanding whether political calculations tied to elections play a role in the decision to implement shutdowns. In a supplementary analysis I focus on years with state legislative assembly elections and the 2019 general election. The results, shown in Appendix A.7.3, reveal that the patterns observed in the main analysis are not primarily driven by years in which state elections took place. However, during the 2019 general election there was a noticeable increase in the relationship between political support for the ruling party and the decreased likelihood of internet shutdowns. This points to the possibility that national elections introduce unique dynamics that affect the deployment of internet shutdowns, likely due to the heightened political stakes of these elections.

2.7. Conclusion

This study offers an in-depth analysis of internet shutdowns in India, revealing how these measures are strategically deployed. The variations in the use of internet shutdowns within India reveal that state officials leverage this tool against opposition strongholds, in response to opposition-led violence, and as a cover for state-led military actions. This contrasts starkly with the patterns observed in physical repression, which do not seem to align with such calculated political motives or targeted precision. Digital tactics, like internet shutdowns, offer governments a swift, cost-effective, and highly localized means to suppress dissent or perceived threats. This is a marked deviation from physical repression, hampered by logistical limitations, such as the need for force mobilization, which make it less adaptable to rapidly changing political circumstances.

These observations have important implications for the democratic potential of digital technology. Initially, the internet was heralded as a tool for liberation. However, its exploitation by governments for repressive purposes has significantly tainted this promise. Digital tools offer an efficient, cost-effective means of repression, potentially increasing their appeal to governments, including those that might have previously avoided overt repressive measures. The precision and lower visibility of digital repression tactics also allow for a more targeted and impactful suppression. The advent of advanced digital technologies, therefore, introduces a potent form of state repression with worrying consequences for political freedoms globally.

Looking ahead, future research in this field can further illuminate the dynamics of digital repression. A more nuanced analysis of network disruptions, differentiating among filtering, throttling, censoring, and complete shutdowns, would provide deeper insights into the operationalization and strategic choices within digital repression. Exploring the impact of both digital and physical repression on local political dynamics and public support could shed light on the broader implications for governments that employ these tactics. Furthermore, while this study focuses on India, internet shutdowns are increasingly observed worldwide. Future research should explore these tactics in different geopolitical contexts to understand how repressive strategies vary across regions and under various regime types. Such comparative studies would enrich our understanding of internet shutdowns as part of the larger landscape of state repression.

CHAPTER 3

Subnational Protest Diffusion: How the Farmers' Protest Spread across India

Protests often spread geographically, whether across neighboring college campuses, towns, or countries. Scholars have debated whether this protest diffusion is simply due to nearby places experiencing common conditions that drive protest at around the same time, or whether being exposed to protest actually foments protest in nearby areas through a causal effect. If there is a causal effect of exposure to nearby protest, is geographic proximity really what matters or is geography simply correlated with other forms of proximity, like shared religious, linguistic, or political identity, through which protest spreads? This article sheds light on how protests diffuse using both observational and experimental analysis in the case of India. In both analyses, I find that while identity ties matter to some degree, geographic proximity is important in its own right, and matters not only because it exposes people to protest but because it makes people more likely to react to protest holding exposure constant. Even in a highly digitally connected world, geographic proximity matters.

3.1. Introduction

Protests rarely occur in isolation. Instead, they tend to diffuse, particularly within countries, influencing the emergence and success of subsequent movements. The Color Revolutions and the Arab Spring exemplify this "contagion" effect, where protests spread rapidly across cities and countries. Understanding how protests travel and gain traction is crucial to comprehending broader socio-political movements. A successful protest wave may result in larger, sustained actions or new policies, whereas a failed one could fizzle out and weaken the movement's objectives. Research shows that geographically proximate protests increase the likelihood of local protests by amplifying shared grievances, providing effective protest strategies, and offering focal points for collective action coordination (Beissinger, 2007; Givan, Roberts and Soule, 2010; Gleditsch and Rivera, 2017; Weyland, 2012). Although scholars have explored protest waves across countries, comprehensive research on the dynamics of subnational protest diffusion remains limited.

Existing theories propose a variety of mechanisms to explain how protests spread within countries, including common shocks that synchronize grievances across a population, the diffusion of technologies that facilitate organization, the spread of grievances that provoke collective emotions, and the transmission of information regarding authorities' likely responses to protests. Each of these theories suggest different channels through which protests might diffuse, yet they often overlap and influence each other, complicating the analysis of how protests truly spread. For example, common shocks like economic downturns or political scandals can simultaneously affect diverse regions, creating a shared sense of injustice or urgency that precipitates protests. However, distinguishing the impact of these shocks from other factors can be difficult, as they often trigger secondary effects such as changes in communication or shifts in political opportunity structures that might also contribute to protest diffusion. Existing theories struggle to disentangle these related drivers of protest diffusion. In particular, a critical question remains about whether geographic proximity — traditionally viewed as a key factor in protest diffusion — truly has an independent effect, or if it merely acts as a proxy for other types of connections such as shared identities or common networks through which protests spread.

This study addresses these gaps by disaggregating the various networks — geographic, ethnolinguistic, and political ties — through which protests might diffuse. These linkages facilitate information flow while shaping how individuals interpret and respond to it. Previous research has show that recruitment into social movements often involves personal connections with existing members (Gould, 1993; Snow, Zurcher Jr and Ekland-Olson, 1980). Modern communication networks further amplify these ties, enabling rapid sharing of logistical information and protest tactics (Earl and Kimport, 2011; Pierskalla and Hollenbach, 2013; Steinert-Threlkeld, 2017; Tufekci, 2017). Yet, geographic proximity remains a dominant empirical measure of diffusion, potentially obscuring other influential mechanisms.[1]

Geographic and identity linkages, like shared language, religion, or political affiliation, can all influence the process of diffusion. Geographic proximity often dictates exposure to protests through

[1]Notable exceptions to this are Gleditsch (2007) and Weidmann (2015).

local media and social networks, highlighting shared concerns and civic interests, and making it easier for individuals to join. Social identity linkages, like shared religion or language, galvanize participation through the recognition of common grievances and positive social rewards. Political ties offer strategic benefits, revealing opportunities to find strength in numbers and mobilize power locally. By fostering exposure and reinforcing the social benefits of participation, these ties build support networks that enable the diffusion of protest movements. However, understanding which linkage is most potent remains challenging.

This paper refines our understanding of protest diffusion by analyzing the India Farmers' Protest, a case of subnational protest contagion. By mapping connections between Indian districts based on shared ethno-linguistic and political traits, this study identifies the underlying mechanisms driving the spread of protests and determines which factors most effectively promote widespread mobilization.

A key challenge in understanding protest diffusion lies in distinguishing exposure and from uptake. Exposure occurs when individuals encounter protest information or witness protests directly or through media channels. Uptake, however, involves actively deciding to support or join the protest after being exposed. Observational analysis can shed light on the channels through which protests diffuse, but it cannot clarify whether these linkages simply increase exposure or promote protest uptake. To address this, this study employs an online vignette experiment to isolate individual responsiveness to protest information conveying specific messages about protests and their participants. Participants were randomly assigned to treatment groups with varying protest attributes that either aligned with or differed from their own identities. By holding exposure constant while varying protest characteristics, the experiment reveals how different linkages influence protest diffusion. In combination, the observational analysis and experiment provide insights into how different linkages shape both exposure and uptake of protest information.

3.2. How Do Protests Diffuse?

The frequent clustering of protests have predominantly been explained through two primary theories: common shocks and diffusion. The common shocks hypothesis posits that geographically proximate areas often share characteristics and experiences that make them more likely to erupt into protest around the same time (Hale, 2013). For instance, neighboring regions might have similar political grievances or be hit by the same economic downturn, increasing their propensity to engage in collective action. In contrast, the diffusion hypothesis considers how protests in one location influence the likelihood of subsequent protests in adjacent areas. According to this theory, diffusion occurs by altering the informational and strategic landscapes in surrounding areas, thereby catalyzing a chain reaction of protests (Lohmann, 1994; Tarrow, 1991; Givan, Roberts and Soule, 2010). This paper aims to discern between these theories, investigating how protest diffusion might occur when accounting for the potential impact of common shocks.

An individual's decision to join a protest depends on two key factors: (1) receiving information about the protest (exposure) and (2) evaluating that information in a way that increases their likelihood of participating (uptake). Individuals are more likely to be exposed to nearby protests because they are more likely to be covered by local news sources and social media, and may directly involve members of their social networks (Starr and Starr, 2021; Kopstein and Reilly, 2000). Similarly, individuals sharing common identity traits with active protesters, such as a language, religion, or political affiliation, have a greater chance of being exposed to the protests through their social connections with the participants. Once exposed, these identity linkages also shape how individuals process and respond to the protests. Learning about protests that involve people with similar traits can enhance empathy with the movement, leading to a stronger willingness to participate. Therefore, exposure and uptake mechanisms should amplify protest diffusion in regions with shared identity ties.

To understand how protests spread, we first need to establish a theoretical framework that explains individual participation in protest. Drawing on classic models of protest behavior, this framework assumes that individuals weigh the expected costs against the expected benefits of protesting. Potential costs range from the perceived risk of sanctions, such as state repression or social disapproval, as well as travel and opportunity costs, and the inherent challenges of coordinating participation. Expected benefits include policy changes if the protest succeeds, social rewards from peers, and the intrinsic satisfaction derived from expressing one's views and grievances. Importantly, the inclination to express oneself depends on the salience of grievances; people might become accustomed to adverse conditions until those grievances are highlighted by exposure to protests.

Proximity to protests can significantly alter individual's cost-benefit calculations by reshaping their perceptions of both the risks and rewards associated with participation. For example, observing nearby protests can reduce the perceived risk of repression if the government appears overwhelmed or unable to control multiple protest sites. At the same time, the visibility of neighboring protests can heighten the perceived effectiveness and thus the potential policy gains from participating, while also enhancing the social rewards as protest behavior gains legitimacy and support in the community. This interaction of costs and benefits, influenced by nearby protest activity, is key to understanding how protest diffusion might function.

Demonstration effects are a primary mechanism in protest diffusion (Weyland, 2009; Bamert, Gilardi and Wasserfallen, 2015; Beissinger, 2007). Protest is a potentially high-risk form of dissent that faces significant collective actions problems. These arise because protest movements rely on widespread participation in order to bring about non-excludable benefits that can be enjoyed by everyone, not just the participants. This creates a freeriding problem whereby individuals hope to benefit from protest efforts without actively participating themselves, potentially undermining the movement before it gains momentum. Proximity to other protests can help overcome these collective action problems by increasing the salience of shared grievances, providing focal points for coordination, and raising expectations of protest success (Beissinger, 2007; Brinks and Coppedge, 2006; Gleditsch and Ward, 2006).

Witnessing nearby protests magnify the salience of shared grievances, sparking discussions about similar concerns and revealing widespread dissatisfaction. Protests serve as a spotlight, highlighting injustices in neighboring regions and making local issues more pressing. For example, Kuran (1998) argues that when groups observe ethnic conflict occurring in other countries, they become more aware of their own ethnic-based grievances. Similarly, when individuals learn about a protest movement taking place in their own country, the relevance and prominence of their related grievances should intensify, making them more likely to consider participating in protest themselves.

Coordinating a protest requires setting a time and place and disseminating the details to potential participants. Nearby protests can act as a focal point, helping protesters synchronize and coordinate their efforts (Carter and Carter, 2020). Seeing others participate generates a cascade effect, motivating people to join by visibly demonstrating participation and fostering a sense of safety in numbers (Kuran, 1998; Lohmann, 1994). As awareness of widespread dissatisfaction grows, so does the expectation of successful large-scale mobilization, thereby lowering the perceived risks of individual participation (DeNardo, 2014). Therefore, nearby protests make it easier to coordinate and generate participation for local protest events.

Exposure to nearby protests can alter the calculus of potential participants by enhancing their optimism about protest success. Observing successful collective action nearby provides tangible evidence that political change is possible. People often use mental shortcuts like the availability and representative heuristics, giving more weight to recent and similar examples when making decisions (Weyland, 2007). As a result, exposure to protest occurring in similar places and consisting of participants with shared identity traits should lead individuals to reassess their own chances of success, boosting the likelihood of local outbreaks.

Taken together, nearby protests increase the chance that people will be exposed to these events while also influencing their willingness to participate. Thus by highlighting shared grievances, facilitating coordination, and increasing confidence in successful outcomes, the spread of protest to interconnected regions becomes more likely.

3.3. The Indian Farmers' Protest

A spate of protests led by Indian farmers arose in response to three controversial Farm Bills passed in September 2020 aiming to overhaul India's agricultural sector. Around 60% of India's 1.3 billion population rely on agriculture to make a living. These pro-market Farm Bills reduced state protections for small farmers in an attempt to incentivize investment and economic growth. However, farmers feared these new rules would allow big corporations to dominate the agriculture industry and decimate their livelihoods. In recent years, rising debt among Indian farmers has led to a drastic escalation in the suicide rate for this group (Singh, 2020). The Farm Bills served as a flash point to mobilize dissent against these accumulating grievances and push back against the controversial reforms.

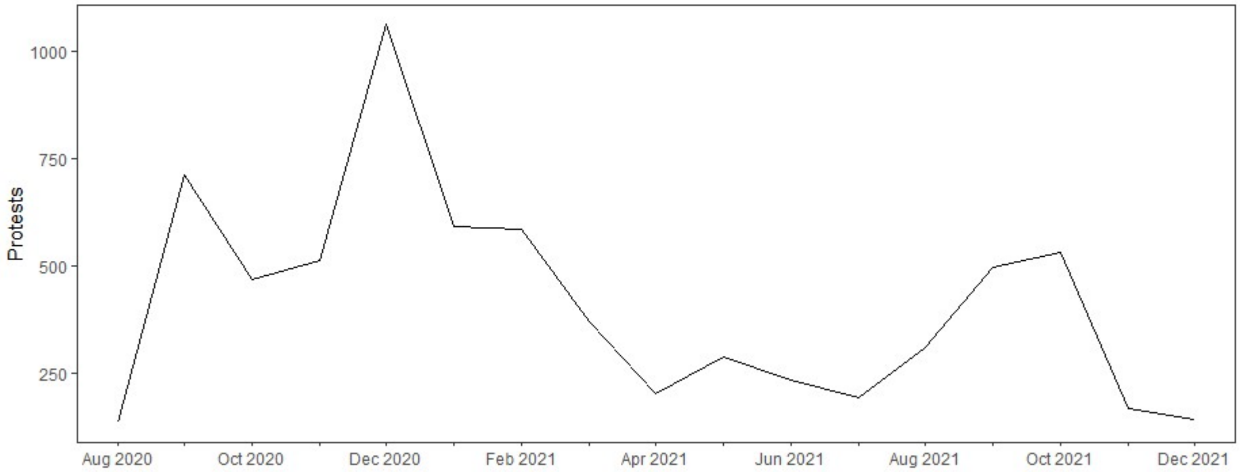


FIGURE 3.1. The Frequency of India Farmers' Protests Over Time

The protest movement was initially concentrated among Sikh farmers demonstrating in the northern states of Punjab and Haryana. However, as the movement grew, it attracted participants who spanned India's diverse social geography (Mander, 2021). Farm unions led a march to the capital city followed by a prolonged sit-in on the city's borders that was joined by other farmers and members of agricultural trade unions all over India. The Farmers' Protest hit a fever pitch when on January 26, 2021 - India's Republic Day - tens of thousands of protesters rode tractors into the capital, dismantling barricades and clashing with police. The Delhi police commissioner reported that this event resulted in the injury of 400 officers as well as widespread destruction in the city. This level of violence marked a stark divergence from the otherwise peaceful protest movement. Following the incident, several farmer groups chose to leave the protest and movement's leadership swiftly reinforced the importance of maintaining peaceful tactics. The Farmers' Protest was formally ended on December 9, 2021 following Prime Minister Modi's announcement that the Farm Bills would be repealed.

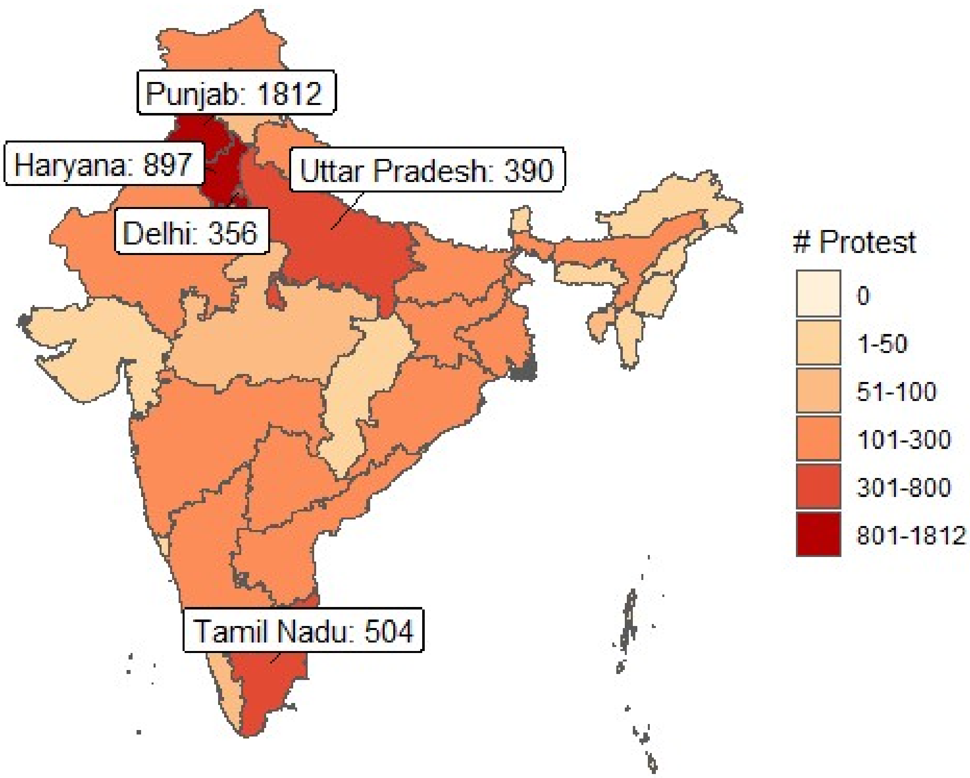


FIGURE 3.2. India Farmers' Protests by State

The India Farmers' Protest presents an ideal case for studying the diffusion of protest within a country and exploring the different mechanisms by which such diffusion occurs. India's diverse landscape, with its multitude of languages, religions, and political parties, played a significant role in shaping the contours of this protest. The movement's ability to mobilize diverse groups underscores its significance as an economic protest capable of crossing various identity lines, highlighting the interplay between economic grievances and identity politics within protest movements. By focusing on this protest, this paper develops valuable insights into the dynamics of protest diffusion, helping us understand how economic protests can spread across different identity lines and how these lines interact with geographic and social ties to shape the diffusion of contemporary social movements.

3.4. Diffusion Pathways in the India Farmers' Protest

Geographic and social ties play a pivotal role in the dissemination, reception, and evaluation of information about ongoing protests. These linkages not only determine the chances of learning about a protest but also shape how individuals perceive and react to such information.

Traditionally, the concept of 'proximity' in protest diffusion has primarily referred to physical closeness. Geographic proximity is a key factor as it often correlates with enhanced media coverage and greater visibility of protests. Individuals living near protest sites are more likely to witness these events firsthand or learn about them through media channels and local social networks. This direct exposure can impact individual protest propensity, potentially lowering the perceived risks of participation and heightening the perceived legitimacy and effectiveness of the protest movement. Moreover, protests can spread in ways that are not strictly bound by identity lines. In areas divided by distinct identities, geographic proximity might still enable the transmission of protest dynamics through more universalistic channels. For example, seeing a successful protest nearby may embolden individuals across identity lines, suggesting that change is possible and that collective action can yield results. This can lead to a broader, more inclusive understanding of community and collective grievances, promoting a sense of shared purpose that transcends specific linguistic, religious, or political divisions. Thus, the influence of mere physical proximity can enhance the visibility and perceived viability of protest movements, encouraging participation across diverse demographic and identity groups.

This dynamic was evident in the India Farmers' Protests, which originated in the agriculturally significant regions of Punjab and Haryana. These protests gradually spread to other parts of India, exemplifying how demonstrations in one region can inspire and ignite similar movements in neighboring areas through a contagion effect. This observation leads to my first hypothesis:

> **H1:** Districts that are *geographically adjacent* to areas experiencing protest are more likely to witness protest diffusion.

However, in today's highly interconnected world, the concept of 'proximity' has evolved to encompass more than mere physical distance. Digital communication technologies have redefined traditional notions of proximity by enabling rapid dissemination of news about protests far beyond their geographic origin.

Language serves as a critical bridge for the flow of information, allowing for the transfer of ideas, experiences, and strategies related to protest movements across vast physical distances. Protests and their underlying causes can be communicated more swiftly and effectively to regions sharing a common language with the protest's origin. The Farmers' Protest, initially led by Punjabi-speaking populations in India's northern states, highlights the significant role of linguistic ties in protest diffusion. The prevalence of Punjabi as a primary language among the protesting communities meant that districts with significant Punjabi-speaking populations were likely more receptive to the protest messages. This shared linguistic identity not only facilitated a clearer and more immediate understanding of the farmers' grievances and demands but also fostered a deeper emotional resonance with the issues at stake. Such understanding likely encouraged higher rates of mobilization and participation among Punjabi speakers, regardless of their geographic proximity to the original protest sites. In this vein, linguistic ties enhanced the accessibility and relatability of the movement, allowing the protests to cross geographic distances and resonate across diverse regions.

Religious ties also play an important role in the diffusion of protest movements, particularly when these movements are rooted in shared religious identities or grievances. The organizational structures within religious communities are particularly adept at rapidly disseminating information, which can catalyze and coordinate protest activities over wide areas. The Farmers' Protest was initially led predominantly by Sikh farmers from the northern states. This religious identity not only defined the early phase of the protests but also played a crucial role in how the movement expanded. Districts with significant Sikh populations, sharing both religious identity and the associated socio-political grievances, were more likely to resonate with the initial protest messages. These areas, bound by common religious practices and networks, likely experienced faster and more robust mobilization, driven by the strong community ties and the organizational capabilities in Sikh communities. This religious solidarity among Sikh-dominated districts, facilitated not just the awareness of the protests but also active participation. Shared religious identity amplified the perceived legitimacy and urgency of the farmers' demands, making the movement more accessible and compelling to those within the Sikh community. Thus, even when controlling for protest in geographically proximate areas, my second hypothesis regarding the dynamics of protest diffusion through linguistic and religious social identity networks is formulated as follows:

H2: Districts that share a substantial *ethno-linguistic population* associated with the protests are more likely to experience protest diffusion.

Political similarities can significantly impact the dynamics of protest diffusion by providing insights into the potential likelihood of repression or success. Observing the response of a particular political party or government in one district can give individuals in politically similar regions a sense of what to expect. For instance, if people see that a BJP government in one district responds with repression or concessions, they may infer that a BJP-led administration in their own district is likely to react similarly, either intensifying or reducing their inclination to participate.

In addition to inferred expectations about repression or success, political affiliations can play a direct role in aligning protesters around common goals. Shared political ideologies or support for specific parties can rapidly mobilize communities when the goals of a protest movement resonate with their beliefs. Networks connected through political affiliations facilitate the swift dissemination of information, encouraging participation from those who see their political objectives reflected in the aims of a protest.

