

UNIVERSITY OF CALIFORNIA, MERCED

The Negative Effects of "Good" Institutions on Political Violence

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in

Political Science

by

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Dedication

To my family, for their endless love, support, and encouragement.

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The Negative Effects of "Good" Institutions on Political Violence

Abstract

In my dissertation, I investigate the negative effects of "good" institutions on political violence. I define "good" institutions as seemingly well-intended institutional changes or decisions. Across three stand-alone articles, I show that "good" institutions do not always reduce political violence because the actors who design and implement institutions tailor them to meet their own interests, which are often not reducing political violence.

In the first paper, I examine how granting regional autonomy changes the level of insurgent violence. Because regional autonomy gives more independence to lower tiers of government, it is commonly thought to reduce insurgent violence. I argue that elite-captured regional autonomy enables elites to expropriate citizen wealth, generating citizen grievances that fuel insurgent violence. To test the implications of my theory, I investigate the effect of the creation of two new, autonomous Indian states—Chhattisgarh and Jharkhand—on insurgent violence. Using difference-in-difference and geographical regression discontinuity designs, I find the creation of these states to be associated with statistically and substantively significant increases in Maoist violence. More importantly, the relationship between these new autonomous states and violence is stronger when elites are better able to exploit local resources.

The second paper in my dissertation examines the effect of investigations by the International Criminal Court (ICC) of alleged human rights crises on non-state civil conflicts. To date, scholars interested in the effect of the ICC on civil conflicts have treated the "state" as a unitary actor. In this paper, I argue that the ICC's investigations of government leaders and of rebel leaders have different effects on civil conflict. Because of its biased case selection process, when the ICC investigates government leaders, it constrains governments, provides incentives for the rebel groups to fight, and leads to more non-state conflicts—that is, conflicts between rebels. When the ICC investigates rebel leaders, it does not constrain rebel groups, strengthens the government's control, and reduces non-state conflicts. I evaluate the implications of the theory with difference-in-difference tests using data on non-state conflicts cross-nationally and find support.

My third dissertation paper looks at freedom of information (FOI) laws and protests. While more transparency in government should theoretically increase citizens' satisfaction and make them less likely to participate in protests, I argue that providing citizens with information about the wrongdoings of lower-tier officials without giving them ways to punish those officials leads to more protests. Using a difference-in-difference design and cross-country data, I find that adopting FOI laws is associated with statistically and substantively significant increases in the number of protests. More importantly, I found the relationship between the adoption of FOI law and the number of protests is conditioned by

electoral democracy, a measure of ways to punish government officials. When there are ways to punish those officials, FOI laws do not lead to more protests.

In full, the three papers in my dissertation contribute to the literature on institutions and political violence by examining the negative effects of seemingly well-intended institutions on political violence and carefully considering the preferences of the actors who designed or implemented these institutions. The dissertation contributes to the discussion of the effectiveness of institutional decisions on reducing political violence—why some institutional decisions failed to reduce political violence? My work shows that different institutional designs can be subjected to this common rule—institutions are products of actor’s preferences. We should not expect these elite actors to design or implement institutions to fulfill ordinary people’s goals rather than their own.

Part I

Regional Autonomy and Insurgent Violence

Abstract

Does regional autonomy increase or alleviate insurgent violence? I argue that elite-captured regional autonomy enables elites to expropriate citizen wealth, generating citizen grievances that fuel insurgent violence. To test the implications of my theory, I investigate the effect of the creation of two new, autonomous Indian states—Chhattisgarh and Jharkhand—on insurgent violence. Using difference-in-difference and geographical regression discontinuity designs, I find the creation of these states to be associated with statistically and substantively significant increases in Maoist violence. More importantly, the relationship between these new autonomous states and violence is stronger when elites are better able to exploit local resources.

1.1 Introduction

Regional autonomy is often expected to decrease insurgent violence by alleviating citizens' grievances and meeting their needs. On November 1, 2000, in an effort to accommodate citizen demands for better political representation and faster economic development, the Indian government granted autonomy to two new regions by creating two new states: Jharkhand was carved out of Bihar, and Chhattisgarh was carved out of Madhya Pradesh. Since their creation, however, Chhattisgarh and Jharkhand have suffered from Maoist insurgency. In the case of India, then, regional autonomy seems to have led to more insurgent violence. Under what conditions does granting regional autonomy aggravate rather than alleviate insurgent violence?