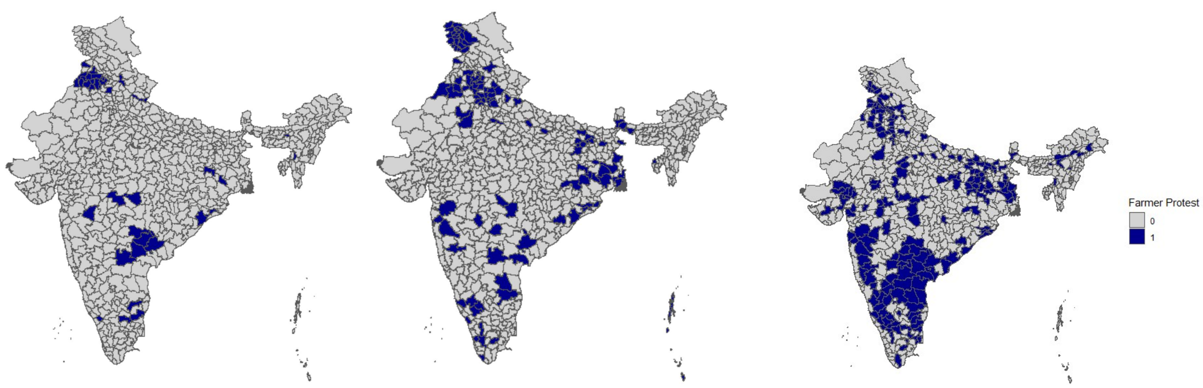
The Farmers' Protests in India demonstrated this effect, revealing significant intersections with various political ideologies and affiliations, particularly those opposing certain government policies. Notably, the protests garnered considerable support from opposition parties at both the state and national levels who criticized the central government's agricultural policies. The Indian National Congress (INC) and the Aam Aadmi Party (AAP), for instance, leveraged their platforms to express solidarity with farmers and amplify criticism of the ruling party's approach. This alignment suggests that districts where these opposition parties held significant support were more receptive to the protest's message and more inclined to join, due to their pre-existing opposition to the ruling party's policies.

Given the diverse political landscape in India and varied party responses to the Farmers' Protests, understanding the broader political climate and party support within districts is crucial for assessing the role of political similarity in protest diffusion. Therefore, my third hypothesis, which considers the role of political alignment across districts in the spread of the Farmers' Protests, posits:

H3: Districts that share a majority *political affiliation* as those experiencing protests are more likely to experience protest diffusion.

3.5. Empirical Approach and Data

I use a two-way fixed effects model to investigate the subnational diffusion of the India Farmers' Protest. Building on previous work on spatial diffusion (Gleditsch, 2009; Weidmann, 2015), I extend the typical 'spatial lag' model to assess the extent to which the occurrence of these protest events can be explained by different types of diffusion, ranging from conventional geographic contagion to the spread of protest between locations that are connected through ethno-linguistic and political ties. Using the Armed Conflict Location & Event Data Project (ACLED), I identify protest events related to the movement by applying a keyword coding procedure, detailed in Appendix B.1, on all protests that took place in India between August 1, 2020 and December 31, 2021, which I aggregate to the district-week level.[2]. Figure 3.3 shows how the Farmers' protests spread across districts over time.



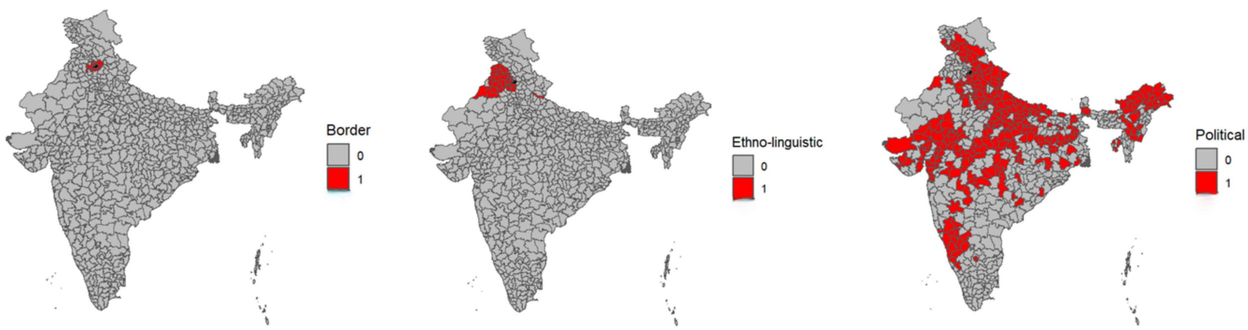
(a) November 15, 2020 (b) November 29, 2020 (c) December 13, 2020

FIGURE 3.3. These figures depict the districts where the Farmers' protests occurred over the span of five weeks in 2020.

Following previous studies on diffusion, I measure geographic proximity using shared borders between districts. However, I also consider several other measurement strategies such as centroid distance and second and third order geographic contiguity which I present in Appendix B.3. Next, I create novel proximity measures to identify districts with shared social identity and political characteristics. For social identity ties, I calculated proximity based on significant demographic

[2]I use the 666 GADM level 2 (district) borders to define India's districts because they match the units from other data sources used in this project such as the India Census. However, the number of districts in India has since increased to 750.

thresholds for religious and linguistic communities related to the Farmers' Protests, using data from the 2011 Indian Census. Specifically, I identify districts where Punjabi speakers and Sikh adherents each constitute more than 10% of the local population. Political alignments are identified using district votes shares in state legislative assembly (Vidhan Sabha) elections from the Trivedi Centre for Political Data. Because these elections are held at different times across Indian states, I took the most recent state election that occurred prior to the start of the Farmers' Protests. These election results were then aggregated from the constituency to the district level to find the political party that received the most votes in each district. The resulting dataset on district linkages indicates whether district-dyads share a geographic border, ethno-linguistic population, or dominant partisan affiliation. Figure 3.4 illustrates these linkages for the case of Ambala district in Haryana.



(a) (b) (c)

FIGURE 3.4. Geographic, Social, and Political Proximity of Ambala, Haryana.

I then construct several spatial lag variables using data on district-week protest events and the linkages between districts to capture potential diffusion processes. Districts are exposed to protest when a protest occurs in a district with which they share a tie and the type of exposure depends on the characterization of that tie. For example if a district borders another district that experienced a protest in a particular week, that district would be considered to be geographically exposed to protest in that week. Alternatively, a district is politically exposed to protest if it shared a dominant partisan affiliation with another district experiencing protest.

The main threat to inference in identifying the pathways of diffusion is the alternative explanation that the characteristics uniting different locations are actually causing the clustering of protest events. In other words, if protest is driven by district-level characteristics rather than interactions between districts. In fact, a common critique of the diffusion literature is that rather than protest flowing from one place to another, it is really just that places with similar underlying protest propensity tend to cluster in space. Thus, what may appear as diffusion is actually the clustering of units with a similar propensity to protest. I deal with this threat by including district and week fixed effects to flexibly control for time-invariant district characteristics that might make it more likely to experience protest, as well as weekly time shocks that affect all districts in the sample. This serves to control for the characteristics and underlying protest propensity of districts in India, ensuring that the observed clustering of protest events is not merely a result of shared characteristics among districts that are not controlled for in the analysis. Moreover, the relatively limited time window in which the Farmers' Protest occurred provides some added confidence that the characteristics of the districts are fixed throughout the protest period. The results therefore should partial out the effect of clustered underlying protest propensity and shocks to uncover the effect of protest exposure on subsequent protest outbreak.

My identification strategy relies on the assumption that there were no temporally varying factors that heterogeneously impacted a district's protest propensity. However, I also rerun my analysis with an added control for rainfall shocks to account for potential time-variant shocks that are common across geographically proximate districts, as detailed in Appendix B.4. I deal with serial correlation by including time spells that indicate the number of weeks that have passed since a district experienced protest (as well as in squared and cubic transformations), as proposed by Carter and Signorino (2010).[3]

3.6. Results

I estimate a logit model with two-way fixed effects using weekly data on protest events related to the Indian Farmers' Protest. I test the relative impact of different types of exposure on protest diffusion by including geographic exposure in a joint model with social identity and political exposure measures. As presented in Appendix B.2, there are relatively low levels of correlation between

[3] I include time spells to control for serial correlation rather than a lagged dependent variable because there is evidence that using a lagged dependent variable in fixed effects models introduces bias.

exposure from geography and social identity (0.30) and political party (0.17), making this a suitable approach to assess the relative importance of geographic, identity, and political ties for protest diffusion.

TABLE 3.1. The Relative Impact of Different Kinds of Protest Exposure on Protest Outbreak

	(1)	(2)	(3)
Geographic Exposure	0.2256***	0.2204***	0.1980***
	(0.0729)	(0.0723)	(0.0665)
Identity Exposure		1.253***	
		(0.1516)	
Political Exposure			0.2031
			(0.1553)
Fixed-effects			
District	✓	✓	✓
Week	✓	✓	✓
Observations	40,050	40,050	40,050
Squared Correlation	0.31722	0.31730	0.31763
Pseudo R^2	0.35754	0.35769	0.35777

Clustered (District & Week) standard-errors in parenthesis
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 3.1 presents the results from the joint models with district and week fixed effects. The analysis reveals that districts geographically exposed to protest related to the India Farmers' Protest in the previous week are more likely to experience a related protest. This holds true even when controlling for social identity and political exposure. The predicted probability of protest occurring in geographically exposed districts is 0.21, in contrast to just 0.06 in districts without such exposure. This underscores the spatial clustering of protests and suggests that bordering districts can significantly influence the protest dynamics of their neighbors. Further analysis, shown in Appendix B.3, confirms these findings using alternate measures of geographic proximity, such as centroid distance and higher-order geographic contiguity.

Social identity exposure also emerges as a noteworthy driver of protest contagion. When there is no geographic exposure, districts exposed to protest through their social ties have a predicted protest probability of 0.41, compared to only 0.08 without such exposure. Similarly, when districts are exposed to protest from their geographic neighbors, the probability of protest increases from

0.09 to 0.45 when they are also socially exposed. This suggests that ethno-linguistic ties served as a prominent factor influencing the spread of protest events during the India Farmers' Protest.

In contrast, I do not find evidence in support of protest diffusion across political linkages. Unlike other socio-cultural factors, political ties do not appear to have played a major role in the spread of the India Farmers' Protest. This could be attributed to the unique nature of the protest, which may have transcended typical political boundaries and united diverse political groups with a common cause against the Farm Bills, thereby obscuring traditional political diffusion pathways.

The spread of the India Farmers' Protest illustrates the different paths across which protests diffuse. Geographic proximity appears to have initiated the diffusion process, with neighboring regions quickly becoming involved. Ethno-linguistic ties then played a crucial role in extending the protest beyond immediate neighbors. While political connections did not seem to drive protest diffusion, connections between religious and linguistic communities allowed the movement to transcend state borders and resonate with individuals in areas with similar social demographics, even if they were not geographically adjacent.

3.7. Vignette Experiment on Drivers of Protest Diffusion

As a complement to the observational analysis, I conducted a vignette experiment embedded in an online survey in India. This experiment was designed to investigate how information about protests involving groups that are geographically, religiously, linguistically, and politically proximate or distant influences individual support for the protest and propensity to participate in similar actions. The diffusion of protest hinges on the premise that individuals receive information about an ongoing protest and assess that information such that it affects their inclination to participate in a similar protest. While the observational analysis provided evidence about the pathways of protests diffusion, it cannot fully differentiate whether diffusion results from mere awareness of the protest (i.e. hearing about it) or active engagement based on this information. The nature of diffusion might vary depending on the shared characteristics between the locations where protest occurred and potential new sites of protest. As such, the experiment is instrumental for distinguishing whether the reception and subsequent action upon information about protests differ when individuals share certain characteristics with the protesters.

The online vignette experiment was implemented in the spring of 2023 by the survey firm, Lucid, in collaboration with two fellow graduate students. In order to obtain a diverse sample, the survey was conducted in the states of Haryana, Kerala, and Nagaland, which were strategically selected due to their significant variation in terms of geography and population demographics, particularly with respect to religious diversity. The survey targeted a sample of 1,000 individuals, all of whom were at least 18 years of age and balanced with regard to gender. To ensure that our sample consisted of genuinely engaged and attentive respondents, we inserted two attention checks at the onset and midway through the survey, and only those participants who correctly responded to these checks were included in our analysis. Nonetheless, it is important to address concerns related to non-response bias. To mitigate this, we opted to restrict our analysis to participants who completed the entire survey, thereby ensuring that our findings are based on a consistent dataset. Consequently, the effective sample size consists of 963 individuals. Detailed information regarding the geographic and demographic distribution of the survey participants can be found in Appendix B.5.1.

Table 3.2. Vignette Experiment Treatment Groups

	Same Identity	Different Identity
Close District	Group 1	Group 2
Far District	Group 3	Group 4

The vignette experiment was based on a 2 x 2 design where participants were randomly assigned to one of four treatment groups. In each treatment group, participants were provided with different information about the identities and locations of a fictitious protest, as shown in Table 3.2. The vignette experiment was divided into three rounds, each of which involved new treatment assignments, unique protest scenarios, and distinct identity linkages. The first vignette focused on a protest concerning unsanitary water conditions and varied the geographic and religious identities of the protesters. The second vignette centered around a protest related to poor road conditions and varied the geographic and linguistic identities of the protesters. Finally, the third vignette, about a protest over waste management, varied the geographic and political identities of the protesters. The text used in these vignettes is as follows:

"There was a protest last week in [close/far district] in response to [unsanitary water conditions/poor road conditions/issues with the removal and storage of city garbage]. The protest was led by a [same/different Religious/Linguistic/Political Group] seeking greater government [protections for sanitary water conditions/attention to improving the quality of roads/waste management procedures]."

As an example, a respondent who indicated they are *Hindu* and live in *Faridabad (Haryana)* would receive the following information for the first vignette, depending on their assigned treatment group:

Treatment Group 1: "There was a protest last week in *Gurgaon (Haryana)* in response to unsanitary water conditions. The protest was led by a *Hindu group* seeking greater government protections for sanitary water conditions."

Treatment Group 2: "There was a protest last week in *Gurgaon (Haryana)* in response to unsanitary water conditions. The protest was led by a *Muslim group* seeking greater government protections for sanitary water conditions."

Treatment Group 3: "There was a protest last week in *Madurai (Tamil Nadu)* in response to unsanitary water conditions. The protest was led by a *Hindu group* seeking greater government protections for sanitary water conditions."

Treatment Group 4: "There was a protest last week in *Madurai (Tamil Nadu)* in response to unsanitary water conditions. The protest was led by a *Muslim group* seeking greater government protections for sanitary water conditions."

The protest scenarios were carefully designed such that they minimized the possibility of participants inferring additional information about the identity and attributes of the protest participants. Conducting research on sensitive topics, particularly those related to protests, can give rise to social desirability bias, where participants may be inclined to provide responses that conform to perceived social norms or expectations, rather than expressing their genuine beliefs. To counteract this bias, and to reduce the chance of evoking anger or strong emotions among the participants, I selected protest topics that were fictional and intentionally benign. Additionally, the online and anonymous nature of the survey environment was deliberately established to mitigate social desirability bias and encourage participants to offer more candid responses. For more in-depth details on the vignette experiment, please refer to Appendix B.5.

I estimated the relative effects of geographic and identity linkages by comparing participant attitudes and support for the depicted protests across the treatment groups. The outcomes are constructed using survey questions, detailed in Appendix B.5.4, which encompass participants' attitudes towards the protest and protesters, in addition to their stated inclinations to participate, advocate for friends' involvement, post on social media, or organize a similar protest. The average responses to each of the questions demonstrate that respondents across all vignettes were, on average, most likely to indicate higher agreement with the statements about their support for the protest movement, participation in a similar protest, and posting on social media about the protest movement. In contrast, respondents were less likely to agree with the statements about asking a friend to participate in a similar protest, support for the protesters, and organizing a similar protest. The main dependent variable represents the average value across these six questions. I also measure participant support for the protest by taking the average of their responses from the first two questions on their support for the protest movement and the protesters. Finally, I measure participants' willingness to participate in a similar protest by averaging the remaining questions asking about their willingness to engage in actions related to a similar protest.

Although respondents were randomly assigned to treatment groups, effectively reducing potential issues related to selection bias, I also control for several pre-treatment covariates. These include participant gender, religion, language, political party, and the district in which they reside. For my analysis, I stacked the results from the three vignettes such that each participant appears three times in my data, with their respective treatment assignment and outcomes for each vignette. I include respondent fixed effects to account for baseline differences between individuals that might influence their responses across the vignettes. By doing this, the results identify within-person changes due to treatment assignment rather than between-person differences. Since I am using repeated measures from the participants, I clustered the standard errors by respondent to minimize issues associated with correlated errors.

Table 3.3 shows relative impact of protest characteristics on subsequent protest attitudes, with dummy variables indicating whether or not the participant received information that the protesters shared their reported identity and if the protest took place in a geographically proximate or distant district. These results demonstrate that receiving information about protests led by groups who share the respondent's identity improve overall protest attitudes and willingness to participate, but do not significantly impact respondents' support for the protest. On the other hand, presenting respondents with information about a protest occurring in a proximate district resulted in improved overall attitudes towards the protests, including greater levels of support for the protesters and willingness to participate in activities related to a similar protest.

TABLE 3.3. The Effect of Identity and Geographic Proximity on Protest Attitudes

	Total		Support		Participate	
	(1)	(2)	(3)	(4)	(5)	(6)
Same Identity	0.055**	0.060*	0.044	0.045	0.060**	0.068*
	(0.023)	(0.067)	(0.217)	(0.344)	(0.018)	(0.056)
Close Geography	0.064***	0.069**	0.098***	0.099**	0.047*	0.055
	(0.006)	(0.032)	(0.006)	(0.045)	(0.055)	(0.117)
Identity∗Geography		−0.010		−0.002		−0.015
		(0.817)		(0.982)		(0.752)
Respondent Controls	✓	✓	✓	✓	✓	✓
Observations	2,644	2,644	2,643	2,643	2,640	2,640
R^2	0.818	0.818	0.695	0.695	0.816	0.816
Adjusted R^2	0.714	0.714	0.520	0.520	0.712	0.712

Adjusted p-values for clustered respondent standard-errors in parenthesis
*Signif Codes: ***: 0.01, **: 0.05, *: 0.1*

Figure 3.5 shows the effects for each treatment group. The base group, *far geography and different identity*, serves as a benchmark against which the outcomes of the other treatment groups are compared. When comparing outcomes in the different treatment groups to those of the base group, respondents who were told the protesters both were aligned with their identity and geographically proximate (Close-Same) were more likely to indicate that they were supportive of the protest and inclined to participate in a similar protest. Participants who received information that the protest was in a geographically proximate district but led by a group with a different identity (Close-Different) were also more likely to report supportive attitudes about the protest. Interestingly, respondents were not significantly more likely to support protests that were led by identity groups that matched the participants' identities but occurred in distant locations (Far-Same). This suggests that geographic proximity plays a larger role in influencing protest diffusion that social identity linkages.

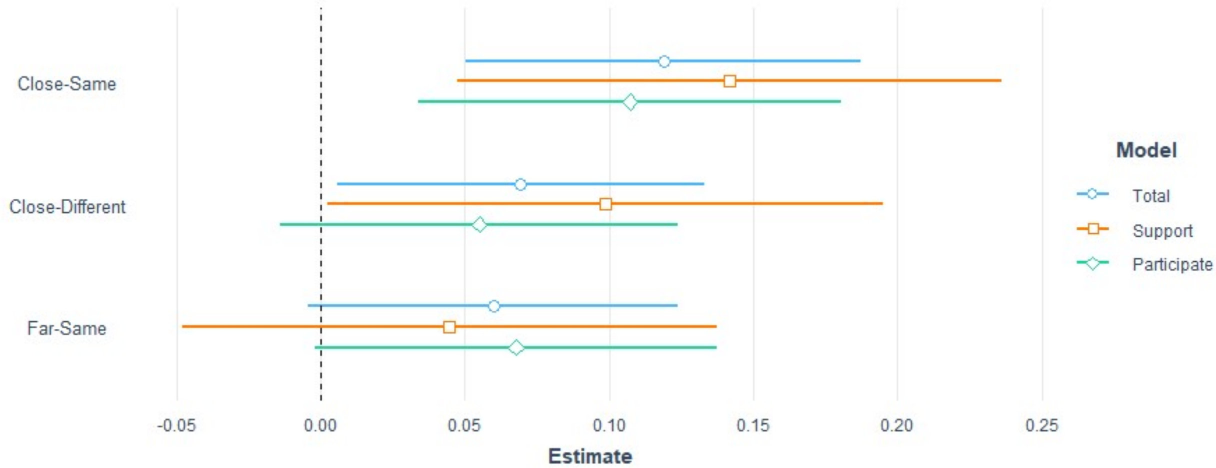


FIGURE 3.5. The Effect of Treatment Assignment on Protest Attitudes.

The results for each individual outcome variable, presented in Appendix Table A.G.9, show that receiving a treatment with protesters of the same identity did increase the likelihood of posting about the protest on social media, but did not significantly impact responses to the other questions. In contrast, when told the protests were occurred nearby, individuals were significantly more likely to report higher levels of support for both the protest movement and protesters and displayed an increased willingness to participate in a similar protest.

In Appendix B.6, I include the results of an analysis looking at heterogeneous treatment effects across different identity features. This analysis suggests that the results in Table 3.3 are driven by participants who do not share religious, linguistic, and political affiliations with the majority group. The results of models looking only at these participants match those in my main models, with geographic proximity, rather than shared identity, producing a significant effect on overall protest attitudes and support for the protesters. However, when looking at only participants who are Hindu and support the BJP, the results offer an interesting contrast. The results of these models show that participants who share the majority religious and political identities are in fact more likely to participate in protests led by groups who share their identity. Although these results are only significant at the 90% level, they suggest that members of dominant identity groups react differently to information about protests, responding more to shared a shared identity with the protesters than to proximate geography.

3.8. Conclusion

The proliferation of digital tools, such as the internet and social media, has significantly altered the landscape of social interactions and information dissemination, allowing groups to form connections and share information across vast geographic expanses. This evolution challenges traditional perceptions of how our physical and social worlds intersect, particularly in the context of protest diffusion. In light of these developments, this study critically examined the role of geographic proximity versus various social ties in the spread of protests. The findings underscore that while social ties, particularly linguistic and religious affiliation, play a substantial role in facilitating protest diffusion, geographic proximity still emerges as a predominant factor. This observation suggests that, despite the global reach of digital communication technologies, physical proximity continues to significantly influence the spread of protest.

An intriguing aspect revealed by this research is the interplay between exposure to protest information and the subsequent uptake or engagement with protest activities. The question arises whether the diffusion of protest is primarily driven by mere exposure to information about ongoing protests or whether it depends on how individuals process and react to this information, particularly when they share specific characteristics with the protesters. This distinction is crucial for understanding the dynamics of protest diffusion and the potential impact of shared identities on individuals' willingness to participate in similar actions. Future research should delve deeper into how different social linkages interact and influence protest diffusion, especially considering the varying susceptibilities of individuals to engage in protest activity upon receiving information.

Finally, the India Farmers' Protest, while providing a rich case study, may represent a unique scenario in terms of the actors involved and its rapid spread across diverse social landscapes in India. Therefore, further studies are needed to assess the external validity of these results. Investigating other protest movements with different goals, strategies, and contexts will offer a more comprehensive understanding of the factors influencing the dynamics of protest diffusion.

CHAPTER 4

Incumbency Advantage in Violence: A Global Subnational Analysis

Since 2000, one third of all elections around the world have been affected by election-related violence. Agents of election violence are often rewarded with state patronage or forbearance, which is easier to access for incumbents. On the other hand, legislative incumbents may be less reliant on violence because they have access to other tools to win elections. Does holding a legislative seat increase or decrease the incidence of electoral violence perpetrated by that party? Does the effect of legislative incumbency differ across contexts? We assess the external validity of existing analyses of the effects of local incumbency on pre-election violence by creating a global subnational dataset of constituency-level election results, pre-election violence events, and other characteristics at the local, party, and national levels. We use a close-election regression discontinuity design to test for the effect of legislative incumbency in different types of contexts. Our data shows that local incumbent parties perpetrate more election violence. However, we do not find a significant relationship between legislative incumbency and electoral violence.

4.1. Introduction

All but a handful of countries in the world have held elections in the past ten years (Hyde and Marinov, 2012). Elections occur across a wide range of regime types, including long-standing democracies, closed autocracies and hybrid regimes that combine elements of democracies and autocracies. As elections have become widespread, so has political violence carried out in connection with electoral competition. Between 2000 and 2020, 36% of all legislative elections around the world were affected by pre-election violence.[1] In addition, recent episodes and support for election violence in democracies considered "consolidated" like the United States has brought renewed interest to the question from a broader set of political scientists and policymakers (Kleinfeld, 2021; Kalmoe and Mason, 2022; Westwood et al., 2022).

Election violence has deep and long-lasting consequences on democratic outcomes such as the perceived legitimacy of elections (Burchard, 2015), political participation (Bekoe and Burchard, 2017; Burchard, 2020; Bratton, 2008; Condra et al., 2018), economic growth (Doctor and Bagwell, 2020), and future violence (Höglund, 2009). It undermines the basic principles of democratic competition in a highly public and shocking way. We define election violence as "coercive force, directed toward electoral actors and/or objects, that occurs in connection with electoral competition, where 'coercive force' includes threats, unlawful detention, forcible curtailment of movement or displacement, and attacks that cause actual bodily harm" (Birch, 2020, 6). We focus on pre-election violence, which can influence election results through a range of channels, including by dissuading targeted groups from turning out (Collier and Vincente, 2014; Rauschenbach and Paula, 2019), increasing identity-based voting (Wilkinson, 2006), and facilitating electoral fraud (Birch, 2020).

Despite the importance and prevalence of election violence, there are still a number of open questions about how and where it is carried out. How is pre-election violence targeted in contentious elections? When is election violence carried out directly by parties, and when is it outsourced to independent actors or organizations like armed militias or organized criminal groups? How does the electoral context, including the capacity of political parties and their susceptibility to punishment for using violence, shape the incentives and ability to use election violence?

In this paper, we examine how local legislative incumbency shapes the incentives and ability of parties to carry out pre-election violence. Local partisan elites play a critical role in organizing violence, which is typically carried out by violent actors who are coordinated and incentivized with violent rhetoric, patronage, or policy benefits. We theorize that local incumbents have differential access to the platforms and resources that facilitate the organization of violence, especially when they are from the national ruling party. Local incumbents may also have stronger incentives to remain in office than challengers have to take it, due to loss aversion and transition costs.

[1] According to NELDA, 376 out of 1046 legislative elections held since 2000 are characterized by "significant violence involving civilian deaths immediately before, during, or after the election". The ECAV data (2000-2012) provides a similar figure: 206 out of 645 elections (32%) experienced election violence.

We test this theory using a close elections regression discontinuity design using subnational data from 36 pairs of elections in majoritarian legislative electoral systems between 2000 and 2020.[2] One of the overarching goals of this project is to test the generalizability of existing findings on the targeting and organization of election violence across a wider range of violent elections. The first step in this project was therefore to systematically collect geolocated subnational data on as many violent elections as possible. Our current set of cases cover a broad array of elections in countries that span the world's political, social, and physical geography. These include several African countries which have typically been the focus of the literature, as well as countries that that been underrepresented in the literature such as wealthy democracies like the United States and countries in South and Southeast Asia where election violence is prevalent. While we are only able to find data on a minority of violent elections between 2000 and 2020, this more representative dataset allows us to examine how contextual factors like the quality of liberal democracy, level of state capacity, or presence of ongoing conflict might shape the effects of incumbency on election violence.

In our preliminary analysis, we do not find a significant relationship between local incumbency and election violence. While election violence is more often perpetrated by local incumbents in our cases, the results of our regression discontinuity analysis do not indicate a clear connection between incumbency and election violence. Instead, we find a consistently null effect for the relationship between incumbency and election violence. In our results section, we present our findings and discuss a strategy for improving the scope and quality of our data for future iterations of this project.

We build on a rich set of studies that investigate where election violence occurs. Many of these have argued that election violence occurs in relatively competitive districts, including those with closer previous vote margins (Wilkinson, 2006) and those with more equally sized ethnic or political groups (Chacón, Robinson and Torvik, 2011; Müller-Crepon, 2020). Many of the empirical tests of this theory, however, come from a small number of cases including most prominently India and Kenya. While there have been notable attempts in recent years to test theories of election violence targeting across a number of countries in a single region (Wahman and Goldring, 2020; Müller-Crepon, 2020), these have generally been focused on sub-Saharan Africa. It is an open question

[2] By "pairs of elections" we mean a previous election that determines incumbency and then an election that is affected by violence.

whether findings from these well-studied cases might extend to other regions with relatively high levels of election violence like South Asia, East Asia and the Pacific, and the Middle East and North Africa. It is even less clear whether findings from these well-studied cases might inform our understanding of election violence in higher-income cases like the U.S. election in 2020. This project attempts to fill that gap by studying how pre-election violence targeting differs across the various political systems where it occurs. Doing so, we build on recent attempts to meta-analyze empirical tests of theories in comparative politics (Zhukov, Davenport and Kostyuk, 2019; Dunning et al., 2019; Blair, Christensen and Rudkin, 2021).

Our study also speaks to a large literature on the effects of incumbency on political campaigning and reelection. In relatively established democracies, a number of studies have identified consistent patterns of pro-incumbent bias in election results (Trounstine, 2011; Fouirnaies and Hall, 2014). In other contexts like India, however, voters may actually be biased against incumbent candidates (Lee, 2020). We most directly build on studies that have found that national incumbents are more likely to perpetrate election violence (Hyde and Marinov, 2012; Taylor, Pevehouse and Straus, 2017; Birch, 2020). By investigating how incumbency affects election violence at the local level, we help unpack the mechanisms through which those macro relationships might operate. We also build on existing attempts to assess the external validity of incumbency effects beyond Western democracies (De Magalhaes, 2015; Lee, 2020), and generalize single-country studies of the effects of incumbency on violence (Fergusson et al., 2021; Nellis and Siddiqui, 2018).

4.2. Theory

Political parties and candidates have a range of different means to compete in elections. Candidates and parties can campaign to persuade voters that they are the better candidate, or to mobilize their supporters to turn out to vote. However, in many elections, parties also use anti-democratic means to win elections, including clientelism, electoral violence, and fraud. In the words of Wilkinson and Haid (2009, 3), election violence is a "particularly brutal form of campaign expenditure." Most of these strategies require some degree of local infrastructure, such as canvassers to run get-out-the-vote (GOTV) campaigns, local activists to organize rallies, or various types of brokers to facilitate clientelism (Mares and Young, 2016). Election violence is also typically carried out through local infrastructure, such as local party structures, police, military bases, or militia (Birch, Daxecker and Höglund, 2020).

Parties who want to campaign using election violence must coordinate and incentivize their associated violent actors. This can be achieved using a variety of tools, including rhetoric, patronage, forbearance, and direct command. Local incumbency may increase the ability of parties to inspire and incite affiliated violent actors to act on their behalf. During the 2008 election in Zimbabwe, although incumbent ZANU-PF candidates were rarely named as the direct perpetrators of violence, they played a pivotal role in organizing violence carried out by ZANU-PF supporters, independence "war veterans", youth militia, and the army. Particularly in the ruling party's stronghold regions, ZANU-PF candidates would hold "re-education meetings" where candidates would call their supporters to use violence or oversee public acts of violence carried out by other actors. In some districts, these incumbent-led re-education meetings set off campaigns of violence against opposition voters and candidates (Watch, 2008).

Local incumbents may even be able to use election violence as part of a campaign to persuade voters that they are the better party in a way that challengers cannot. Most surveys show that voters do not like election violence (Rosenzweig, 2021), or that at best the supporters of violent parties are indifferent to it (LeBas and Young, 2023). However, in some circumstances, incumbents may be able to use election violence and repression more generally to signal hardline policies that key constituencies value. When governments campaign on law and order or control of economic or religious dissidents, repression against groups perceived as threatening can be interpreted by their supporters as good policy. In Chile, for instance, Pinochet may have used localized violence against left-wing activists to escalate the feeling of threat and convince the public that his reelection was necessary to maintain public order (Esberg, 2020). In Egypt after the the 2013 coup, the military government used a violent crackdown in part to legitimate its claims to be the best option to protect secular groups from the threat of the Muslim Brotherhood (Lachapelle, 2022).

Incumbents are also better positioned to use patronage, such as preferential access to land or government services, to reward violent agents (Boone, 2011; Klaus, 2020). For example, in Zimbabwe since the 2000s, ZANU-PF has motivated key military elites to provide support to ZANU-PF's election campaigns through a potent mix of ideological appeals and access to patronage such as diamond rents or state-owned enterprises (Tendi, 2013; Mangongera, 2014). At a more local level, violent actors like war veterans and soldiers would use resources controlled by local politicians, like government vehicles and fuel (Watch, 2008). In post-WWII Sicily, the mafia appears to have provided electoral support to the Christian Democratic Party in exchange for preferential access to construction contracts facilitated by incumbents (De Feo and De Luca, 2017).

Finally, local incumbents can enable aligned violent actors to operate by protecting them from the normal punitive actions of the state. This can either be used to lower the cost of participating in election violence, or to provide a policy benefit to violent actors in the form of forbearance (Holland, 2016). In India, for example, local incumbents at various levels of government can decide whether to let partisan or identity-based violence escalate, or to use law enforcement to stop it (Wilkinson, 2006). In Zimbabwe, after the military killed six demonstrators following the 2018 election, a commission of inquiry was organized but no one in the army was ever punished (Amnesty International, 1 Aug 2019). Incumbents can also allow violent actors to create their own patronage flows, for instance by running extortion rackets. In Kenya's elections in the early 2000s, KANU politicians seem to have allowed the ethno-religious group known as the Mungiki to extort public transportation routes in exchange for campaign support (Kagwanja, 2003; Truth, Justice, and Reconciliation Commission, 2008).

Alternatively, there are reasons to expect that local challengers might perpetrate more election violence. If incumbency confers advantages in other areas of campaigning like patronage or persuasion, then incumbents may be able to rely more heavily on these preferred strategies and less on violence. Narrow losers may in turn rely more on violence to compete with their stronger incumbent opponents. In Colombia from 1990 to 2006, paramilitary groups perpetrated more violence when the right-wing candidates they were aligned with narrowly lost elections (Fergusson et al., 2021). It is possible that in some contexts legislative incumbency does not confer access to resources like a platform for violent rhetoric, financial flows that can be used as patronage, or the ability to shield violent actors from prosecution. This may be the case if power over those resources is controlled by other political actors (local executives like mayors, or national executives) or if certain candidates (such as those from the ruling party) have access to those resources regardless of legislative incumbency.

4.2.1. Within-Case Heterogeneity: Control of the National Executive

To the extent that our theory is correct, we expect that the effects of local incumbency should be particularly pronounced for certain types of parties. First, we expect that the effects of local incumbency should be strongest for parties that also hold the national executive. Many of the ways that local incumbency may facilitate the incentives and ability to perpetrate election violence may be heightened when the local incumbent is from the party of the national executive. Local incumbents from national opposition parties may have more access to some resources such as constituency development funds (Ochieng'Opalo, 2022), but many resources that can be used in patronage exchanges such as major social policy programs are controlled by national executives. National executives may selectively decide whether or not to allow local incumbents to politicize social policy flows. In Zimbabwe, legislative incumbents from ZANU-PF get access to a wide range of state resources that they can use to campaign, which can be used in violent and non-violent forms of campaigning, while opposition incumbents do not (Rusinga, 2021). The ability of incumbents to signal hardline policies by organizing pre-election violence – to the extent voters view certain types of pre-election violence positively – may also be strongest when the local and national incumbent are from the same party and credit can clearly be attributed.

By contrast, when parties that represent minority or subaltern groups that have been excluded from power win seats, they may be more willing or able to prevent violence. In India, for instance, districts that incorporated more members of subaltern groups into political parties in the 1960s experienced less Maoist violence in subsequent decades (Chandra and Garcia-Ponce, 2019). Particularly in countries with ongoing civil wars, potential fighters may be less likely to join insurgent groups when they see that they are able to win local office.

To the extent that narrow losers are more likely to use violence, the effects of narrowly losing an election may still be stronger for national incumbents. In Colombia in the 1990s and early 2000s, Fergusson et al. (2021) show that when previously excluded left-wing parties narrowly win local office, they are more likely to be victimized. In this case, the historic national incumbents have a comparative advantage in violence due to strong ties between the official security forces and paramilitary groups and the ability to pass legislation pardoning the perpetrators of violence (Acemoglu, Robinson and Santos, 2013).

4.2.2. Across-Case Heterogeneity: Conflict, State Capacity, and Quality of Democracy

The effects of incumbency on violence may also vary in important ways across different contexts. First, the effects of local incumbency could be shaped by whether or not there is an active or recent conflict. Conflict – whether intra- or international – is associated with a proliferation of violent actors in society. After civil wars, combatants both for and against the government may be coopted by political parties to perpetrate electoral violence (Christensen and Utas, 2008; Sterck, 2020). American veterans of the Vietnam war have also been linked to violent white nationalist and right-wing paramilitary groups (Belew, 2018). The pools of ex-combatants left behind by conflict may serve as experienced, networked, and radicalized agents willing to perpetrate violence on behalf of a political party they support.

How might the existence of violent actors mobilized by recent or ongoing conflict condition the effects of incumbency? To the extent that incumbent politicians are truly necessary to inspire, organize, and incentivize violence at a local level, then the effect of incumbency could be even stronger in conflict-affected contexts. In Burundi, for example, the 2010 elections took place with approximately 30,000 recently demobilized combatants in the country. The largest group of 12,000 ex-combatants had demobilized from the CNDD-FDD, led by the incumbent president Pierre Nkurunziza. The second largest group of ex-combatants, demobilized from the FNL-Palipehutu, was associated with the party of Nkurunziza's main challenger for the presidency (Colombo, d'Aoust and Sterck, 2019). According to Human Rights Watch, much of the violence before Burundi's 2010 elections was perpetrated by ex-combatants who had joined the parties' youth wings, particularly of the incumbent CNDD-FDD. Ex-combatants, who had few employment opportunities in the wake of civil war, were incentivized with party and state funds (Human Rights Watch, 2010). The case of Burundi in 2010 illustrates how the combination of access to state resources through incumbency and ex-combatants willing to perpetrate violence enabled severe and wide-spread election violence.

A second contextual factor that may shape the effect of incumbency on election violence is state capacity. We adopt a procedural conceptualization of state capacity, where a high capacity state is one that is impartial, professional, and independent (Rothstein and Teorell, 2008; Fukuyama, 2013). In other words, in high capacity states, politicians cannot divert resources into campaign expenditures, or influence legal processes to protect their allies from prosecution. In high-capacity states, incumbency should not affect election violence, because state resources are shielded from incumbents. In low-capacity states, by contrast, incumbents should be more capable of using state resources to amplify their political messages, incentivize aligned violent actors, and shield those who perpetrate election violence on their behalf from prosecution.

Finally, the effect of incumbency may depend on the national quality of democracy. In more democratic systems, voters who dislike violence are more capable of punishing violent politicians at the polls. Despite some notable exceptions of politicians who seem to thrive despite public reputations for political violence, most voters seem to dislike or at best be indifferent to violence perpetrated by their preferred candidate (Banerjee et al., 2014; Rosenzweig, 2021). To the extent that more democratic elections enable voters to vote out politicians who perpetrate violence, the overall level of election violence should be lower in more democratic contexts. The difference between incumbents and challengers should also be reduced if incumbents are in fact perpetrating more election violence, or if incumbents are particularly blamed for election violence by voters.

4.3. Methodology

The core contribution of this project is to test theories about how pre-election violence is targeted in a way that prioritizes generalizability and explicitly considers how context might shape targeting. Testing a theory about how national context shapes subnational targeting of election violence requires that we have disaggregated local data at a global scale.

Several recent data releases have made this possible for a large number of countries in the world. We start by identifying the universe of cases of contentious elections using the NELDA data.[3] Most importantly, we draw on the recent expansion of the ACLED conflict events data to regions other than sub-Saharan Africa, and the recent expansion of the CLEA data on constituency-level election returns. Section 4.3.1 describes how we identified the universe of contentious elections since 2000, and then built a dataset that has constituency-level election results and election violence events for

[3]In a few cases, such as the 2019 Indian election, we have found that the NELDA data seems to undercount contention. We are working on validating the NELDA coding and expanding the universe of contentious elections.

as many of the relevant cases as possible. Section 4.3.2 describes how we identify election-related violence, and associate it with particular political parties, from all events and actors coded in the ACLED data.

We apply a close-election regression discontinuity design to this data to identify the effects of different types of incumbents on the level and type of election violence in a constituency. Section 4.3.3 describes our estimation strategy and the plausibility of its identifying assumptions in this particular application.

4.3.1. Building a More Representative Subnational Dataset

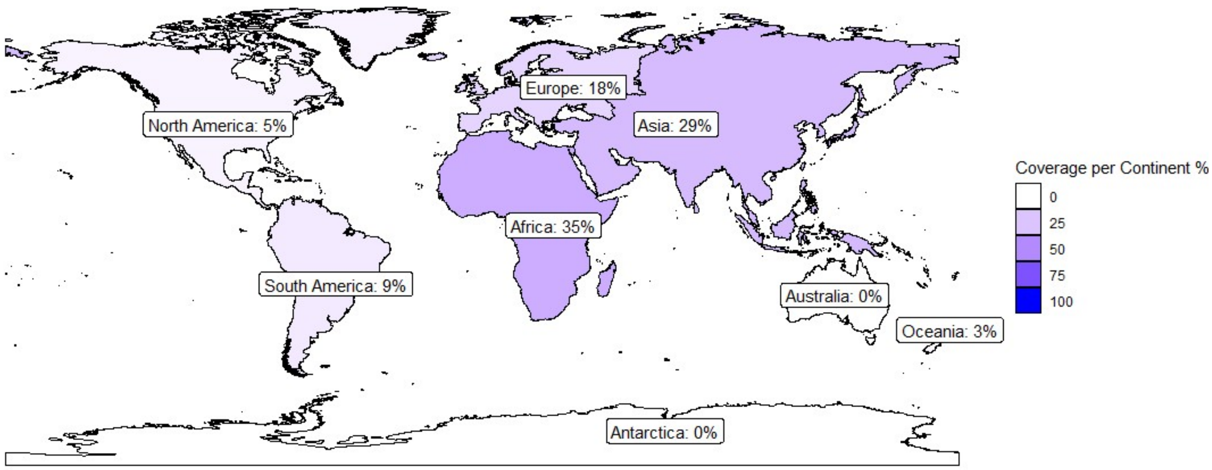
Our goals in this study are to test the generalizability of existing findings on how pre-election violence is targeted, and to understand how contextual factors affect the way that election violence is targeted. An important first step is therefore to identify the universe of elections in which violence occurs. To do this, we rely on version 6.0 of the NELDA dataset, which identifies all direct election events between 1945 and 2020 (Hyde and Marinov, 2012). We focus on legislative elections (excluding executive elections and referenda) because these enable us to test predictions about geographic targeting across districts. This leaves us with 1046 elections between 2000 and 2020.

We also restrict our focus to elections that NELDA codes as involving some level of violence because we are interested in explaining not whether election violence occurs but how it is targeted in elections where it is used by at least one party. We thus restrict our sample to legislative elections in which NELDA coders identified some opposition harassment (NELDA15), violence against demonstrators (NELDA31), or significant violence (NELDA33). This results in 376 contentious elections 2000-2020. Figure 4.1 shows how the post-2000 legislative elections coded by NELDA as contentious are distributed across world regions. About 35% of contentious elections have been in Africa, 29% in Asia, 18% in Europe, 9% in South America, and 5% in North America.

In this version of the analysis, we focus on the 223 legislative elections in our cases with single-member district electoral systems or mixed majoritarian systems.[4] From this universe of cases, we

[4]We may expand our dataset to elections with proportional representation where we can identify thresholds for additional seats in the future.

FIGURE 4.1. Contentious elections by world region (NELDA)



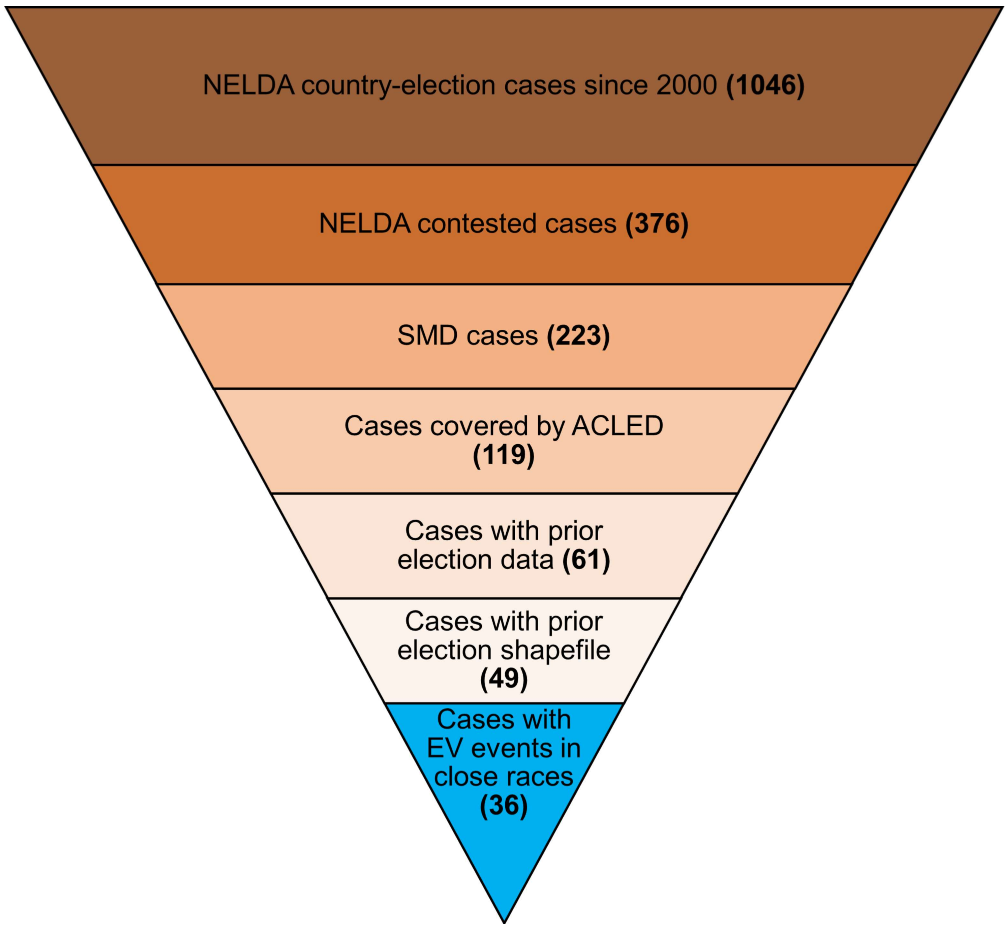
are only able to investigate the targeting of election violence in cases where we have consistent, geolocated data on violent events. For our primary analysis we rely on the ACLED data, which we discuss in more depth in the next section. The coverage of the ACLED data varies greatly by region. Data is available starting in the late 1990s/early 2000s for sub-Saharan Africa, but for most of the rest of the world coverage begins between 2016 and 2018. This leaves us 119 cases out of the 223 contentious SMD elections since 2000 that are covered by the ACLED data.

In addition to data on violent events, we also need constituency-based election results for the prior legislative elections in order to test for the effects of legislative incumbency. Between the Constituency-Level Elections Archive (CLEA) and other unofficial efforts to publish election results such as Adam Carr's Psephos website, we were able to find constituency-level results for 61 out of the 119 SMD contentious elections for which we had ACLED data.[5] We could find or build maps of the constituency boundaries for 49 of those cases. The final criteria for inclusion in our dataset is that at least one of the constituency-level races is decided by a slim margin. We use several thresholds ranging between 1 and 5 percentage points to operationalize a close election in our analysis. We lose 13 elections for which we have the rest of the data by focusing only on close constituency-level races. This leaves us with 36 elections in our final dataset. Figure 4.2 shows how

[5]CLEA curates clean constituency-level results from official sources; the Psephos website will sometimes use media or NGO sources. Appendix Figure A.G.4 shows that, for seven diverse cases, the CLEA and Psephos results for the winner and runner-up are correlated at $\rho > 0.95$.

each of these inclusion criteria affect the number of elections that we are able to analyze. Appendix C.5 presents the list of all legislative election events 2000-2020 in the NELDA data with details on whether or not they are included in our data, and if not, why they are excluded.