Because regional autonomy gives additional independence to lower tiers of government, it is commonly argued to reduce civil conflict and insurgent violence. With more autonomy, groups of citizens have more control over their political, social, and economic affairs (Lijphart 1977, 1996, Lustick, Miodownik and Eidelson 2004); Local government better addresses the needs of citizens, decreasing the attractiveness of joining insurgent movements (Cederman, Gleditsch and Buhaug 2013, Collier and Hoeffler 2004). However, some scholars suggest that granting regional autonomy increases civil conflict and insurgent violence by promoting social group identities (Cornell 2002, Hardgrave 1993, Hechter 2000, Kymlicka 2005), producing policies that favor certain groups over others (Horowitz 1992, Suberu 1993), and changing the power relationship between the central government and local groups (Cornell 2002, Kymlicka 2005, Leff 1999, Roeder 1991, Snyder 2000). Empirically, there is mixed evidence whether regional autonomy alleviates or promotes civil conflict and insurgent violence (Brancati 2006, Christin and Hug 2012, Ishiyama 2009, Saideman et al. 2002). The disparities in theories and empirical studies suggest that some conditionalities have been overlooked when analyzing the complex relationship between different actors in the process of gaining regional autonomy.

In this paper, I argue that regional autonomy leads to conflict under an as-yet-unstudied condition: elite capture. Regional autonomy leads to conflict when it allows elites to manipulate institutions for their own interest, thereby creating political and economic grievances that fuel insurgent violence. To test the implications of my theory, I investigate the effect of the creation of two new, autonomous Indian states — Chhattisgarh and Jharkhand — on insurgent violence. In the case of India, the creation of these two new states enabled the elites to tailor new institutions to their needs, especially in the mining sector, leading to more Maoist insurgent incidents. Using a difference-in-difference research design that exploits cross-sectional and temporal variation in the creation of new states and a geographical regression discontinuity design utilizing geographic characteristics of new states' creation, I show that regional autonomy is statistically and substantively associated with increases in incidents related to the Maoist insurgency. Using data on international resource prices and the existence of resources in districts within Chhattisgarh and Jharkhand, I show that when the value of elite-exploitable minerals increases, regional autonomy leads to more Maoist insurgent incidents.

This paper presents an important argument for how regional autonomy increases violent

conflicts; that is granting regional autonomy creates opportunities for elites to manipulate and tailor new institutions to meet their interests at the expense of ordinary citizens. Granting regional autonomy does not necessarily give citizens control over their political, social, and economic affairs. When regional autonomy is captured by elites, it creates political and economic grievances that lead to insurgent violence. Although I test the theory using data on the creations of new states and the Maoist insurgency in India, my theory about elite capture in regional autonomy and political violence is generalizable to other developing democracies. The argument has important policy implications: if the government wants to improve citizens' political rights and reduce political violence, democratization should be implemented carefully to avoid elite capture.

1.2 Regional Autonomy and Civil Conflict

Because regional autonomy grants greater independence to lower tiers of government, it is commonly thought to reduce civil conflict and insurgent violence. With more autonomy, groups of citizens are expected to have more control over their issues, protect themselves from the central government, and be better able to ensure the provision of public goods (Lijphart 1977, 1996, Lustick, Miodownik and Eidelson 2004). When citizens' needs are better addressed, scholars argue, they will find the possibility of joining an insurgent movement less attractive (Cederman, Gleditsch and Buhaug 2013, Collier and Hoeffler 2004). In addition, when groups are empowered politically through regional autonomy, they are less likely to seek power through violent means. Thus, scholars argue that regional autonomy reduces civil conflict by bringing government closer to the people, providing opportunities for citizens to participate in government, and giving groups control over their political, social, and economic affairs (Brancati 2006). Regional autonomy leads to better economic development (Asher and Novosad 2015), which can reduce conflict by reducing poverty (Dasgupta, Gawande and Kapur 2017).