FIGURE 4.2. Number of 2000-2020 legislative elections based on each inclusion criteria



We pool our country-election cases according to different contextual factors that according to our theory should impact the relationship between legislative incumbency and the perpetration of election violence. The three grouping we use are: regime type, state capacity, and conflict experience. The data for regime type comes from V-Dem's Regimes of the World variable. We code countries with a score of 0 (closed autocracy) or 1 (electoral autocracy) as autocracies and countries with a score of 2 (electoral democracy) or 3 (liberal democracy) as democracies. The data for state capacity comes from the World Bank's Government Effectiveness variable in the Worldwide Governance Indicators, averaged for each country over the past five years. We consider countries to be low capacity if their score is between -2 and -0.5, medium capacity if their score is between -0.5 and 0, and high capacity if their score is between 0 and 2. The data for conflict experience comes from UCDP's Armed Conflict data set. We define countries as having active conflict if they were a major participant in an interstate or intrastate conflict in the same year as their focal election; countries are defined as being post-conflict if they were a major participant in an interstate or intrastate conflict in the five years prior to their focal election; countries are defined as being in peacetime otherwise.

Table 4.1 shows the distribution of contentious elections included in our dataset and in the NELDA dataset (2000-2020) by region, regime type, state capacity, and conflict context.

Table 4.1. Distribution of Contentious Elections in Our Data and Nelda for Different Categories

Category	Our Cases	Nelda
Africa	58%	35%
Asia	28%	29%
Europe	6%	18%
North America	6%	5%
Oceania	0%	3%
South America	3%	9%
Autocracy	69%	70%
Democracy	31%	30%
Low Capacity	58%	52%
Medium Capacity	28%	24%
High Capacity	14%	23%
Peacetime	58%	61%
Post Conflict	11%	11%
Active Conflict	31%	28%

4.3.2. Identifying Election Violence

The next key step in our methodology is to identify election violence events from all of the events that ACLED records. We build on Birch (2020) to conceptualize electoral violence as "coercive force, directed toward electoral actors and/or objects, that occurs in connection with electoral competition" (8). Coercive force includes violent attacks such as murder and assault, but also events that are coercive but do not necessarily result in physical bodily harm such as unlawful detention and threats. This conceptualization of election violence is particularly relevant for a study focused on how election violence is targeted.

This conceptualization excludes violent events that are not connected to electoral competition, such as crime events or civil war battles, even if they occur in close proximity to an election. As shown in Table 4.1, about one in four elections in the past twenty years has been carried out during an active conflict. States and other armed actors in these environments might use violence to influence the contenders for political office and their supporters, while at the same time fighting rebel, criminal, or identity-based organizations militarily. While events that are not electorally motivated may very well affect election outcomes, they are not the focus of this study.

We identify election violence in ACLED using several steps:

(1) Remove events that do not involve violence, including those with the event subtypes peaceful protest, agreement, change to group activity.

(2) Identify whether the actors or associated actors are political parties.[6]

(3) For cases with a small number of ACLED events we were able to manually check whether remaining events are associated with the election using the ACLED notes. However, for cases with over 500 events, we defined a set of key words which we used to filter the ACLED notes for electoral violence. These key words included a generalized set of election-related terms such as election, voter, and ballot, as well as a set of country-specific terms such as the name of the country's legislative body and the political parties active in the country.

[6]We did this by compiling a set of political party names associated with each country in our data from Party Facts, CLEA, and Psephos. We then identified ACLED events where the actors or associated actors matched the list of political parties active in that country.

After running this criteria-based coding, we validated up to 100 events per election manually using a team of undergraduate coders. We randomly selected 100 ACLED events within 6 months of each election in our dataset selecting 50% of the events from those that we had coded as election violence and 50% from those that we had coded as not election violence. Overall, our manual coders agreed that 79% of the events coded as election violence were in fact election related based on their reading of the event description and background research into the case. They agreed that 92% of the events coded as not election violence were in fact not election related.

We identify which party, if any, is associated with each violent event using both the ACLED actor data and a case-specific mapping of alliances between parties and violent actors in the ACLED data. Some alliances are already coded into the ACLED data, which identifies violent actors and "associated actors." In other cases, we draw on external datasets on pro-government militias (Carey, Mitchell and Lowe, 2013), militant groups' electoral participation (Matanock, 2016), and government relations with armed actors (Otto, Scharpf and Gohdes, 2020).

After a first wave of research that considered all violence around elections to be election related (Daxecker, 2012; Goldsmith, 2015), there have been several notable recent efforts to isolate election violence from other violent events. We do the same using the ACLED data, which we favor primarily because it includes not only international newspapers but local media and NGO reports, is human coded with consistent information on fatalities and actors, and has recent global coverage. We will ultimately compare our data to three other recent election violence datasets: the Electoral Contention and Violence (ECAV) data (Daxecker, Amicarelli and Jung, 2019), the Deadly Electoral Conflict Dataset (DECO) (Fjelde and Höglund, 2022), and the Countries at Risk of Election Violence (CREV) data (Birch and Muchlinski, 2020). All three have made notable advances in parsing election-related events from all violent events around an election. We ultimately decided to apply a similar methodology to the ACLED data instead of using either ECAV, DECO, or CREV for several reasons, primarily related to the total pool of candidate events that they consider. The ECAV data codes events from three international newswires – an impressive effort but one that we suspect exacerbates the urban bias found in many media-based violent events datasets (Von Borzyskowski and Wahman, 2021). The DECO data takes its pool of potential election violence events from the Uppsala Conflict Data Program Georeferenced Event Dataset (UCDP GED). It is focused only on lethal violence, and its reliance on the UCDP GED data may lead to variation in the quality of its data across countries depending on their conflict status. Finally, the Countries at Risk of Electoral Violence (CREV) dataset uses the Integrated Crisis Early Warning Systems event data, focusing on 101 countries at risk of electoral violence, and then codes ten different types of election violence events using an automated approach (Birch and Muchlinski, 2020). In future iterations of the project we will compare our election violence data coded from ACLED against these other recent data releases to test whether it is more or less concentrated in urban areas and has greater or similar levels of violence in cases where there is overlap in coverage. We may combine our ACLED data with other sources to expand our coverage. Given the known limitations of media-based election violence data, we will also conduct supplementary analyses where we assess the sensitivity of our results to likely reporting bias.

4.3.3. Estimation Strategy

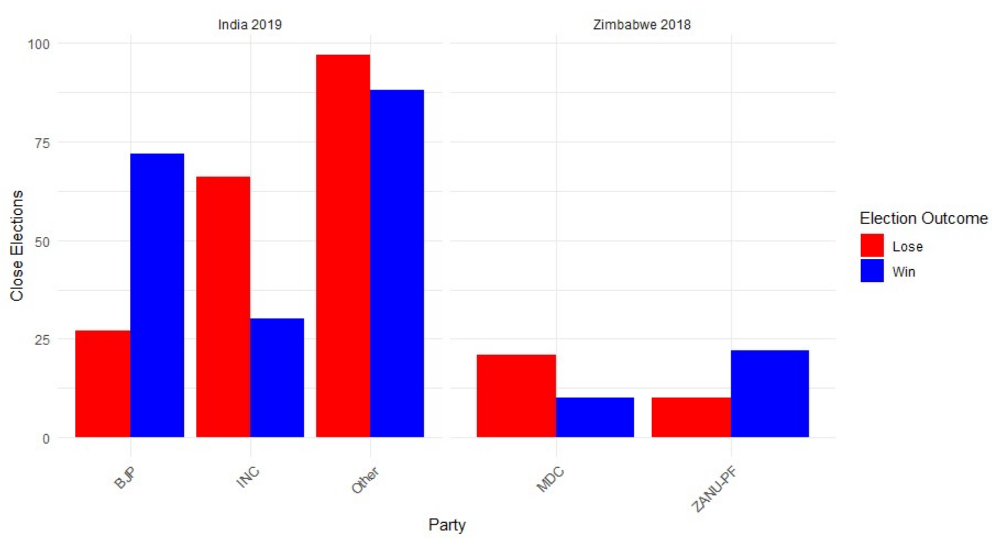
We test whether parties that narrowly win a legislative seat are more likely than parties that narrowly lose to perpetrate election violence during the subsequent legislative election. The electoral outcomes associated with a particular party are potentially correlated with a host of district-level characteristics that can confound the relationship between an electoral victory and the subsequent occurrence of party-led electoral violence. We employ an RD design based on close elections to address this problem. The RD design is well-suited for evaluating how winning an election affects the level of violence used by the winning party in the subsequent election period. Following this design, we find parties with vote margins that fall just above and just below the threshold needed to win a legislative seat, creating two groups with similar potential outcomes that only vary with respect to whether or not they actually won the election. This allows us to identify the effect of winning a legislative seat on the subsequent use of election violence.

Our data consists of districts in which the legislative incumbent narrowly beat the second place candidate. The legislative incumbent and second place candidates were identified as the candidates who received the highest and second highest vote shares in that district. The vote margin for each candidate was then calculated by subtracting the opposing candidate's vote share from that of the focal candidate. We define narrow elections as those with a vote margin within a 5%, 2%, and 1% threshold. The number of districts with narrow elections identified by our different thresholds are listed in Table 4.2. To illustrate this for two cases in our data, Figure 4.3 shows the number of wins and loses in close races for the main political parties contesting the elections in Zimbabwe 2018 and India 2019.

TABLE 4.2. Electoral Races by Bandwidth

	Bandwidth			
Category	All	5%	2%	1%
All Races Pooled	7281	3791	3486	3395
Autocracy	5135	2630	2430	2368
Democracy	2146	1161	1056	1027
Low Capacity	4149	1926	1749	1706
Medium Capacity	2040	1215	1128	1094
High Capacity	1092	650	609	595
Peacetime	3105	1548	1385	1338
Post Conflict	1104	688	666	657
Active Conflict	3072	1555	1435	1400

FIGURE 4.3. Number of Close Elections by Party Outcome



Next, we find the number of election violence events perpetrated by the first and second place parties in these districts. As a result, our data includes a single observation for each district with a close election along with its associated electoral margin and a count of election violence events perpetrated by the winning and losing parties. Table 4.3 shows the distribution of election violence events perpetrated by the first and second place parties for each of our thresholds. Figures 4.4 and 4.5 show the distribution of election violence perpetrated by political parties in close races for the cases of Zimbabwe 2018 and India 2019, respectively.

TABLE 4.3. Number of EV Events Perpetrated by the Winning or Losing Party for Different Bandwidths

	Bandwidth			
Category	All	5%	2%	1%
All Races Pooled	7675	1829	455	105
Autocracy	6718	1713	423	94
Democracy	957	116	32	11
Low Capacity	3010	263	98	78
Medium Capacity	3881	1502	328	14
High Capacity	784	64	29	13
Peacetime	1628	200	48	35
Post Conflict	1024	55	21	5
Active Conflict	5023	1574	386	65

Our identification strategy depends on the assumption that there is no systematic manipulation of electoral results around the winning threshold. McCrary (2008) provides a formal test to check the validity of this assumption in our data by assessing whether the marginal density of the relative vote share received by first and second place candidates is continuous around the winning threshold. We conduct a McCrary test to check for discontinuities in the relative vote shares for national legislative incumbent parties. National incumbent parties represent the most likely offenders for systematic manipulation of electoral results because they may control the conduct of elections and

FIGURE 4.4. Election Violence Perpetrated by Party Close Races in Zimbabwe 2018

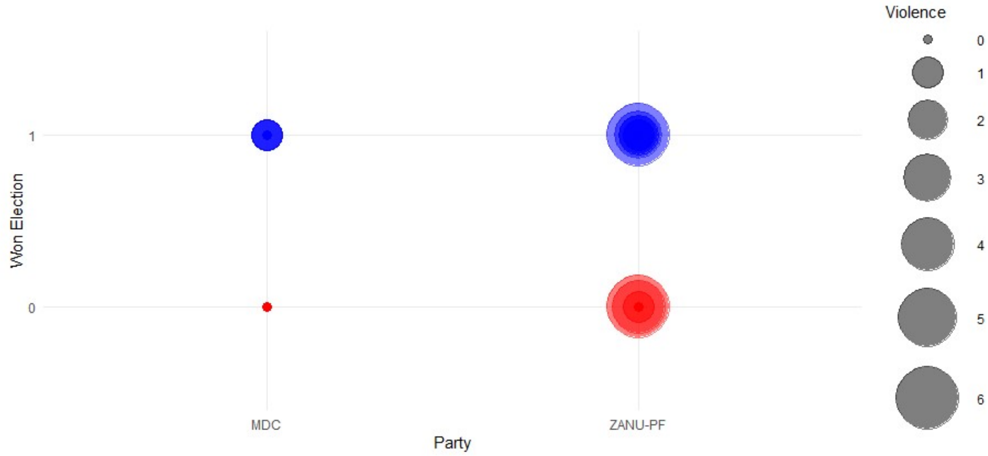
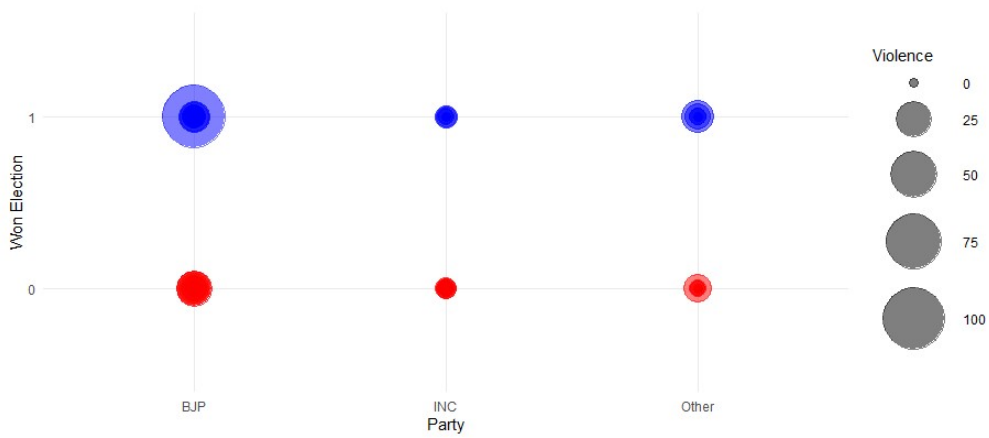
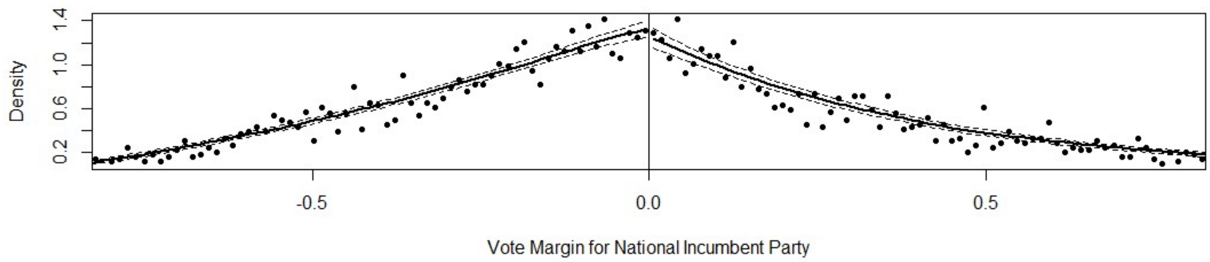


FIGURE 4.5. Election Violence Perpetrated by Party in Close Races in India 2019



distribution of election results. If this is the case, we should see a jump in the density of vote margins associated with candidates of national incumbent parties at the winning threshold. However, the results of our McCrary test (Figure 4.6) show that the density of vote margins received by national incumbent candidates is continuous across the winning threshold, supporting our identification assumption that there is no systematic manipulation of electoral results. We also run McCrary tests to check for systematic manipulation by national executive parties, clientelist parties, and national incumbent parties that use state resources in their electoral campaigns (results in Appendix C.1.1). The results of these tests indicate that elections were not systematically manipulated, except in the case of clientelist parties.

FIGURE 4.6. McCrary Test: Sorting Around the Winning Threshold for National Incumbent Party



Note: Each point represents a bin. Bin size is .0012. Discontinuity estimate (standard error): -0.057 (0.07).

4.4. Results

We estimate the local causal effect of legislative incumbency on the use of election violence using the *rdrobust* package which implements a triangular kernel-weighted local linear regression on observations that fall within the narrow bandwidths of 5%, 2%, and 1%.[7] This analysis tests whether there is a change in the use of election violence at the winning threshold where the party's vote margin changes from negative to positive by estimating a linear regression on each side of the threshold. We also conduct a difference-in-means analysis and third-order polynomial regression (see Appendix A.G.3 and Appendix A.G.4 for results). Finally, we re-run our difference-in-means estimation for our pooled cases using a logged and categorical version of our dependent variable, presented in Appendix A.G.5 and Appendix A.G.6.

In Table 4.4 we present the results of our local linear estimation using different bandwidths to define close elections. Contrary to our expectations, we do not find a statistically significant relationship between legislative incumbency and the use of election violence. While it is possible that legislative incumbency does not affect a party's use of election violence, we plan to investigate this relationship further using future iterations of our data. We think it is likely that our null results stem from noise in our data on party-led election violence. In our current data, we have very few observations of election violence perpetrated by the first and second place parties which

[7]We use triangular kernal weights in our estimation because they give more to observations that are closer to the victory margin threshold.

diminish further as we limit our sample to races with vote margins that are small enough to confidently estimate treatment effects. As a result, our estimates rely on sparse and potentially noisy measurements of our dependent variable. In future iterations of this project, we plan to improve the quality of our data by expanding the cases in our data and refining the procedure used to identify occurrence and perpetrators associated with election violence.

TABLE 4.4. Local Linear Estimates

	Bandwidth		
Category	5%	2%	1%
All Races Pooled	-0.921 (0.204)	0.627 (0.304)	-0.172 (0.435)
Autocracy	-1.308 (0.196)	0.879 (0.31)	-0.248 (0.438)
Democracy	-0.067 (0.324)	0.04 (0.627)	0.007 (0.941)
Low Capacity	0.157 (0.571)	0.091 (0.784)	-0.365 (0.218)
Medium Capacity	-2.795 (0.165)	1.533 (0.28)	0.219 (0.478)
High Capacity	-0.24 (0.352)	-0.371 (0.525)	-0.948 (0.338)
Peacetime	0.027 (0.788)	-0.093 (0.441)	-0.127 (0.392)
Post Conflict	-0.09 (0.641)	0.567 (0.052)	0.522 (0.296)
Active Conflict	-1.973 (0.18)	1.385 (0.29)	-0.274 (0.525)

4.5. Conclusion

Our analysis on the impact of legislative incumbency on pre-election violence has yielded inconclusive results as we did not identify a statistically significant relationship between local incumbency and the perpetration of election violence. This null finding might suggest that the dynamics of incumbency do not universally translate into increased election-related violence, indicating that incumbents may not necessarily leverage violence as a tool for electoral advantage as commonly presumed. However, these inconclusive results could also stem from limitations in data availability.

We faced substantial challenges when compiling this global subnational dataset related to data availability and accuracy. The omission of key data on the election results and incidents of election violence in numerous contentious elections may skew our understanding of the relationship between incumbency and election violence. Recognizing these limitations, we plan to update our analysis as more comprehensive data becomes available. Recent releases of more detailed and geographically diverse election data promise to enhance our understanding of these dynamics. By incorporating this new information, we aim to refine our analysis and potentially reveal patterns that were not previously detectable.

Despite the data limitations in our study, the null results may accurately reflect the lack of a direct effect between incumbency and electoral violence. This outcome could imply that other factors, such as the strength of democratic institutions, the rule of law, societal norms against violence, or international oversight, might effectively mitigate the influence of incumbency on electoral violence. In future iterations of this paper, we will explore these dimensions more thoroughly, enhancing our theoretical and empirical framework to better understand the conditions under which electoral violence emerges.

APPENDIX A

Strategic Disruptions

A.1. Distribution of Political Support

Table A.1. Summary Statistics for Political Support by State

	Mean Support	Min Support	Max Support
Total	**4.10**	**-61.64**	**86.33**
Andhra Pradesh	4.97	-11.89	26.26
Arunachal Pradesh	-3.10	-61.64	49.67
Assam	-9.64	-52.47	35.08
Bihar	-8.65	-47.18	20.49
Chhatisgarh	6.54	-14.89	27.43
Goa	3.15	-3.51	9.284
Gujarat	4.22	-18.32	25.45
Haryana	4.80	-18.59	28.73
Himachal Pradesh	5.75	-13.44	27.07
Jammu & Kashmir	-11.90	-51.65	35.30
Jharkhand	-5.99	-43.57	22.06
Karnataka	-5.38	-56.36	24.76
Kerala	-0.856	-23.19	20.57
Ladakh	-31.44	-51.45	-16.52
Madhya Pradesh	1.13	-25.79	25.79
Maharashtra	-5.26	-43.46	24.24
Manipur	-3.07	-48.62	39.56
Meghalaya	-6.75	-58.18	32.88
Mizoram	4.88	-25.56	25.56
Nagaland	-10.44	-59.10	35.01
NCT of Delhi	20.12	8.04	27.78
Orissa	11.22	-23.28	34.54
Pondicherry	-1.79	-51.94	64.52
Punjab	4.67	-45.55	22.96
Rajasthan	1.87	-25.83	25.83
Sikkim	9.13	-3.83	27.77
Tamil Nadu	59.46	2.267	86.33
Telangana	12.51	-17.91	26.34
Tripura	-2.89	-48.87	19.37
Uttar Pradesh	7.85	-42.62	35.15
Uttarakhand	7.53	-8.72	16.97
West Bengal	17.04	-32.32	37.31

A.2. Identifying and Classifying ACLED Events

This coding procedures used to classify the perpetrators of ACLED events was adapted from a working paper by Lauren Young and Marika Miner titled "A Global Analysis of the Targeting and Effects of Election Violence."

A.2.1. Identifying Disorder Events

I subset all ACLED events to only include those associated with collective action and violent disorder by excluding events with the following sub-event types: Agreement, Change to group activity, Headquarters or base established, Non-violent transfer of territory, Peaceful protest, and Other. Figure A.1 shows the number of violent events that occurred in each state between 2016-2022.

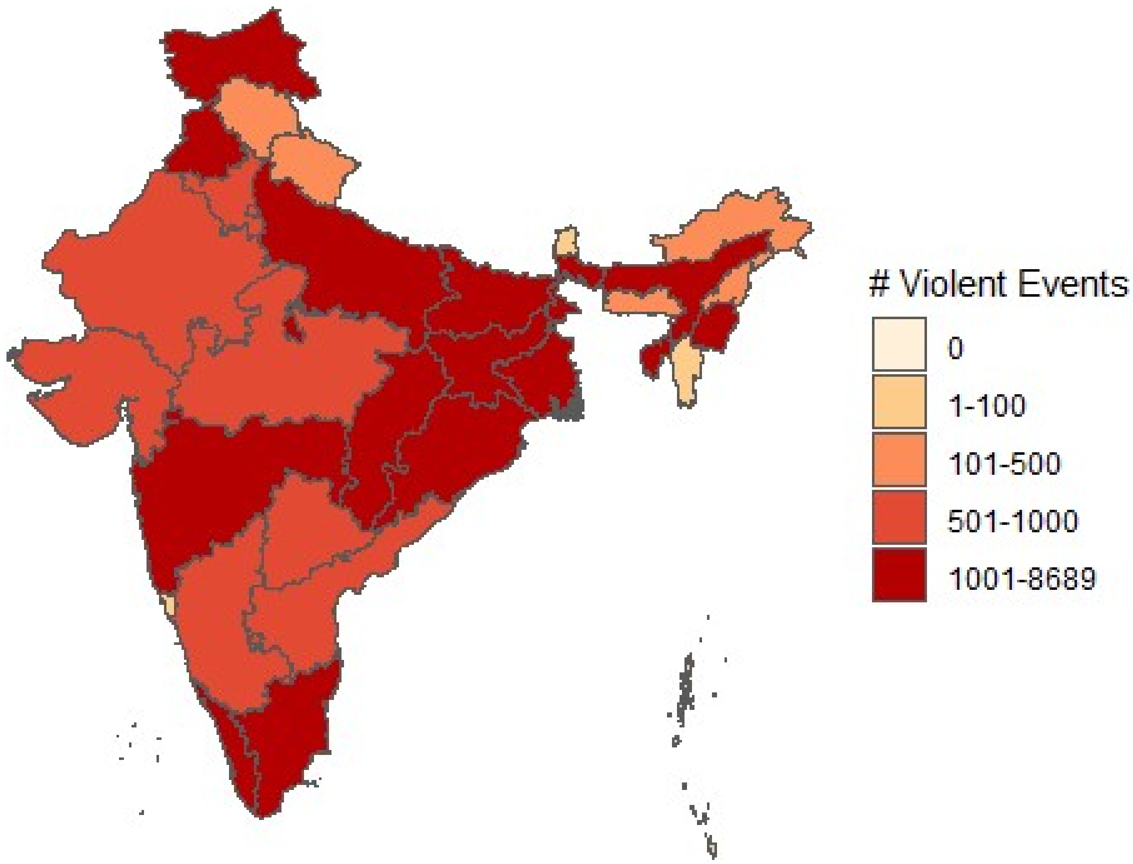


FIGURE A.1. State-Wise Violent Events, 2016-2022

A.2.2. Defining Event Perpetrators

To identify the perpetrators of violent events in the ACLED data I used the event actor and associated actor variables in ACLED, using a sub-event type-specific logic. The identification rules are as follows:

(1) Actor 1 and associated actor 1 are the perpetrators
  (a) Abduction/forced disappearance
  (b) Air/drone strike
  (c) Arrests
  (d) Attack
  (e) Grenade

(f) Looting/property destruction

(g) Remote explosive/landmine/IED

(h) Sexual Violence

(i) Shelling/artillery/missile attack

(j) Suicide bomb

(2) Actor 1 and associated actor 1 and actor 2 and associated actor 2 are both the perpetrators

(a) Armed clash

(b) Excessive force against protesters

(c) Disrupted weapons use

(d) Government regains territory

(e) Protest with intervention

(3) Actor 1 and associated actor 1 and actor 2 and associated actor 2 are both the perpetrators unless actor 2 is coded as civilians (inter2 = 7). When actor 2 is coded as civilians, then actor 1 and associated actor 1 are the perpetrators

(a) Mob violence

(b) Violent demonstration

A.2.3. Identifying Opposition Perpetrated Violence

I identify disorder events that are perpetrated by opposition parties by finding matches between the event perpetrators and the opposition political parties and their affiliates for each state. The set of opposition parties for each state-year consist of all district-level opposition parties in that state identified as those which received the highest district-level vote share in the most recent prior assembly election, when excluding the party of the Chief Minster, during the year the event took place. In order to identify opposition-affiliated groups, I created a list of all district-level opposition parties in India during the study period. I then researched each party to find their affiliated groups, which often include the party's youth wing, women's group, and armed faction. Table A.2 and Figure A.2 show the distribution opposition-perpetrated violent events in each state during the study period.

Table A.2. Summary Statistics for Violent Events by State

	Violent Events	Opposition Violence	% Opposition Violence
Total	**36,380**	**3,019**	**8.3**
Andaman & Nicobar Islands	14	0	0.00
Andhra Pradesh	720	156	21.67
Arunachal Pradesh	238	7	2.94
Assam	1,411	162	11.48
Bihar	1,437	88	6.12
Chandigarh	244	0	0.00
Chhatisgarh	1,251	11	0.88
Dadra & Nagar Haveli	1	0	0.00
Daman & Diu	2	0	0.00
Goa	67	6	8.96
Gujarat	935	49	5.24
Haryana	942	26	2.76
Himachal Pradesh	159	30	18.87
Jammu & Kashmir	8,689	49	0.56
Jharkhand	1,107	33	2.98
Karnataka	621	91	14.65
Kerala	1,119	163	14.57
Ladakh	21	1	4.76
Lakshadweep	0	0	0.00
Madhya Pradesh	672	44	6.55
Maharashtra	1,047	94	8.98
Manipur	1,164	29	2.49
Meghalaya	295	1	0.34
Mizoram	40	1	2.50
NCT of Delhi	696	64	9.20
Nagaland	132	4	3.03
Orissa	1,516	154	10.16
Pondicherry	112	1	0.89
Punjab	1,934	274	14.17
Rajasthan	623	37	5.94
Sikkim	29	12	41.38
Tamil Nadu	1,631	120	7.36
Telangana	606	46	7.59
Tripura	1,082	97	8.96
Uttar Pradesh	2,461	185	7.52
Uttarakhand	256	18	7.03
West Bengal	3,106	966	31.10

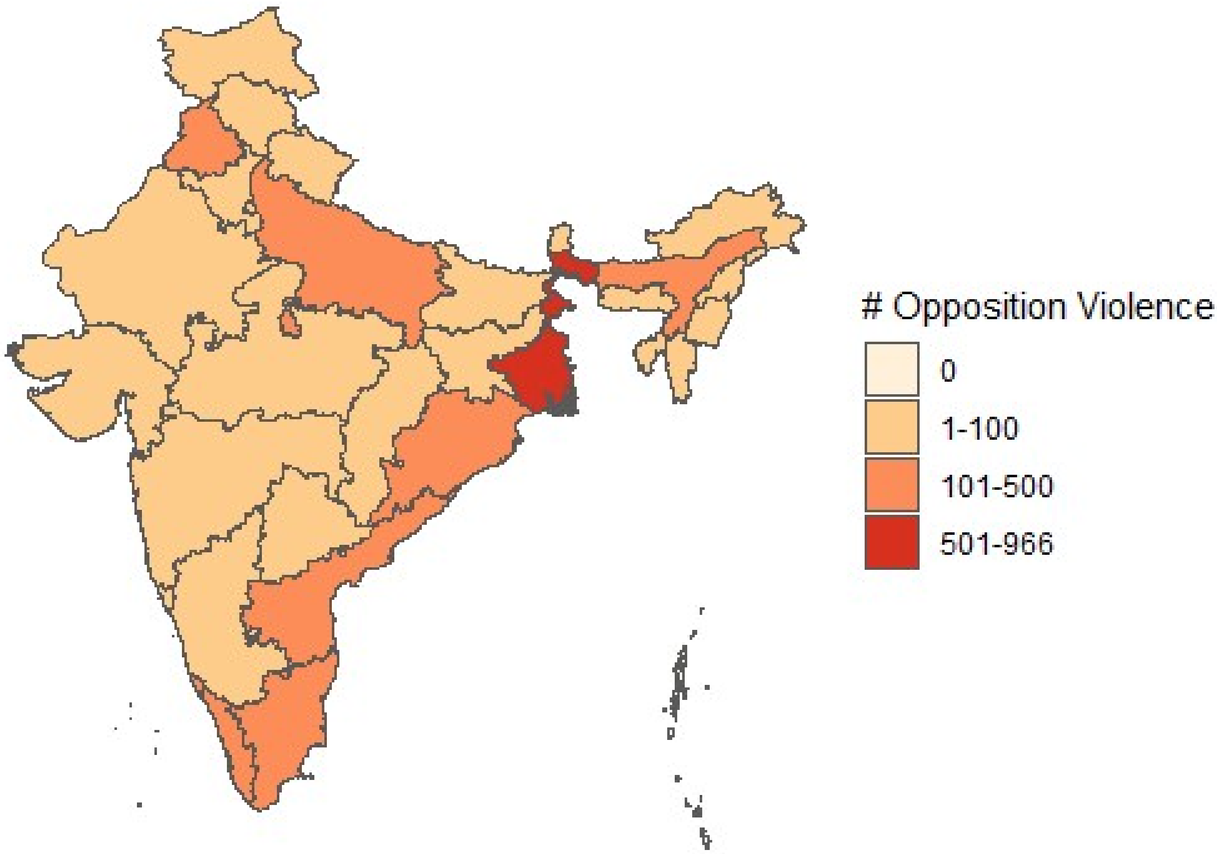


FIGURE A.2. State-Wise Opposition-Perpetrated Violent Events, 2016-2022

A.2.4. Identifying Military-Led Violence

To identify violent events that are perpetrated by the military, I first removed events considered to be protests, riots, arrests, or disrupted weapons use. The remaining events are classified as being military-led if the event perpetrator is coded as Governments and State Security Services (inter = 1). Figure A.3 shows the number of military-led violent events in each state between 2016-2022.

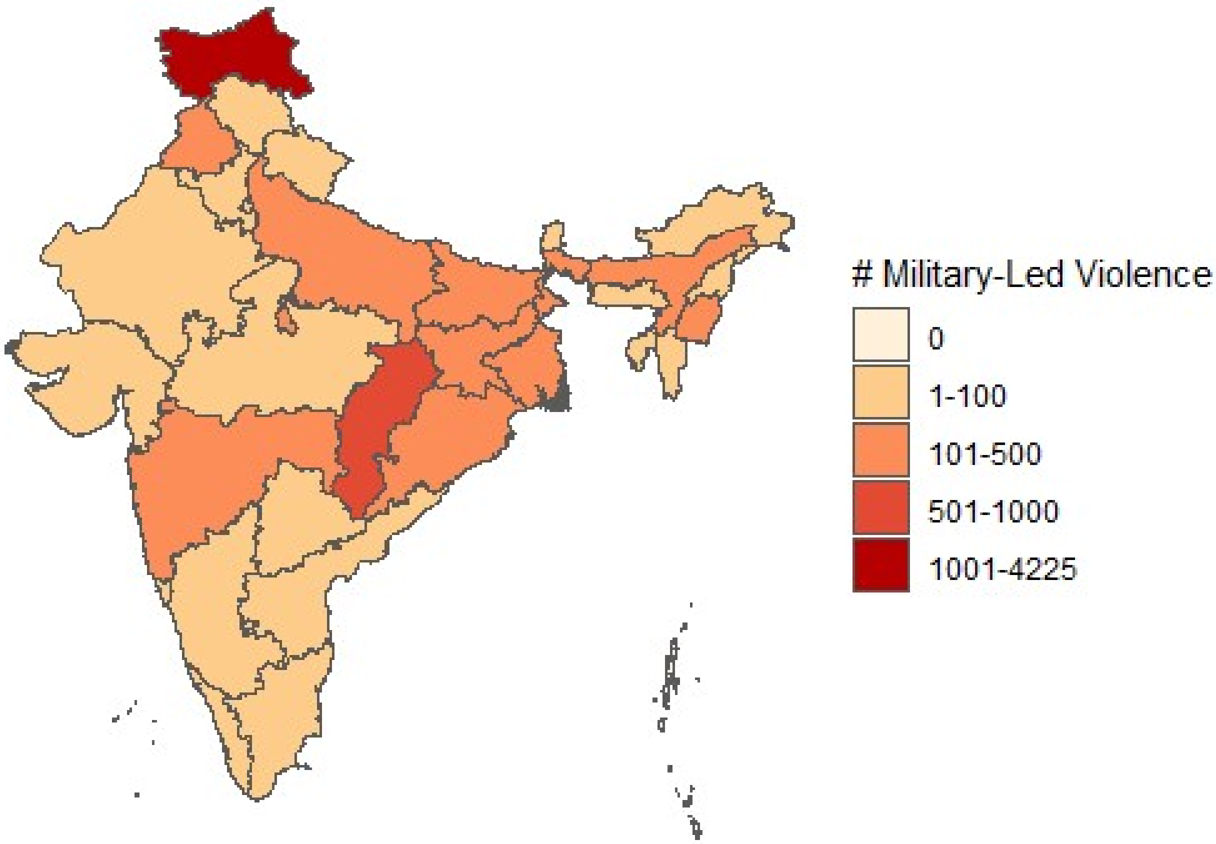


Figure A.3. State-Wise Military-Led Violent Events, 2016-2022

A.2.5. Identifying Physical State Repression

To identify physical state repression, I first removed events considered to be peaceful protests, explosion/remote violence, battles, or disrupted weapons. I then subset these events to those with the following interaction codes: 15 (Military vs Rioters), 16 (Military vs Protesters), 17 (Military vs Civilians). The remaining events are classified as physical state repression if the event perpetrator is coded as Governments and State Security Services (inter = 1). Figure A.4 shows the number of physical repression events in each state between 2016-2022.

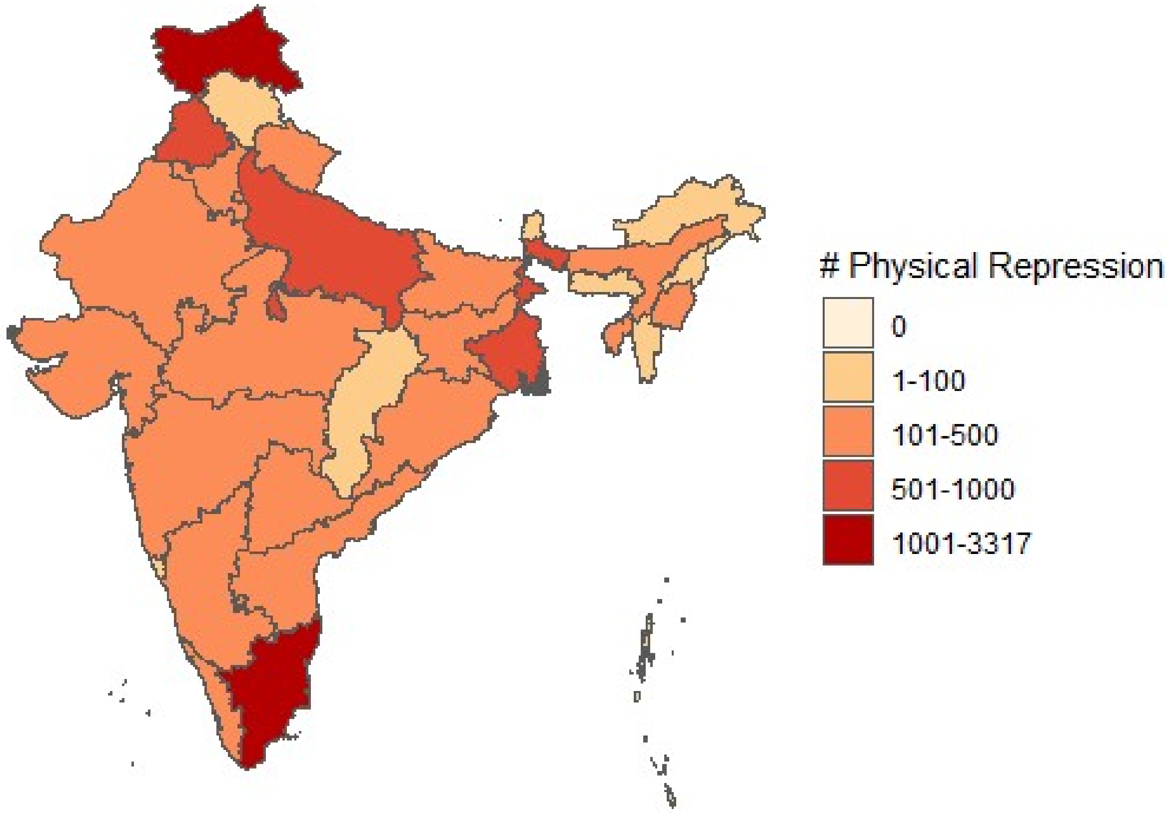


Figure A.4. State-Wise Physical State Repression Events, 2016-2022

A.3. Justifications for Internet Shutdowns in India

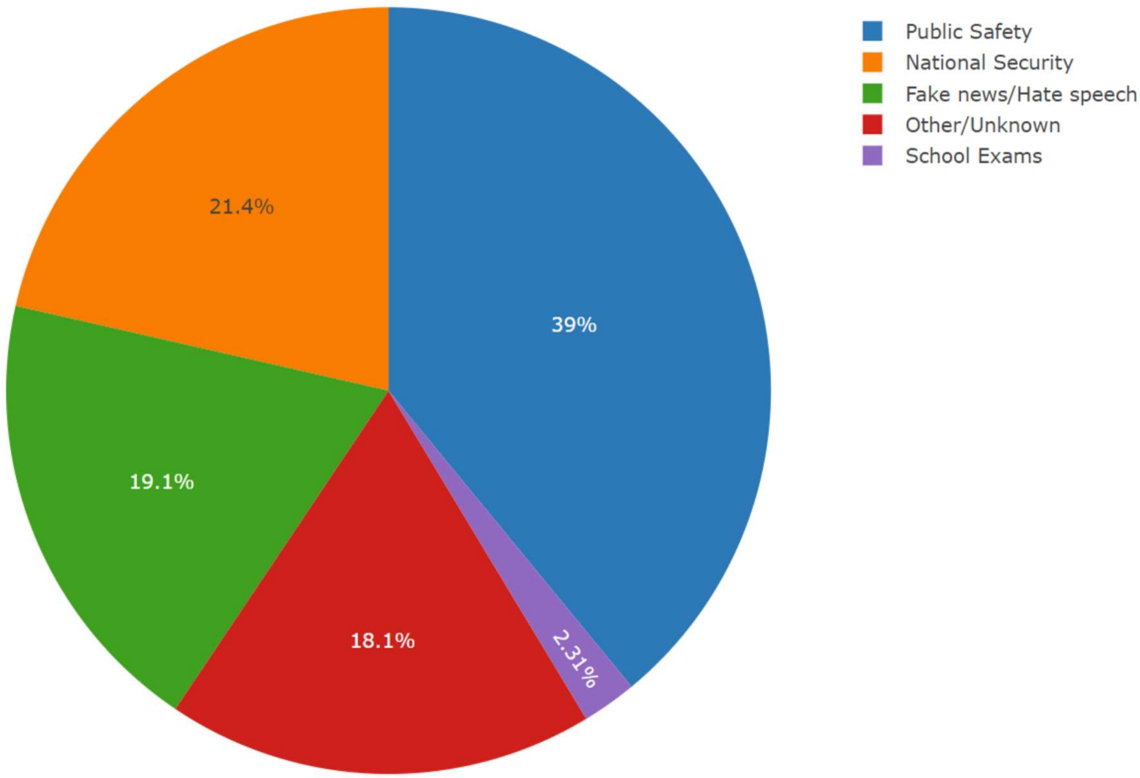


FIGURE A.5. Internet Shutdowns in India by Official Justification, 2016-2022

A.4. Alternate Political Support Measures

TABLE A.3. Political Support for State Ruling Party - Vote Majority

	Internet Shutdown		Physical Repression	
	(1)	(2)	(3)	(4)
CM Win$_{t-1}$	-0.7000**	-0.6316***	0.3153***	0.3196***
	(0.2814)	(0.2425)	(0.1087)	(0.1216)
Violent Events$_{t-1}$		0.1099***		0.3565***
		(0.0141)		(0.0842)
Violent Exposure$_{t-1}$		0.0659***		0.0482**
		(0.0109)		(0.0244)
Fixed-effects				
State	✓	✓	✓	✓
Week	✓	✓	✓	✓
Observations	155,805	155,805	237,765	237,765
Pseudo R^2	0.38585	0.40080	0.10043	0.12356

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A.4. Political Support for State Ruling Party - Category

	Internet Shutdown		Physical Repression	
	(1)	(2)	(3)	(4)
Opposition Stronghold$_{t-1}$	1.076***	0.9687***	-0.2519	-0.2794
	(0.3235)	(0.2772)	(0.1567)	(0.1798)
Swing District$_{t-1}$	0.1331	0.0324	-0.1263	-0.1354
	(0.1529)	(0.1270)	(0.1310)	(0.1376)
Violent Events$_{t-1}$		0.1044***		0.3585***
		(0.0153)		(0.0836)
Violent Exposure$_{t-1}$		0.0654***		0.0478*
		(0.0105)		(0.0244)
Fixed-effects				
State	✓	✓	✓	✓
Week	✓	✓	✓	✓
Observations	155,805	155,805	237,765	237,765
Pseudo R^2	0.39278	0.40679	0.09917	0.12244

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A.5. Looking Beyond Violent Events

Table A.5. Political Support for State Ruling Party with Total Events

	Internet Shutdown			Physical Repression		
	(1)	(2)	(3)	(4)	(5)	(6)
Political Support$_{t-1}$	-0.0246***	-0.0265***	-0.0242***	0.0084***	0.0045	0.0061**
	(0.0046)	(0.0043)	(0.0046)	(0.0028)	(0.0031)	(0.0028)
Total Events$_{t-1}$		0.0629***	0.0577***		0.2655***	0.2716***
		(0.0149)	(0.0170)		(0.0418)	(0.0436)
Total Exposure$_{t-1}$		0.0266***	0.0264***		0.0144	0.0143
		(0.0072)	(0.0071)		(0.0111)	(0.0111)
Political Support$_{t-1}$*Total Events$_{t-1}$			-0.0007***			-0.0008***
			(0.0002)			(0.0003)
Fixed-effects						
State	✓	✓	✓	✓	✓	✓
Week	✓	✓	✓	✓	✓	✓
Observations	155,805	155,805	155,805	237,765	237,765	237,765
Pseudo R^2	0.39529	0.40195	0.40230	0.10035	0.14551	0.14574

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A.6. Opposition Perpetrated Total Events

	Internet Shutdown		Physical Repression	
	(1)	(2)	(3)	(4)
Opposition Events$_{t-1}$	0.1938**	0.1557	0.1707***	0.0975***
	(0.0917)	(0.0966)	(0.0206)	(0.0164)
Unaffiliated Events$_{t-1}$		0.0535**		0.1057***
		(0.0273)		(0.0064)
Total Exposure$_{t-1}$		0.0272*		0.0395***
		(0.0151)		(0.0066)
Fixed-effects				
District	✓	✓	✓	✓
Week	✓	✓	✓	✓
Observations	45,390	45,390	229,779	229,779
Pseudo R^2	0.39459	0.39796	0.22223	0.23100

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A.6. ACLED Bias

TABLE A.7. Political Support for State Ruling Party in Urban Areas

	Internet Shutdown			Physical Repression		
	(1)	(2)	(3)	(4)	(5)	(6)
Political Support$_{t-1}$	-0.0210***	-0.0205***	-0.0235***	0.0104***	0.0098***	0.0093***
	(0.0034)	(0.0033)	(0.0033)	(0.0032)	(0.0036)	(0.0034)
Violent Events$_{t-1}$		0.1129***	0.1527***		0.3975***	0.3935***
		(0.0240)	(0.0191)		(0.0706)	(0.0678)
Violent Exposure$_{t-1}$		0.0252***	0.0242***		0.0298	0.0302
		(0.0096)	(0.0092)		(0.0212)	(0.0217)
Political Support$_{t-1}$*Violent Events$_{t-1}$			0.0021***			0.0007
			(0.0006)			(0.0014)
Fixed-effects						
State	✓	✓	✓	✓	✓	✓
Week	✓	✓	✓	✓	✓	✓
Observations	73,008	73,008	73,008	121,968	121,968	121,968
Pseudo R^2	0.48230	0.48756	0.48830	0.09573	0.11989	0.11994

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A.8. Opposition Perpetrated Violence in Urban Areas

	Internet Shutdown		Physical Repression	
	(1)	(2)	(3)	(4)
Opposition Violence$_{t-1}$	0.2567***	0.2462***	0.1522***	0.0172
	(0.0626)	(0.0612)	(0.0315)	(0.0425)
Unaffiliated Violence$_{t-1}$		0.0401		0.1945***
		(0.0253)		(0.0137)
Violent Exposure$_{t-1}$		0.0131		0.0674***
		(0.0159)		(0.0138)
Fixed-effects				
District	✓	✓	✓	✓
Week	✓	✓	✓	✓
Observations	25,272	25,272	123,057	123,057
Pseudo R^2	0.45415	0.45492	0.20989	0.21973

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A.9. Military Led Violence in Urban Areas

	(1)	(2)	(3)	(4)
Shutdown_{t-1}	0.1464***	0.1118***		
	(0.0472)	(0.0259)		
$\text{Violent Events}_{t-1}$		0.0238		0.0284
		(0.0142)		(0.0172)
$\text{Violent Exposure}_{t-1}$		0.0060**		0.0061**
		(0.0026)		(0.0027)
$\text{Physical Repression}_{t-1}$			0.0319	-0.0202
			(0.0220)	(0.0167)
Fixed-effects				
District	✓	✓	✓	✓
Week	✓	✓	✓	✓
Observations	125,961	125,961	125,961	125,961
R^2	0.22400	0.23178	0.22310	0.23104

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A.7. Regional and Temporal Heterogeneity

A.7.1. Excluding Jammu and Kashmir

TABLE A.G.1. Political Support for State Ruling Party, Excluding J&K

	Internet Shutdown			Physical Repression		
	(1)	(2)	(3)	(4)	(5)	(6)
Political Support$_{t-1}$	-0.0057	-0.0058	-0.0057	0.0094**	0.0085**	0.0090**
	(0.0086)	(0.0086)	(0.0091)	(0.0042)	(0.0040)	(0.0039)
Violent Events$_{t-1}$		0.3535***	0.3553***		0.4971***	0.5195***
		(0.0465)	(0.0525)		(0.0858)	(0.0805)
Violent Exposure$_{t-1}$		0.0046	0.0045		-0.0243*	-0.0244*
		(0.0228)	(0.0225)		(0.0130)	(0.0132)
Political Support$_{t-1}$*Violent Events$_{t-1}$			-0.0002			-0.0018
			(0.0019)			(0.0015)
Fixed-effects						
State	✓	✓	✓	✓	✓	✓
Week	✓	✓	✓	✓	✓	✓
Observations	72,102	72,102	72,102	230,505	230,505	230,505
Pseudo R^2	0.13583	0.14538	0.14538	0.07281	0.08990	0.09003

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A.G.2. Opposition Perpetrated Violence, Excluding J&K

	Internet Shutdown		Physical Repression	
	(1)	(2)	(3)	(4)
Opposition Violence$_{t-1}$	0.4054***	0.2355*	0.1512***	0.0310
	(0.0873)	(0.1276)	(0.0333)	(0.0425)
Unaffiliated Violence$_{t-1}$		0.3678***		0.1938***
		(0.0853)		(0.0214)
Violent Exposure$_{t-1}$		0.0023		0.0441***
		(0.0288)		(0.0084)
Fixed-effects				
District	✓	✓	✓	✓
Week	✓	✓	✓	✓
Observations	19,680	19,680	222,519	222,519
Pseudo R^2	0.10134	0.10794	0.19508	0.19795

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A.G.3. Military Led Violence, Excluding J&K

	(1)	(2)	(3)	(4)
Shutdown$_{t-1}$	-0.0068	-0.0111**		
	(0.0041)	(0.0043)		
Violent Events$_{t-1}$		0.0050***		0.0048**
		(0.0017)		(0.0023)
Violent Exposure$_{t-1}$		0.0005*		0.0005*
		(0.0003)		(0.0003)
Physical Repression$_{t-1}$			0.0078***	0.0008
			(0.0016)	(0.0036)
Fixed-effects				
District	✓	✓	✓	✓
Week	✓	✓	✓	✓
Observations	243,936	243,936	243,936	243,936
R^2	0.07818	0.07858	0.07829	0.07858

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A.7.2. State Political Party Control

TABLE A.G.4. Political Support for State Ruling Party in BJP-Controlled States

	Internet Shutdown			Physical Repression		
	(1)	(2)	(3)	(4)	(5)	(6)
Political Support$_{t-1}$	-0.0569***	-0.0547***	-0.0529***	0.0065	0.0122*	0.0109*
	(0.0153)	(0.0158)	(0.0138)	(0.0088)	(0.0063)	(0.0059)
Violent Events$_{t-1}$		0.0628***	-0.0247		0.2881***	0.3108***
		(0.0117)	(0.1542)		(0.0928)	(0.1038)
Violent Exposure$_{t-1}$		0.0158*	0.0149		0.0741***	0.0760***
		(0.0090)	(0.0101)		(0.0086)	(0.0070)
Political Support$_{t-1}$∗Violent Events$_{t-1}$			-0.0028			0.0017
			(0.0046)			(0.0016)
Fixed-effects						
State	✓	✓	✓	✓	✓	✓
Week	✓	✓	✓	✓	✓	✓
Observations	54,962	54,962	54,962	109,758	109,758	109,758
Pseudo R^2	0.49066	0.49261	0.49286	0.11070	0.13251	0.13275

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A.G.5. Political Support for State Ruling Party in Non-BJP-Controlled States

	Internet Shutdown			Physical Repression		
	(1)	(2)	(3)	(4)	(5)	(6)
Political Support$_{t-1}$	0.0361	0.0255	0.0244	0.0119	0.0070	0.0074
	(0.0270)	(0.0227)	(0.0222)	(0.0088)	(0.0061)	(0.0056)
Violent Events$_{t-1}$		0.2380***	0.2216***		0.4378***	0.4552***
		(0.0440)	(0.0550)		(0.0880)	(0.0974)
Violent Exposure$_{t-1}$		0.0563**	0.0569**		0.0256	0.0255
		(0.0284)	(0.0289)		(0.0269)	(0.0272)
Political Support$_{t-1}$∗Violent Events$_{t-1}$			0.0023			-0.0012
			(0.0029)			(0.0022)
Fixed-effects						
State	✓	✓	✓	✓	✓	✓
Week	✓	✓	✓	✓	✓	✓
Observations	31,490	31,490	31,490	126,806	126,806	126,806
Pseudo R^2	0.34542	0.35956	0.35975	0.11066	0.13509	0.13517

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A.7.3. Election Years

Table A.G.6. Political Support for State Ruling Party in State Assembly Election Years

	Internet Shutdown			Physical Repression		
	(1)	(2)	(3)	(4)	(5)	(6)
Political Support$_{t-1}$	0.0125	0.0128	0.0113	0.0126**	0.0113*	0.0114*
	(0.0169)	(0.0166)	(0.0187)	(0.0061)	(0.0059)	(0.0059)
Violent Events$_{t-1}$		0.2962	0.4138		0.4297***	0.4336***
		(0.3104)	(0.4996)		(0.1204)	(0.1124)
Violent Exposure$_{t-1}$		0.1882	0.1868		-0.0175	-0.0173
		(0.2035)	(0.2025)		(0.0278)	(0.0283)
Political Support$_{t-1}$*Violent Events$_{t-1}$			0.0048			-0.0005
			(0.0110)			(0.0024)
Fixed-effects						
State	✓	✓	✓	✓	✓	✓
Week	✓	✓	✓	✓	✓	✓
Observations	2,058	2,058	2,058	35,266	35,266	35,266
Pseudo R^2	0.10976	0.11869	0.11908	0.10093	0.11426	0.11427

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A.G.7. Political Support for State Ruling Party in 2019

	Internet Shutdown			Physical Repression		
	(1)	(2)	(3)	(4)	(5)	(6)
Political Support$_{t-1}$	-0.0484***	-0.0457***	-0.0498***	-0.0019	0.0010	0.0005
	(0.0130)	(0.0146)	(0.0138)	(0.0101)	(0.0074)	(0.0079)
Violent Events$_{t-1}$		0.0987***	0.1779***		0.2411***	0.2407***
		(0.0370)	(0.0274)		(0.0685)	(0.0644)
Violent Exposure$_{t-1}$		0.0062	0.0073		0.0249	0.0253
		(0.0135)	(0.0132)		(0.0235)	(0.0236)
Political Support$_{t-1}$*Violent Events$_{t-1}$			0.0030***			0.0005
			(0.0007)			(0.0010)
Fixed-effects						
State	✓	✓	✓	✓	✓	✓
Week	✓	✓	✓	✓	✓	✓
Observations	15,022	15,022	15,022	33,644	33,644	33,644
Pseudo R^2	0.43511	0.44205	0.44479	0.11820	0.13556	0.13560

Clustered (State & Week) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

APPENDIX B

Subnational Protest Diffusion

B.1. ACLED Keyword Coding

I identified ACLED events that were related to the India Farmer's protest using these steps:

(1) Collect all protest or riot events from ACLED occurring in India during the period that the Farmer's protest was active (8/01/2020 - 12/31/2021).

(2) Use keyword coding on the notes and actor columns to identify events related to the Farmers' Protest. This list of actors was identified in an iterative process that involved researching organizations that participated in the Indian Farmer's Protests and then matching those groups to the associated actors in the ACLED data as well as researching the associated actors listed in the ACLED data to determine if they were affiliated with the Indian Farmer's Protests.

   (a) Notes keywords list: farm, agriculture, agricultural, crop

   (b) Actor keywords list: AIAWU: All India Agricultural Workers Union; AICCTU: All India Central Council of Trade Unions; AIFTU: All India Federation of Trade Unions; AIKKMS: All India Peasants and Farm Labourers Association; AIKS: All India Kisan Sabha; AIKSCC: All India Farmers' Struggle Coordination Committee; AITUC: All India Trade Union Congress; AIUTUC: All India United Trade Union Centre; BKU: Bharatiya Kisan Union; BKMU: Bharatiya Khet Mazdoor Union; BKS: Indian Farmers' Collective; CITU: (Centre of Indian Trade Unions); Farmers; HMS: Workers Assembly of India; INTTUC: Indian National Trinamool Trade Union Congress; INTUC: Indian National Trade Union Congress; KKU: Kirti Kisan Union; KMSC: Farm Labourer Struggle Committee; KSC: Kisan Sangarsh Committee; LPF: Labour Progressive Federation; RKS: Rashtriya Kisan Sangathan; SKM: United Farmers Front; TUCC: Trade Union Coordination Centre

B.2. Exposure Measures

TABLE A.G.1. Correlation between Diffusion Measures

	Geographic	Social	Political
Geographic	1	0.30	0.17
Social	0.30	1	0.06
Political	0.17	0.06	1

B.3. Alternate Measures of Geographic Proximity

In this section, I present the models using alternate measures of geographic proximity. My main analysis identifies geographic exposure as districts that share a border with another district experiencing protests related to the India Farmers' movement in the previous week. To ensure that my results are not merely a product of this measurement strategy, I repeat my main analysis using several alternate approaches. First, I consider geographic exposure between districts with centroids that are located within 100km of each other. Next, I examine second and third order geographic contiguity. Second order geographic contiguity is calculated by squaring the first order geographic border contiguity matrix, resulting in a value of 1 for district dyads that are direct neighbors or neighbors of neighbors. I then repeated this process to identify third order geographic contiguity.

Table A.G.2. Alternate Measures of Geographic Exposure on Protest Outbreak

	(1)	(2)	(3)	(4)
Direct Border Exposure	0.2256***			
	(0.0729)			
Centroid Exposure		0.2476***		
		(0.0705)		
Second Border Exposure			0.3100***	
			(0.1006)	
Third Border Exposure				0.4097***
				(0.1317)
Fixed-effects				
District	✓	✓	✓	✓
Week	✓	✓	✓	✓
Observations	40,050	40,050	40,050	40,050
Squared Correlation	0.31722	0.31718	0.31761	0.31802
Pseudo R^2	0.35754	0.35756	0.35800	0.35845

Clustered (District & Week) standard-errors in parenthesis
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

B.3.1. Centroid Proximity

Table A.G.3. Geographic Centroid Exposure on Protest Outbreak

	(1)	(2)	(3)
Centroid Exposure	0.2476***	0.2411***	0.2186***
	(0.0705)	(0.0693)	(0.0658)
Social Exposure		1.230***	
		(0.1519)	
Political Exposure			0.2034
			(0.1571)
Fixed-effects			
District	✓	✓	✓
Week	✓	✓	✓
Observations	40,050	40,050	40,050
Squared Correlation	0.31718	0.31725	0.31759
Pseudo R^2	0.35756	0.35771	0.35780

Clustered (District & Week) standard-errors in parenthesis
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

B.3.2. Second and Third Order Geographic Proximity

TABLE A.G.4. Second Order Geographic Exposure on Protest Outbreak

	(1)	(2)	(3)
Second Border Exposure	0.3100***	0.3042***	0.2819***
	(0.1006)	(0.1001)	(0.0940)
Social Exposure		1.206***	
		(0.1608)	
Political Exposure			0.1701
			(0.1514)
Fixed-effects			
District	✓	✓	✓
Week	✓	✓	✓
Observations	40,050	40,050	40,050
Squared Correlation	0.31761	0.31768	0.31793
Pseudo R^2	0.35800	0.35814	0.35816

Clustered (District & Week) standard-errors in parenthesis
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A.G.5. Third Order Geographic Exposure on Protest Outbreak

	(1)	(2)	(3)
Third Border Exposure	0.4097***	0.4030***	0.3830***
	(0.1317)	(0.1307)	(0.1243)
Social Exposure		1.163***	
		(0.1609)	
Political Exposure			0.1574
			(0.1478)
Fixed-effects			
District	✓	✓	✓
Week	✓	✓	✓
Observations	40,050	40,050	40,050
Squared Correlation	0.31802	0.31809	0.31831
Pseudo R^2	0.35845	0.35858	0.35859

Clustered (District & Week) standard-errors in parenthesis
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

B.4. Controlling for Rainfall Shocks

Below I present the results from models with an added control for rainfall shocks. These shocks are measured as the difference in a district's level of precipitation in a week from the rolling average precipitation levels in that district-week over the ten years prior.

TABLE A.G.6. Protest Exposure on Protest Outbreak with Rainfall Shock Control

	(1)	(2)	(3)
Geographic Exposure	0.2386***	0.2314***	0.2253***
	(0.0860)	(0.0850)	(0.0814)
Social Exposure		1.385***	
		(0.1599)	
Political Exposure			0.0980
			(0.1600)
Rainfall Shock	-2.584	-2.641	-2.569
	(2.147)	(2.152)	(2.147)
Fixed-effects			
District	✓	✓	✓
Week	✓	✓	✓
Observations	32,769	32,769	32,769
Squared Correlation	0.32525	0.32538	0.32537
Pseudo R^2	0.36182	0.36205	0.36188

Clustered (District & Week) standard-errors in parenthesis
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

B.5. Details on Vignette Experiment

B.5.1. Participant Demographics

Appendix Table A.G.7 shows the characteristics of my full experimental sample.

	Overall (N=963)
State	
Haryana	322 (33.4%)
Kerala	431 (44.8%)
Nagaland	210 (21.8%)
Gender	
Female	447 (46.4%)
Male	514 (53.4%)
Other	2 (0.2%)
Religion	
Agnostic	5 (0.5%)
Atheist	10 (1.0%)
Buddhist	2 (0.2%)
Christian	227 (23.6%)
Hindu	575 (59.7%)
Jain	5 (0.5%)
Muslim	120 (12.5%)
None	6 (0.6%)
Sikh	11 (1.1%)
Something Else	2 (0.2%)
Language	
Assamese	23 (2.4%)
Bengali	11 (1.1%)
Gujarati	1 (0.1%)
Hindi	368 (38.2%)
Malayalam	423 (43.9%)
Marathi	2 (0.2%)
Odia	1 (0.1%)
Other	107 (11.1%)
Punjabi	21 (2.2%)
Tamil	4 (0.4%)
Telugu	2 (0.2%)
Political Party	
All India Trinamool Congress (AITC)	8 (0.8%)
Bahujan Samaj Party (BSP)	3 (0.3%)
Bharatiya Janata Party (BJP)	455 (47.2%)
Communist Party of India - Marxist (CPI-M)	76 (7.9%)
Communist Party of India (CPI)	48 (5.0%)
India National Congress (INC)	228 (23.7%)
National People's Party (NPP)	13 (1.4%)
Nationalist Congress Party (NCP)	21 (2.2%)
Other	111 (11.5%)

TABLE A.G.7. Descriptive Statistics of Survey Participants

B.5.2. Data Handling

For my analysis, I construct a stacked version of the data from these 963 participants, resulting in a total of 2889 observations across all three vignettes. However, due to the nature of the vignette experiment, which is premised on providing participants with varying information about protests, dependent on the participants' identity features, I excluded certain observations from respondents who reported identities that did not conform to the specified options in the experiment. These observations were excluded because it was not possible to provide these participants with alternate, unspecified identity features with the correct information that corresponded with their treatment assignment or to identify treatment effects. As an example, in the treatments for same identity, a participant who indicated *Other* as their linguistic identity would received about a protest led by an *Other group*. Thus drawing conclusions from these participants is not feasible.

Specifically, in the first vignette using religious identity, I excluded 8 observations from participants who reported their religion as *None* or *Something else*, resulting in 955 total observations. For the second vignette on linguistic identity, I excluded 107 participants who reported their language as *Other*, resulting in 856 total observations. In the third vignette on political affiliation, I excluded 111 participants who selected *Other* for the political party they supported, resulting in 852 total observations. Thus, stacking the resulting data from each vignette produced a dataset with 2,663 observations.

B.5.3. Protest Vignettes

The information provided to participants about the location and identities of the protest vary depending on their assigned treatment group. In the vignettes below, the values of ***district*** are manipulated such that they either show the name of a district that is either proximate or distant from the district where the participant indicated they reside. Similarly, the values of ***group*** are manipulated such that they either match or differ from the identities of the participant, which were identified using questions asked earlier in the survey about the participants' identities.

Vignette 1: Water Conditions - Religion

In recent years, there have been many different protests organized in response to public grievances.

Now I am going to present you with several fictional scenarios about a protest and ask you to imagine how you would respond to it.

"There was a protest last week in ***district*** in response to unsanitary water conditions. The protest was led by a ***religious group*** seeking greater government protections for sanitary water conditions."

Vignette 2: Road Conditions - Language

"There was a protest last week in ***district*** in response to poor road conditions. The protest was led by a ***linguistic group*** seeking greater government attention to improving the quality of roads."

Vignette 3: Waste Management - Political Party

"There was a protest last week in ***district*** in response to issues with the removal and storage of city garbage. The protest was led by supporters of a ***political party*** seeking improved government waste management procedures."

B.5.4. Vignette Experiment Outcomes

Following each vignette, participants were presented with the following statements regarding the fictitious protest and asked to respond with how much they agreed or disagreed with the statement. Their response choices were (1) Strongly agree, (2), Agree, (3) Neither agree nor disagree, (4) Disagree, (5) Strongly Disagree.

(1) I support this protest movement.
(2) I support these protesters
(3) I would participate in a protest against [unsanitary water conditions/poor road conditions/poor waste management].
(4) I would ask a friend to participate in a protest against [unsanitary water conditions/poor road conditions/poor waste management].
(5) I would organize a protest against [unsanitary water conditions/poor road conditions/poor waste management].
(6) I would post on social media (e.g. Facebook, WhatsApp, Twitter) about [unsanitary water conditions/poor road conditions/poor waste management].

Table A.G.8 below shows the average value for each of these questions across all respondents. The values were computed by translating the question responses as follows: Strongly agree (2), Agree (1), Neither agree nor disagree (0), Disagree (-1), Strongly disagree (-2).

TABLE A.G.8. Vignette Experiment Mean Outcomes

	Q1	Q2	Q3	Q4	Q5	Q6	Total
Vignette 1	0.95	0.79	1.02	0.95	0.75	0.98	0.91
Vignette 2	1.23	1.07	1.09	1	0.87	1.09	1.06
Vignette 3	1.19	1.08	1.1	1.01	0.85	1.07	1.05
Average	1.12	0.98	1.07	0.99	0.82	1.05	1.01

TABLE A.G.9. The Effect of Identity and Geographic Proximity on All Outcome Variables

	(1)	(2)	(3)	(4)	(5)	(6)
Same Identity	0.021	0.070*	0.059*	0.032	0.055	0.088***
	(0.592)	(0.072)	(0.088)	(0.364)	(0.128)	(0.007)
Close Geography	0.100***	0.099**	0.071**	0.045	0.017	0.051
	(0.008)	(0.018)	(0.048)	(0.214)	(0.630)	(0.110)
Respondent Controls	✓	✓	✓	✓	✓	✓
Observations	2,640	2,630	2,636	2,634	2,631	2,635
R^2	0.667	0.671	0.726	0.728	0.783	0.772
Adjusted R^2	0.477	0.483	0.569	0.573	0.659	0.641

Adjusted p-values for clustered respondent standard-errors in parenthesis
*Signif Codes: ***: 0.01, **: 0.05, *: 0.1*

B.6. Heterogeneous Effects of Vignette Experiment

TABLE A.G.10. The Effect of Identity and Geographic Proximity on Protest Attitudes for Hindu Participants

	Total	Support	Participate
Same Identity	0.053*	0.042	0.058*
	(0.067)	(0.315)	(0.061)
Close Geography	0.046*	0.074*	0.032
	(0.096)	(0.094)	(0.275)
Respondent Controls	✓	✓	✓
Observations	1,654	1,653	1,650
R^2	0.820	0.696	0.814
Adjusted R^2	0.724	0.534	0.715

Adjusted p-values for clustered respondent standard-errors in parenthesis
*Signif Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A.G.11. The Effect of Identity and Geographic Proximity on Protest Attitudes for Non-Hindu Participants

	Total	Support	Participate
Same Identity	0.058	0.047	0.063
	(0.173)	(0.462)	(0.149)
Close Geography	0.096**	0.139**	0.074*
	(0.024)	(0.021)	(0.099)
Respondent Controls	✓	✓	✓
Observations	990	990	990
R^2	0.815	0.692	0.820
Adjusted R^2	0.696	0.494	0.704

Adjusted p-values for clustered respondent standard-errors in parenthesis
*Signif Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A.G.12. The Effect of Identity and Geographic Proximity on Protest Attitudes for Hindi Participants

	Total	Support	Participate
Same Identity	0.030	0.003	0.043
	(0.378)	(0.955)	(0.247)
Close Geography	0.014	0.046	−0.002
	(0.668)	(0.354)	(0.958)
Respondent Controls	✓	✓	✓
Observations	1,068	1,068	1,066
R^2	0.818	0.707	0.815
Adjusted R^2	0.722	0.552	0.718

Adjusted p-values for clustered respondent standard-errors in parenthesis
*Signif Codes: ***: 0.01, **: 0.05, *: 0.1*

TABLE A.G.13. The Effect of Identity and Geographic Proximity on Protest Attitudes for Non-Hindi Participants

	Total	Support	Participate
Same Identity	0.073**	0.073	0.073**
	(0.029)	(0.143)	(0.036)
Close Geography	0.097***	0.132***	0.079**
	(0.003)	(0.007)	(0.017)
Respondent Controls	✓	✓	✓
Observations	1,576	1,575	1,574
R^2	0.819	0.689	0.818
Adjusted R^2	0.710	0.500	0.708

Adjusted p-values for clustered respondent standard-errors in parenthesis
*Signif Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A.G.14. The Effect of Identity and Geographic Proximity on Protest Attitudes for BJP Participants

	Total	Support	Participate
Same Identity	0.067*	0.052	0.074*
	(0.061)	(0.235)	(0.057)
Close Geography	0.049	0.076*	0.035
	(0.113)	(0.093)	(0.286)
Respondent Controls	✓	✓	✓
Observations	1,316	1,316	1,313
R^2	0.794	0.689	0.786
Adjusted R^2	0.685	0.525	0.674

Adjusted p-values for clustered respondent standard-errors in parenthesis
*Signif Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A.G.15. The Effect of Identity and Geographic Proximity on Protest Attitudes for Non-BJP Participants

	Total	Support	Participate
Same Identity	0.043	0.038	0.046
	(0.175)	(0.503)	(0.148)
Close Geography	0.080**	0.121**	0.059
	(0.024)	(0.029)	(0.110)
Respondent Controls	✓	✓	✓
Observations	1,328	1,327	1,327
R^2	0.835	0.698	0.838
Adjusted R^2	0.734	0.510	0.738

Adjusted p-values for clustered respondent standard-errors in parenthesis
*Signif Codes: ***: 0.01, **: 0.05, *: 0.1*

B.7. Ethical Considerations

While this survey received IRB approval, it is essential to take steps to limit potential harm when conducting this research. The main ethical considerations for this project are (1) the protection of identifying data from the participants and (2) confirmation that the protest vignettes were fictional.

B.7.1. Data Handling

While India is considered to be a democratic country that largely respects the political and human rights of its citizens, special attention must be paid to the handling of the survey data in this project due to the sensitive nature of protest participation in the country. The risk posed by a loss of confidentiality of the participants is minimal because the survey does not ask particularly sensitive questions. The survey asks questions about protest intentions rather than about actual protest participation, protecting participants against retaliation for their responses. My co-investigators and the survey company, Lucid, ensured that survey responses were kept separate from identifying information to minimize the risk of loss of confidentiality.

B.7.2. Vignette Experiment Debrief

This project is focused on how receiving information about a protest event impacts an individual's propensity to participate and support similar protests. The fictional protest vignettes involve topics that are not particularly polarizing, reducing the chance that participation in the experiment will inspire anger or violence. Nevertheless, due to the nature of this research, it was essential to take special care to ensure that participants understood that the protest vignettes were fictional in order to reduce the risk that participants would be mobilized into actually participating in a real protest. As such, I included the following debrief at the end of the survey reminding participants that the protest vignettes were fake. Following the debriefing statement, I included a check to ensure the participants understood that the protest vignettes were fictional. For each of the three protest vignettes, I asked participants whether or not they believed the protest was real. The responses to these questions confirm that a majority of the respondents understood that all of the protest vignettes were not real.

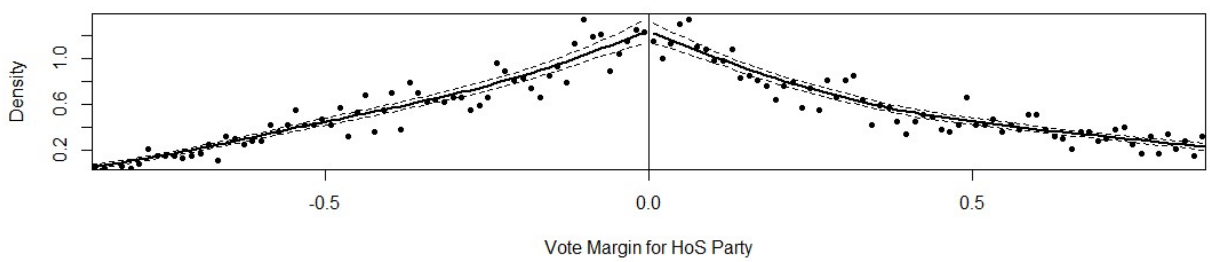
APPENDIX C

Incumbency Advantage in Violence

C.1. Identification Strategy

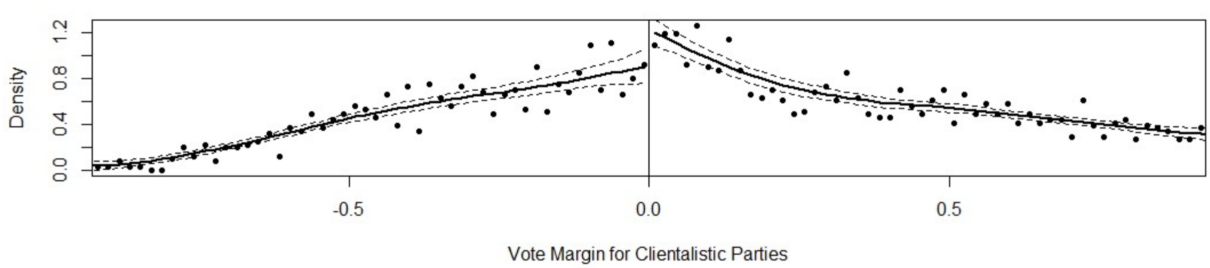
C.1.1. Additional McCrary Tests

FIGURE A.G.1. McCrary Test: Sorting Around the Winning Threshold for the National Executive State Party



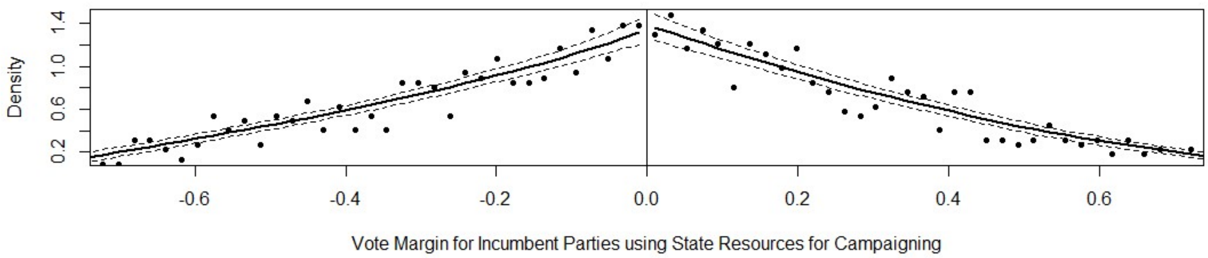
Note: Each point represents a bin. Bin size is 0.0135. Discontinuity estimate (standard error): -0.008 (.085).

FIGURE A.G.2. McCrary Test: Sorting Around the Winning Threshold for Clientelist Parties



Note: Each point represents a bin. Bin size is 0.018. Discontinuity estimate (standard error): 0.297 (0.117).

Figure A.G.3. McCrary Test: Sorting Around the Winning Threshold for Incumbent Parties using State Resources in their Election Campaign



Note: Each point represents a bin. Bin size is 0.021. Discontinuity estimate (standard error): 0.0385 (0.126).

C.1.2. Variation in EV Events

Table A.G.1. Number of EV Events Perpetrated by the Winning Party for Different Bandwidths

	Bandwidth			
Category	All	5%	2%	1%
All Races Pooled	4899	1342	89	65
Autocracy	4313	1277	74	59
Democracy	586	65	15	6
Low Capacity	1751	144	59	50
Medium Capacity	2674	1170	18	9
High Capacity	474	28	12	6
Peacetime	676	103	27	18
Post Conflict	726	26	10	4
Active Conflict	3497	1213	52	43

TABLE A.G.2. Number of EV Events Perpetrated by the Losing Party for Different Bandwidths

	Bandwidth			
Category	All	5%	2%	1%
All Races Pooled	2776	487	366	40
Autocracy	2405	436	349	35
Democracy	371	51	17	5
Low Capacity	1259	119	39	28
Medium Capacity	1207	332	310	5
High Capacity	310	36	17	7
Peacetime	952	97	21	17
Post Conflict	298	29	11	1
Active Conflict	1526	361	334	22

C.2. Supplemental Analyses

TABLE A.G.3. Difference in Means Estimates

	Bandwidth		
Category	5%	2%	1%
All Races Pooled	0.902 (0.456)	-0.721 (0.356)	0.117 (0.453)
Autocracy	1.298 (0.464)	-1.03 (0.359)	0.162 (0.468)
Democracy	0.047 (0.582)	-0.017 (0.821)	0.015 (0.776)
Low Capacity	0.053 (0.561)	0.109 (0.546)	0.2 (0.493)
Medium Capacity	2.66 (0.465)	-2.116 (0.328)	0.054 (0.487)

Table A.G.3. Difference in Means Estimates *(continued)*

Category	Bandwidth 5%	2%	1%
High Capacity	-0.049 (0.585)	-0.081 (0.628)	-0.034 (0.883)
Peacetime	0.014 (0.837)	0.036 (0.624)	0.011 (0.931)
Post Conflict	-0.028 (0.819)	-0.024 (0.903)	0.2 (0.242)
Active Conflict	2.073 (0.458)	-1.621 (0.347)	0.204 (0.501)

Table A.G.4. Polynomial Estimates

Category	Bandwidth 5%	2%	1%
All Races Pooled	-0.921 (0.204)	0.627 (0.304)	-0.172 (0.435)
Autocracy	0.053 (0.972)	-1.116 (0.173)	0.118 (0.81)
Democracy	0.021 (0.855)	-0.044 (0.732)	0.047 (0.606)
Low Capacity	0.056 (0.869)	-0.691 (0.133)	-0.091 (0.69)
Medium Capacity	0.183 (0.941)	-0.758 (0.519)	0.686 (0.316)
High Capacity	-0.529 (0.484)	-1.796 (0.246)	-3.404 (0.279)
Peacetime	-0.108 (0.444)	-0.113 (0.424)	-0.024 (0.817)
Post Conflict	0.729 (0.118)	-0.246 (0.823)	2.819 (0.241)
Active Conflict	0.164 (0.943)	-1.271 (0.196)	0.226 (0.737)

Table A.G.5. Difference in Means Estimates (Logged EV)

Category	5%	2%	1%
All Races Pooled	0.015 (0.412)	-0.007 (0.806)	0.014 (0.68)

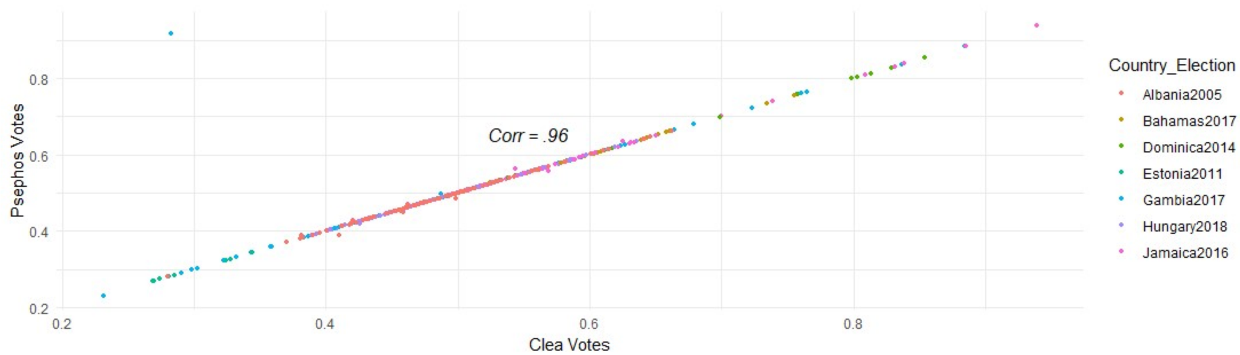
Table A.G.6. Difference in Means Estimates (Categorical EV)

Category	5%	2%	1%
All Races Pooled	0.023 (0.26)	0.01 (0.725)	0.019 (0.634)

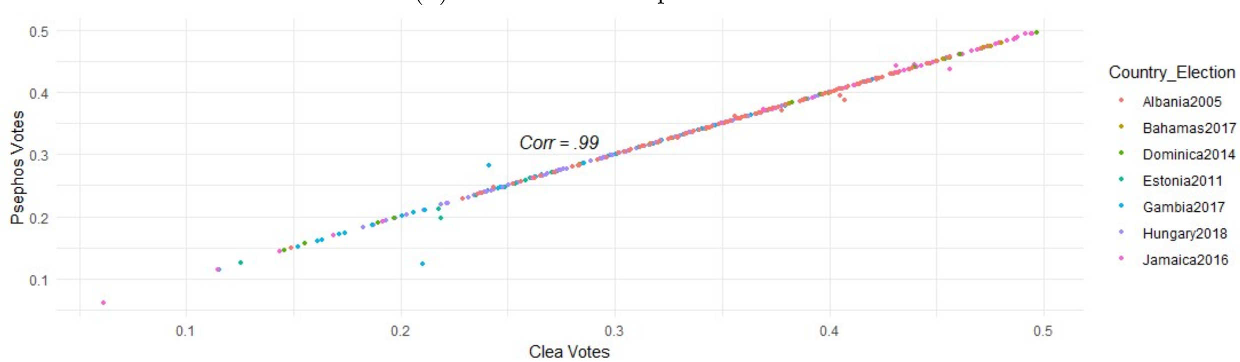
C.3. Validating Psephos Data

Figure A.G.4. Comparison of CLEA and Psephos

(a) Votes for first place candidate



(b) Votes for second place candidate



C.4. Distribution of Literature on Election Violence

Figure A.G.5. Distribution of cases covered in the existing academic literature on election violence

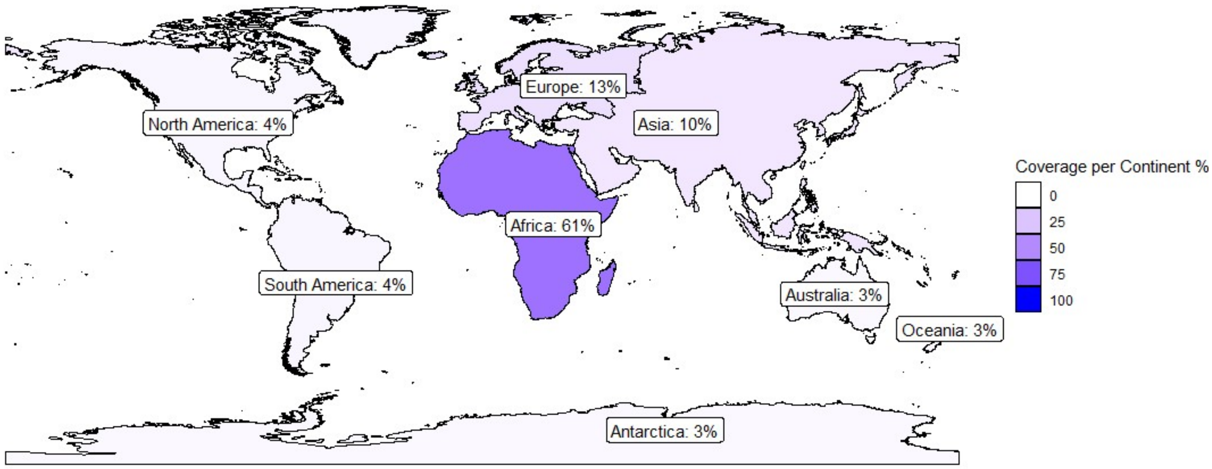


Figure A.G.5 shows the proportion of top election violence articles that cover each country, aggregated to the continent level. Top election violence articles consists of 192 articles from the first 20 pages on Google Scholar with the search term "Electoral Violence".

C.5. Exclusion of Cases from our Data

Table A.G.7. NELDA Elections Exclusion Criteria

Country	Year	Dropped_Reason
Afghanistan	2005	Not ACLED Covered
Afghanistan	2010	Not ACLED Covered
Afghanistan	2018	PR
Albania	2001	No contention
Albania	2001	No contention
Albania	2005	No contention
Albania	2009	Not ACLED Covered
Albania	2013	No contention
Albania	2017	No contention
Algeria	2002	No Prior Election Data
Algeria	2007	No Prior Election Data
Algeria	2012	No Prior Election Data
Algeria	2017	No Prior Election Data
Andorra	2001	No contention
Andorra	2005	No contention
Andorra	2009	No contention
Andorra	2011	No contention
Andorra	2015	No contention
Andorra	2019	No contention
Angola	2008	PR
Angola	2012	PR
Angola	2017	PR
Antigua & Barbuda	2004	Not ACLED Covered
Antigua & Barbuda	2009	No contention
Antigua & Barbuda	2014	No contention
Antigua & Barbuda	2018	No contention
Argentina	2001	No contention
Argentina	2003	No contention

Argentina	2005	No contention
Argentina	2007	No contention
Argentina	2009	No contention
Argentina	2011	No contention
Argentina	2017	No contention
Argentina	2019	No contention
Armenia	2003	No contention
Armenia	2007	Not ACLED Covered
Armenia	2012	Not ACLED Covered
Armenia	2017	No contention
Armenia	2018	No contention
Australia	2001	No contention
Australia	2004	No contention
Australia	2007	No contention
Australia	2010	No contention
Australia	2013	No contention
Australia	2016	No contention
Australia	2019	No contention
Austria	2002	No contention
Austria	2006	No contention
Austria	2008	No contention
Austria	2013	No contention
Austria	2017	Not ACLED Covered
Austria	2019	No contention
Azerbaijan	2000	Not ACLED Covered
Azerbaijan	2001	Not ACLED Covered
Azerbaijan	2005	Not ACLED Covered
Azerbaijan	2006	No contention
Azerbaijan	2010	Not ACLED Covered
Azerbaijan	2015	Not ACLED Covered
Azerbaijan	2020	In our Cases
Bahamas	2002	No contention
Bahamas	2007	No contention

Bahamas	2012	No contention
Bahamas	2017	No contention
Bahrain	2002	No contention
Bahrain	2006	No contention
Bahrain	2006	No contention
Bahrain	2010	Not ACLED Covered
Bahrain	2010	Not ACLED Covered
Bahrain	2014	Not ACLED Covered
Bahrain	2018	In our Cases
Bangladesh	2001	Not ACLED Covered
Bangladesh	2008	Not ACLED Covered
Bangladesh	2014	In our Cases
Bangladesh	2018	In our Cases
Barbados	2003	No contention
Barbados	2008	No contention
Barbados	2013	No contention
Barbados	2018	No contention
Belarus	2000	Not ACLED Covered
Belarus	2000	Not ACLED Covered
Belarus	2004	Not ACLED Covered
Belarus	2008	Not ACLED Covered
Belarus	2012	Not ACLED Covered
Belarus	2016	Not ACLED Covered
Belarus	2019	No EV in Close Races
Belgium	2003	No contention
Belgium	2007	No contention
Belgium	2010	No contention
Belgium	2014	No contention
Belgium	2019	No contention
Belize	2003	No contention
Belize	2008	No contention
Belize	2012	No contention
Belize	2015	Not ACLED Covered

Belize	2020	No contention
Benin	2003	PR
Benin	2007	No contention
Benin	2011	No contention
Benin	2015	No contention
Benin	2019	No Prior Election Data
Bhutan	2007	No contention
Bhutan	2008	No contention
Bhutan	2013	No contention
Bhutan	2013	No contention
Bhutan	2013	No contention
Bhutan	2018	No contention
Bhutan	2018	No contention
Bhutan	2018	No contention
Bolivia	2002	No contention
Bolivia	2005	Not ACLED Covered
Bolivia	2009	No contention
Bolivia	2014	No contention
Bolivia	2020	No contention
Bosnia-Herzegovina	2000	No contention
Bosnia-Herzegovina	2002	No contention
Bosnia-Herzegovina	2006	No contention
Bosnia-Herzegovina	2010	Not ACLED Covered
Bosnia-Herzegovina	2014	Not ACLED Covered
Bosnia-Herzegovina	2018	PR
Botswana	2004	No contention
Botswana	2009	No contention
Botswana	2014	No contention
Botswana	2019	No contention
Brazil	2002	No contention
Brazil	2006	No contention
Brazil	2010	No contention
Brazil	2014	No contention

Brazil	2018	No contention
Bulgaria	2001	No contention
Bulgaria	2005	No contention
Bulgaria	2009	No contention
Bulgaria	2013	Not ACLED Covered
Bulgaria	2014	No contention
Bulgaria	2017	No contention
Burkina Faso	2002	No contention
Burkina Faso	2007	No contention
Burkina Faso	2012	No contention
Burkina Faso	2015	PR
Burkina Faso	2020	PR
Burundi	2005	No contention
Burundi	2010	No Prior Election Data
Burundi	2015	No Prior Election Data
Burundi	2020	No Prior Election Data
Cambodia	2003	Not ACLED Covered
Cambodia	2008	Not ACLED Covered
Cambodia	2013	PR
Cambodia	2018	PR
Cameroon	2002	No contention
Cameroon	2007	No contention
Cameroon	2013	No contention
Cameroon	2013	No contention
Cameroon	2020	No Prior Election Data
Canada	2000	No contention
Canada	2004	No contention
Canada	2006	No contention
Canada	2008	No contention
Canada	2011	No contention
Canada	2015	No contention
Canada	2019	No contention
Cape Verde	2001	No contention

Cape Verde	2006	No contention
Cape Verde	2011	No contention
Cape Verde	2016	No contention
Central African Republic	2005	No contention
Central African Republic	2005	No contention
Central African Republic	2011	No Prior Election Data
Central African Republic	2011	No Prior Election Data
Central African Republic	2015	No contention
Central African Republic	2016	No contention
Central African Republic	2016	No contention
Central African Republic	2020	No Prior Election Data
Chad	2002	No contention
Chad	2011	No contention
Chile	2001	No contention
Chile	2005	No contention
Chile	2009	No contention
Chile	2013	No contention
Chile	2017	No contention
Colombia	2002	No contention
Colombia	2006	No contention
Colombia	2010	No contention
Colombia	2014	Not ACLED Covered
Colombia	2018	PR
Comoros	2004	No contention
Comoros	2004	No contention
Comoros	2009	No contention
Comoros	2009	No contention
Comoros	2015	No contention
Comoros	2015	No contention
Comoros	2020	No contention
Comoros	2020	Not ACLED Covered
Congo	2002	No Prior Election Data
Congo	2002	No Prior Election Data

Congo	2007	No contention
Congo	2007	No contention
Congo	2012	No contention
Congo	2012	No Prior Election Data
Congo	2017	No Prior Election Data
Congo	2017	No Prior Election Data
Costa Rica	2002	No contention
Costa Rica	2006	No contention
Costa Rica	2010	No contention
Costa Rica	2014	No contention
Costa Rica	2018	No contention
Cote d'Ivoire	2000	No Prior Election Data
Cote d'Ivoire	2001	No Prior Election Data
Cote d'Ivoire	2011	No Prior Election Data
Cote d'Ivoire	2016	In our Cases
Croatia	2000	Not ACLED Covered
Croatia	2003	No contention
Croatia	2007	No contention
Croatia	2011	No contention
Croatia	2015	No contention
Croatia	2016	No contention
Croatia	2020	No contention
Cuba	2003	Not ACLED Covered
Cuba	2008	No contention
Cuba	2013	Not ACLED Covered
Cuba	2018	No contention
Cyprus	2001	No contention
Cyprus	2006	No contention
Cyprus	2011	No contention
Cyprus	2016	No contention
Czech Republic	2000	No contention
Czech Republic	2000	No contention
Czech Republic	2002	No contention

Czech Republic	2002	No contention
Czech Republic	2002	No contention
Czech Republic	2004	No contention
Czech Republic	2004	No contention
Czech Republic	2006	No contention
Czech Republic	2006	No contention
Czech Republic	2006	No contention
Czech Republic	2008	No contention
Czech Republic	2008	Not ACLED Covered
Czech Republic	2010	No contention
Czech Republic	2010	No contention
Czech Republic	2010	No contention
Czech Republic	2012	No contention
Czech Republic	2012	No contention
Czech Republic	2013	No contention
Czech Republic	2014	No contention
Czech Republic	2014	No contention
Czech Republic	2017	No contention
Czech Republic	2018	No contention
Czech Republic	2018	No contention
Czech Republic	2020	No contention
Czech Republic	2020	No contention
Democratic Republic of Congo	2006	No Prior Election Data
Democratic Republic of Congo	2011	No Prior Election Data
Democratic Republic of Congo	2018	No Prior Election Data
Democratic Republic of Vietnam	2002	No contention
Democratic Republic of Vietnam	2007	No contention
Democratic Republic of Vietnam	2011	No contention
Democratic Republic of Vietnam	2016	No Prior Election Data
Denmark	2001	No contention
Denmark	2005	No contention
Denmark	2007	No contention
Denmark	2011	No contention

Denmark	2015	No contention
Denmark	2019	No contention
Djibouti	2003	No contention
Djibouti	2008	PR
Djibouti	2013	No Prior Election Data
Djibouti	2018	PR
Dominica	2000	No contention
Dominica	2005	No contention
Dominica	2009	No contention
Dominica	2019	No contention
Dominican Republic	2002	Not ACLED Covered
Dominican Republic	2006	No contention
Dominican Republic	2010	Not ACLED Covered
Dominican Republic	2016	Not ACLED Covered
Dominican Republic	2020	PR
East Timor	2007	No Prior Election Data
East Timor	2012	No contention
East Timor	2017	Not ACLED Covered
East Timor	2018	No contention
Ecuador	2002	No contention
Ecuador	2006	No contention
Ecuador	2009	No contention
Ecuador	2013	Not ACLED Covered
Ecuador	2017	Not ACLED Covered
Egypt	2000	No Prior Election Data
Egypt	2000	No Prior Election Data
Egypt	2000	No Prior Election Data
Egypt	2005	No Prior Election Data
Egypt	2007	No Prior Election Data
Egypt	2010	No Prior Election Data
Egypt	2010	No Prior Election Data
Egypt	2011	No Prior Election Data
Egypt	2011	No Prior Election Data

Egypt	2015	No Prior Election Shapefile
Egypt	2020	No Prior Election Data
Egypt	2020	No Prior Election Data
Egypt	2020	No Prior Election Data
El Salvador	2000	No contention
El Salvador	2003	No contention
El Salvador	2006	No contention
El Salvador	2009	No contention
El Salvador	2012	No contention
El Salvador	2015	No contention
El Salvador	2018	No contention
Equatorial Guinea	2004	No Prior Election Data
Equatorial Guinea	2008	No Prior Election Data
Equatorial Guinea	2013	No contention
Equatorial Guinea	2017	No Prior Election Data
Estonia	2003	No contention
Estonia	2007	No contention
Estonia	2011	No contention
Estonia	2015	No contention
Estonia	2019	No contention
Ethiopia	2000	No Prior Election Data
Ethiopia	2005	No Prior Election Data
Ethiopia	2010	No Prior Election Data
Ethiopia	2015	No Prior Election Shapefile
Federated States of Micronesia	2001	No contention
Federated States of Micronesia	2003	No contention
Federated States of Micronesia	2005	No contention
Federated States of Micronesia	2007	No contention
Federated States of Micronesia	2009	No contention
Federated States of Micronesia	2011	No contention
Federated States of Micronesia	2015	No contention
Federated States of Micronesia	2017	No contention
Federated States of Micronesia	2019	No contention

Fiji	2001	No Prior Election Data
Fiji	2006	No contention
Fiji	2014	No contention
Fiji	2018	No contention
Finland	2003	No contention
Finland	2007	No contention
Finland	2011	No contention
Finland	2015	No contention
Finland	2019	No contention
France	2002	No contention
France	2002	No contention
France	2007	No contention
France	2007	No contention
France	2012	No contention
France	2012	No contention
France	2017	No contention
France	2017	No contention
Gabon	2001	No contention
Gabon	2001	No contention
Gabon	2006	No Prior Election Data
Gabon	2011	No Prior Election Data
Gabon	2018	No contention
Gabon	2018	No contention
Gambia	2002	No contention
Gambia	2007	No contention
Gambia	2012	No Election Violence
Gambia	2017	In our Cases
Georgia	2003	Not ACLED Covered
Georgia	2004	No contention
Georgia	2008	Not ACLED Covered
Georgia	2012	No contention
Georgia	2016	No contention
Georgia	2016	Not ACLED Covered

Georgia	2020	No Prior Election Shapefile
German Federal Republic	2002	No contention
German Federal Republic	2005	No contention
German Federal Republic	2009	No contention
German Federal Republic	2013	No contention
German Federal Republic	2017	No contention
Ghana	2000	No Prior Election Data
Ghana	2004	No Prior Election Shapefile
Ghana	2008	No contention
Ghana	2012	In our Cases
Ghana	2016	In our Cases
Ghana	2020	In our Cases
Greece	2000	No contention
Greece	2004	No contention
Greece	2007	No contention
Greece	2009	No contention
Greece	2012	Not ACLED Covered
Greece	2012	Not ACLED Covered
Greece	2012	Not ACLED Covered
Greece	2012	Not ACLED Covered
Greece	2015	No contention
Greece	2015	No contention
Greece	2015	No contention
Greece	2015	No contention
Greece	2019	No contention
Grenada	2003	No contention
Grenada	2008	Not ACLED Covered
Grenada	2013	No contention
Grenada	2018	No contention
Guatemala	2003	Not ACLED Covered
Guatemala	2007	No contention
Guatemala	2011	Not ACLED Covered
Guatemala	2015	Not ACLED Covered

Guatemala	2019	PR
Guinea	2002	No contention
Guinea	2013	No Prior Election Data
Guinea	2020	In our Cases
Guinea-Bissau	2004	No contention
Guinea-Bissau	2008	PR
Guinea-Bissau	2014	No contention
Guinea-Bissau	2019	No contention
Guyana	2001	Not ACLED Covered
Guyana	2006	No contention
Guyana	2011	Not ACLED Covered
Guyana	2015	Not ACLED Covered
Guyana	2020	PR
Haiti	2000	Not ACLED Covered
Haiti	2000	Not ACLED Covered
Haiti	2006	No contention
Haiti	2006	Not ACLED Covered
Haiti	2010	Not ACLED Covered
Haiti	2015	Not ACLED Covered
Haiti	2015	Not ACLED Covered
Honduras	2001	No contention
Honduras	2005	Not ACLED Covered
Honduras	2009	Not ACLED Covered
Honduras	2013	Not ACLED Covered
Honduras	2017	Not ACLED Covered
Hungary	2002	No contention
Hungary	2002	No contention
Hungary	2006	No contention
Hungary	2006	No contention
Hungary	2010	No contention
Hungary	2010	No contention
Hungary	2014	No contention
Hungary	2018	Not ACLED Covered

Iceland	2003	No contention
Iceland	2007	No contention
Iceland	2009	No contention
Iceland	2013	No contention
Iceland	2016	No contention
Iceland	2017	No contention
India	2004	Not ACLED Covered
India	2009	Not ACLED Covered
India	2014	No contention
India	2019	No contention
Indonesia	2004	No contention
Indonesia	2009	No contention
Indonesia	2014	Not ACLED Covered
Indonesia	2019	PR
Iran	2000	No contention
Iran	2000	Not ACLED Covered
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Iran	2008	Not ACLED Covered
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Iran	2012	Not ACLED Covered
Iran	2016	No Prior Election Shapefile
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Iraq	2000	No contention
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Iraq	2014	Not ACLED Covered
Iraq	2018	No Prior Election Data
Ireland	2002	No contention
Ireland	2007	No contention
Ireland	2011	No contention
Ireland	2016	No contention

Ireland	2020	No contention
Israel	2003	No contention
Israel	2006	No contention
Israel	2009	No contention
Israel	2015	No contention
Israel	2019	No contention
Israel	2019	No contention
Israel	2019	No contention
Israel	2019	No contention
Israel	2020	PR
Italy	2001	No contention
Italy	2006	No contention
Italy	2008	No contention
Italy	2013	No contention
Italy	2018	No contention
Jamaica	2002	No contention
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Jamaica	2016	Not ACLED Covered
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Japan	2005	No contention
Japan	2005	No contention
Japan	2007	No contention
Japan	2009	No contention
Japan	2010	No contention
Japan	2012	No contention
Japan	2014	No contention
Japan	2016	No contention
Japan	2017	No contention

Japan	2019	No contention
Jordan	2003	No contention
Jordan	2007	Not ACLED Covered
Jordan	2010	Not ACLED Covered
Jordan	2013	Not ACLED Covered
Jordan	2016	No contention
Jordan	2020	PR
Kazakhstan	2004	Not ACLED Covered
Kazakhstan	2004	Not ACLED Covered
Kazakhstan	2007	Not ACLED Covered
Kazakhstan	2012	Not ACLED Covered
Kazakhstan	2016	Not ACLED Covered
Kenya	2002	No contention
Kenya	2007	In our Cases
Kenya	2013	In our Cases
Kenya	2017	In our Cases
Kiribati	2002	No Prior Election Data
Kiribati	2002	No Prior Election Data
Kiribati	2003	No contention
Kiribati	2003	No contention
Kiribati	2007	No contention
Kiribati	2007	No contention
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Kiribati	2011	No contention
Kiribati	2015	No contention
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Kiribati	2020	No contention
Kosovo	2010	Not ACLED Covered
Kosovo	2014	Not ACLED Covered
Kosovo	2017	No contention
Kosovo	2019	No contention
Kuwait	2003	No contention

Kuwait	2006	No contention
Kuwait	2008	No contention
Kuwait	2009	No contention
Kuwait	2012	Not ACLED Covered
Kuwait	2012	Not ACLED Covered
Kuwait	2013	No contention
Kuwait	2016	PR
Kuwait	2020	No contention
Kyrgyz Republic	2000	Not ACLED Covered
Kyrgyz Republic	2000	Not ACLED Covered
Kyrgyz Republic	2005	Not ACLED Covered
Kyrgyz Republic	2005	Not ACLED Covered
Kyrgyz Republic	2007	Not ACLED Covered
Kyrgyz Republic	2010	Not ACLED Covered
Kyrgyz Republic	2015	Not ACLED Covered
Kyrgyz Republic	2020	No Prior Election Data
Laos	2002	No contention
Laos	2006	No contention
Laos	2011	No contention
Laos	2016	No contention
Latvia	2002	No contention
Latvia	2006	Not ACLED Covered
Latvia	2010	No contention
Latvia	2011	No contention
Latvia	2014	No contention
Latvia	2018	No contention
Lebanon	2000	Not ACLED Covered
Lebanon	2005	Not ACLED Covered
Lebanon	2009	Not ACLED Covered
Lebanon	2018	No contention
Lesotho	2002	No contention
Lesotho	2007	No Election Violence
Lesotho	2012	No contention

Liberia	2005	No contention
Liberia	2011	In our Cases
Liberia	2014	No contention
Liberia	2017	No contention
Liberia	2020	PR
Libya	2012	No Prior Election Data
Libya	2014	PR
Liechtenstein	2001	No contention
Liechtenstein	2005	No contention
Liechtenstein	2009	No contention
Liechtenstein	2013	No contention
Liechtenstein	2017	No contention
Lithuania	2000	No contention
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Lithuania	2004	No contention
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Lithuania	2008	No contention
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Lithuania	2012	No contention
Lithuania	2016	No contention
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Lithuania	2020	No contention
Luxembourg	2004	No contention
Luxembourg	2009	No contention
Luxembourg	2013	No contention
Luxembourg	2018	No contention
Macedonia (FYROM)	2002	No contention
Macedonia (FYROM)	2006	No contention
Macedonia (FYROM)	2008	Not ACLED Covered
Macedonia (FYROM)	2011	No contention
Macedonia (FYROM)	2014	Not ACLED Covered
Macedonia (FYROM)	2016	Not ACLED Covered

Macedonia (FYROM)	2020	No Prior Election Data
Madagascar	2002	No contention
Madagascar	2007	No Prior Election Data
Madagascar	2013	No Prior Election Data
Madagascar	2019	No contention
Malawi	2004	No EV in Close Races
Malawi	2009	No contention
Malawi	2014	In our Cases
Malawi	2019	In our Cases
Malaysia	2004	No contention
Malaysia	2008	Not ACLED Covered
Malaysia	2013	Not ACLED Covered
Malaysia	2018	In our Cases
Maldives	2005	No Prior Election Data
Maldives	2009	No contention
Maldives	2014	No contention
Maldives	2019	No contention
Mali	2002	No contention
Mali	2002	No contention
Mali	2007	No contention
Mali	2007	No contention
Mali	2013	No contention
Mali	2020	No Prior Election Data
Malta	2003	No contention
Malta	2008	No contention
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Malta	2017	No contention
Marshall Islands	2003	No contention
Marshall Islands	2007	No contention
Marshall Islands	2011	No contention
Marshall Islands	2015	No contention
Marshall Islands	2019	No contention
Mauritania	2001	No Prior Election Data

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Mauritania	2006	No contention
Mauritania	2006	No contention
Mauritania	2013	No Prior Election Data
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Mauritius	2010	No contention
Mauritius	2014	No contention
Mauritius	2019	No contention
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Mexico	2003	No contention
Mexico	2006	No contention
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Mexico	2015	Not ACLED Covered
Mexico	2018	No Prior Election Data
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Moldova	2010	No contention
Moldova	2014	No contention
Moldova	2019	No Prior Election Data
Monaco	2003	No contention
Monaco	2008	No contention
Monaco	2013	No contention
Monaco	2018	No contention
Mongolia	2000	No contention
Mongolia	2004	No contention

Mongolia	2008	Not ACLED Covered
Mongolia	2012	No contention
Mongolia	2016	No contention
Mongolia	2020	PR
Montenegro	2009	No contention
Montenegro	2012	No contention
Montenegro	2016	PR
Montenegro	2020	No contention
Morocco	2002	No contention
Morocco	2007	No contention
Morocco	2011	No contention
Morocco	2016	No contention
Mozambique	2004	No contention
Mozambique	2009	PR
Mozambique	2014	PR
Mozambique	2019	PR
Myanmar (Burma)	2010	No Prior Election Data
Myanmar (Burma)	2015	In our Cases
Myanmar (Burma)	2020	In our Cases
Namibia	2004	No contention
Namibia	2009	No contention
Namibia	2014	No contention
Namibia	2019	No contention
Nauru	2000	No contention
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Nauru	2007	No contention
Nauru	2008	No contention
Nauru	2010	No contention
Nauru	2010	No contention
Nauru	2013	No contention
Nauru	2016	No Prior Election Data
Nauru	2019	No contention

Nepal	2017	No Prior Election Data
Nepal	2017	No Prior Election Data
Netherlands	2002	No contention
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Nicaragua	2006	No contention
Nicaragua	2011	No contention
Nicaragua	2016	Not ACLED Covered
Niger	2004	No contention
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Niger	2011	No contention
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Niger	2020	PR
Nigeria	2003	No Prior Election Data
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Nigeria	2015	In our Cases
Nigeria	2019	No Prior Election Data
North Korea	2003	No contention
North Korea	2009	No contention
North Korea	2014	Not ACLED Covered
Norway	2001	No contention

Norway	2005	No contention
Norway	2009	No contention
Norway	2013	No contention
Norway	2017	No contention
Oman	2003	No contention
Oman	2007	No contention
Oman	2011	No contention
Oman	2015	No contention
Oman	2019	No contention
Pakistan	2002	Not ACLED Covered
Pakistan	2008	Not ACLED Covered
Pakistan	2013	In our Cases
Pakistan	2018	In our Cases
Palau	2000	No contention
Palau	2004	No contention
Palau	2008	No contention
Palau	2012	No contention
Palau	2016	No contention
Palau	2016	No contention
Palau	2020	No contention
Panama	2004	No contention
Panama	2009	No contention
Panama	2014	Not ACLED Covered
Panama	2019	In our Cases
Papua New Guinea	2002	Not ACLED Covered
Papua New Guinea	2007	Not ACLED Covered
Papua New Guinea	2012	Not ACLED Covered
Papua New Guinea	2017	Not ACLED Covered
Paraguay	2003	No contention
Paraguay	2008	No contention
Paraguay	2013	No contention
Paraguay	2018	PR
Peru	2000	Not ACLED Covered

Peru	2001	No contention
Peru	2006	No contention
Peru	2011	No contention
Peru	2011	No contention
Peru	2016	Not ACLED Covered
Peru	2020	No contention
Philippines	2001	Not ACLED Covered
Philippines	2004	Not ACLED Covered
Philippines	2007	Not ACLED Covered
Philippines	2010	Not ACLED Covered
Philippines	2013	Not ACLED Covered
Philippines	2016	In our Cases
Philippines	2019	No Prior Election Data
Poland	2001	No contention
Poland	2005	No contention
Poland	2007	No contention
Poland	2011	No contention
Poland	2015	No contention
Poland	2019	No contention
Portugal	2002	No contention
Portugal	2005	No contention
Portugal	2009	No contention
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Portugal	2015	No contention
Portugal	2019	No contention
Romania	2000	No contention
Romania	2004	Not ACLED Covered
Romania	2008	No contention
Romania	2012	No contention
Romania	2016	No contention
Romania	2020	No contention
Russia (Soviet Union)	2003	Not ACLED Covered
Russia (Soviet Union)	2007	Not ACLED Covered

Russia (Soviet Union)	2011	Not ACLED Covered
Russia (Soviet Union)	2016	Not ACLED Covered
Rwanda	2003	No Prior Election Data
Rwanda	2008	No contention
Rwanda	2013	PR
Rwanda	2018	No Prior Election Data
Saint Kitts and Nevis	2000	No contention
Saint Kitts and Nevis	2004	No contention
Saint Kitts and Nevis	2010	No contention
Saint Kitts and Nevis	2015	No contention
Saint Lucia	2001	No contention
Saint Lucia	2006	No contention
Saint Lucia	2011	Not ACLED Covered
Saint Lucia	2016	No contention
Saint Vincent and the Grenadines	2001	No contention
Saint Vincent and the Grenadines	2005	Not ACLED Covered
Saint Vincent and the Grenadines	2010	No contention
Saint Vincent and the Grenadines	2015	Not ACLED Covered
Saint Vincent and the Grenadines	2020	No contention
Samoa/Western Samoa	2001	No contention
Samoa/Western Samoa	2006	No contention
Samoa/Western Samoa	2011	Not ACLED Covered
Samoa/Western Samoa	2016	No contention
San Marino	2001	No contention
San Marino	2006	No contention
San Marino	2008	No contention
San Marino	2012	No contention
San Marino	2016	No contention
San Marino	2016	No contention
San Marino	2019	No contention
Sao Tome and Principe	2002	No contention
Sao Tome and Principe	2006	No contention
Sao Tome and Principe	2010	No contention

Sao Tome and Principe	2014	No contention
Sao Tome and Principe	2018	No Prior Election Data
Saudi Arabia	2020	No contention
Senegal	2001	No contention
Senegal	2007	No contention
Senegal	2012	No contention
Senegal	2017	In our Cases
Serbia	2007	No contention
Serbia	2008	No contention
Serbia	2012	No contention
Serbia	2014	No contention
Serbia	2016	PR
Serbia	2020	No Prior Election Data
Serbia (Yugoslavia)	2000	No Prior Election Data
Serbia (Yugoslavia)	2003	No contention
Seychelles	2002	No Prior Election Data
Seychelles	2007	No Prior Election Data
Seychelles	2011	Not ACLED Covered
Seychelles	2016	No contention
Seychelles	2020	No contention
Sierra Leone	2002	No contention
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Sierra Leone	2012	No contention
Sierra Leone	2018	No Prior Election Shapefile
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Singapore	2011	No contention
Singapore	2015	No contention
Singapore	2020	Not ACLED Covered
Slovakia	2002	No contention
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Slovakia	2010	No contention
Slovakia	2012	No contention

Slovakia	2016	Not ACLED Covered
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Slovenia	2000	No contention
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Slovenia	2014	No contention
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Solomon Islands	2001	No contention
Solomon Islands	2006	Not ACLED Covered
Solomon Islands	2010	Not ACLED Covered
Solomon Islands	2014	No contention
Solomon Islands	2019	No contention
South Africa	2004	No contention
South Africa	2009	No contention
South Africa	2014	PR
South Africa	2019	No contention
South Korea	2000	No contention
South Korea	2004	No contention
South Korea	2008	No contention
South Korea	2012	No contention
South Korea	2016	No contention
South Korea	2020	No contention
South Ossetia	2009	No Prior Election Data
South Ossetia	2014	No Prior Election Data
South Ossetia	2019	No contention
Spain	2000	Not ACLED Covered
Spain	2004	Not ACLED Covered
Spain	2008	Not ACLED Covered
Spain	2011	No contention
Spain	2015	No contention
Spain	2016	Not ACLED Covered
Spain	2019	No contention

Spain	2019	Not ACLED Covered
Sri Lanka	2000	No contention
Sri Lanka	2001	Not ACLED Covered
Sri Lanka	2004	No contention
Sri Lanka	2010	PR
Sri Lanka	2020	No contention
Sudan	2000	No Prior Election Data
Sudan	2010	No Prior Election Data
Sudan	2015	No Prior Election Data
Surinam	2000	No contention
Surinam	2005	No contention
Surinam	2010	No contention
Surinam	2015	No contention
Surinam	2020	No contention
Swaziland	2003	No contention
Swaziland	2008	No Prior Election Data
Swaziland	2013	No Prior Election Data
Swaziland	2018	No Prior Election Data
Sweden	2002	No contention
Sweden	2006	No contention
Sweden	2010	Not ACLED Covered
Sweden	2014	No contention
Sweden	2018	No contention
Switzerland	2003	No contention
Switzerland	2007	No contention
Switzerland	2011	No contention
Switzerland	2015	No contention
Switzerland	2019	No contention
Syria	2003	No contention
Syria	2007	Not ACLED Covered
Syria	2012	Not ACLED Covered
Syria	2020	No contention
Taiwan	2001	No contention

Taiwan	2004	No contention
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Taiwan	2012	No contention
Taiwan	2016	No contention
Taiwan	2020	No contention
Tajikistan	2000	Not ACLED Covered
Tajikistan	2000	Not ACLED Covered
Tajikistan	2005	Not ACLED Covered
Tajikistan	2005	Not ACLED Covered
Tajikistan	2010	No contention
Tajikistan	2015	Not ACLED Covered
Tajikistan	2020	No Prior Election Data
Tanzania	2000	No Prior Election Data
Tanzania	2000	No Prior Election Data
Tanzania	2005	No contention
Tanzania	2010	No contention
Tanzania	2015	No Prior Election Data
Tanzania	2020	No Prior Election Shapefile
Thailand	2000	No contention
Thailand	2000	No contention
Thailand	2001	Not ACLED Covered
Thailand	2001	Not ACLED Covered
Thailand	2005	No contention
Thailand	2006	No contention
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Thailand	2007	Not ACLED Covered
Thailand	2008	Not ACLED Covered
Thailand	2011	No Prior Election Shapefile
Thailand	2014	In our Cases
Thailand	2019	No Prior Election Data
Togo	2002	No contention
Togo	2007	No contention
Togo	2013	PR

Togo	2018	PR
Tonga	2002	No contention
Tonga	2005	No contention
Tonga	2008	No contention
Tonga	2010	No contention
Tonga	2014	No contention
Tonga	2017	No contention
Trinidad and Tobago	2000	No contention
Trinidad and Tobago	2001	No contention
Trinidad and Tobago	2002	No contention
Trinidad and Tobago	2007	No contention
Trinidad and Tobago	2010	No contention
Trinidad and Tobago	2015	No contention
Trinidad and Tobago	2020	No contention
Tunisia	2004	No Prior Election Data
Tunisia	2009	PR
Tunisia	2014	PR
Tunisia	2019	PR
Turkey	2002	Not ACLED Covered
Turkey	2007	No contention
Turkey	2011	No contention
Turkey	2015	Not ACLED Covered
Turkey	2015	Not ACLED Covered
Turkey	2015	Not ACLED Covered
Turkey	2015	Not ACLED Covered
Turkey	2018	PR
Turkmenistan	2003	No contention
Turkmenistan	2004	No contention
Turkmenistan	2008	No contention
Turkmenistan	2013	No contention
Turkmenistan	2018	No contention
Tuvalu	2002	No contention
Tuvalu	2006	No contention

Tuvalu	2010	No contention
Tuvalu	2015	No contention
Tuvalu	2019	No contention
Uganda	2001	No Prior Election Data
Uganda	2006	No Prior Election Data
Uganda	2011	No Prior Election Shapefile
Uganda	2016	No Prior Election Shapefile
Ukraine	2002	Not ACLED Covered
Ukraine	2006	No contention
Ukraine	2007	No contention
Ukraine	2012	Not ACLED Covered
Ukraine	2014	Not ACLED Covered
Ukraine	2019	In our Cases
United Kingdom	2001	No contention
United Kingdom	2005	No contention
United Kingdom	2010	No contention
United Kingdom	2015	No contention
United Kingdom	2017	Not ACLED Covered
United Kingdom	2019	Not ACLED Covered
United States of America	2000	No contention
United States of America	2002	No contention
United States of America	2004	No contention
United States of America	2006	No contention
United States of America	2008	No contention
United States of America	2010	No contention
United States of America	2012	No contention
United States of America	2014	No contention
United States of America	2016	Not ACLED Covered
United States of America	2018	No contention
United States of America	2020	In our Cases
Uruguay	2004	No contention
Uruguay	2009	No contention
Uruguay	2014	No contention

Uruguay	2019	No contention
Uzbekistan	2004	No contention
Uzbekistan	2005	No contention
Uzbekistan	2009	No contention
Uzbekistan	2014	Not ACLED Covered
Uzbekistan	2019	No contention
Uzbekistan	2020	No contention
Vanuatu	2002	Not ACLED Covered
Vanuatu	2004	Not ACLED Covered
Vanuatu	2008	No contention
Vanuatu	2012	No contention
Vanuatu	2016	No contention
Vanuatu	2020	No contention
Venezuela	2000	Not ACLED Covered
Venezuela	2005	Not ACLED Covered
Venezuela	2010	No contention
Venezuela	2015	Not ACLED Covered
Venezuela	2020	In our Cases
Yemen	2003	Not ACLED Covered
Zambia	2001	No EV in Close Races
Zambia	2006	No Election Violence
Zambia	2011	In our Cases
Zambia	2016	In our Cases
Zimbabwe	2000	No Prior Election Data
Zimbabwe	2005	In our Cases
Zimbabwe	2008	In our Cases
Zimbabwe	2013	In our Cases
Zimbabwe	2018	In our Cases

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