However, a number of scholars suggest that granting regional autonomy increases civil conflict and insurgent violence. One group of theories argues that regional autonomy intensifies conflicts by promoting social group identities, such as ethnic, religious, or regional identities (Cornell 2002, Hardgrave 1993, Hechter 2000, Kymlicka 2005). Stronger identities lead to a stronger out-group bias that fuels conflicts between groups. Regional autonomy may also intensify conflict by producing policies that favor certain groups over others (Horowitz 1992, Suberu 1993). When there are hierarchical relationships between groups, i.e., when some groups dominate others, conflicts are more likely. Alternatively, regional autonomy increases conflicts by changing the power relationship between the central government and local groups. Regional autonomy may empower local groups, weaken central government, and eventually lead to more conflicts (Cornell 2002, Kymlicka 2005, Leff 1999, Roeder 1991, Snyder 2000).

Empirical evidence is mixed regarding whether regional autonomy alleviates or promotes civil conflict or insurgent violence. Studies on specific countries and regions that implemented regional autonomy show mixed evidence. In Canada (Simeon 2004), Spain (Beramendi and Máiz 2004), and Indonesia (Diprose 2009), regional autonomy alleviated

civil conflict, whereas in Kosovo (Gjoni, Wetterberg and Dunbar 2010), Colombia (Eaton 2006), and Sierra Leone (Edwards, Yilmaz and Boex 2015, Rosenbaum and Rojas 1997), regional autonomy increased civil conflict. Cross-country large-N studies have also produced mixed findings (Brancati 2006, Christin and Hug 2012, Ishiyama 2009, Saideman et al. 2002).

The disparities in theories and empirical studies suggest that some conditionalities may have been overlooked when analyzing the complex relationship between different actors in the process of gaining regional autonomy. I argue that regional autonomy leads to conflict under an as-yet-unstudied condition: elite capture. In development studies, scholars have noted the danger of local elite capture in regional autonomy (Bardhan 2002, Bardhan and Mookherjee 2000, Reinikka and Svensson 2004, Verbrugge 2015) and found a link between local elite capture and conflicts (Daley 2006, Fanthorpe 2005). However, few studies have built a generalizable theory that connects regional autonomy, elite capture, and violent conflict. In what follows, I build a generalizable theory explaining how regional autonomy can increase insurgent violence under the condition of local elite capture and empirically test implications of the theory using data on the creation of new states and Maoist conflicts in India.

1.3 Elite-captured Regional Autonomy

To develop a general theory about elite capture in regional autonomy and its effect on insurgent violence, I first define elite capture as a process whereby public resources are taken for the benefit of a few individuals of superior social and political status at the cost of the welfare of the larger population.¹ I am interested in the behavior of two actors: a group of citizens and a group of elites.

Citizens can be any group of people who seek more benefits by changing current institutions or acting on their own; groups of citizens are not limited to those with membership in ethnic or social groups. I assume citizens desire more power—whether *de jure* or *de facto*. Power is defined as the ability to realize one’s preferences.² Citizens are assumed to prefer regional autonomy because it will give them more *de jure* power, specifically better representation in the decentralized regime. These assumptions are not controversial generally or specifically in the case of India. Citizens in Northern Ireland, Scotland, and Wales demanded more autonomy for better representation in the proposed new government. Countries in Latin America adopted regional autonomy to increase citizens’ participation and representation (West 2015). In the case of India, people in scheduled tribes and castes (ST/SC) have mobilized to request more autonomy (Tillin 2011).³ Although citizens can gain *de facto* power through insurgent violence, that approach is costly and uncertain. When

¹This definition of elite capture is not explicitly defined but implicitly implied in several economics and development studies(e.g., Platteau 2004, Reinikka and Svensson 2004).

²*De jure* power is the power allocated by political institutions such as voting legislation or election, and *de facto* power is power that is held by a social group as a consequence of wealth, violence, or other means (Bertocchi and Dimico 2017).

³ST/SCs are officially recognized groups that are historically marginalized and disadvantaged in India.

the benefits are the same from regional autonomy and insurgent violence, citizens prefer regional autonomy over insurgent violence.

Elites are politicians who might benefit from changing the current institutions.⁴ I assume elites also want power—whether it is de jure or de facto. Note that this assumption differs from the assumption that politicians want to stay in power. I assume elites want to maximize their power either by staying in positions that give them de jure power or capturing other de facto powers. Politicians face incentives to be elected and re-elected, as well as to engage in corruption to capture rent. This assumption is not controversial because, in developing countries, government officials tend to materialize their de jure power through corrupt channels. In India, for example, low in-system material gains for politicians and abundant out-system opportunities have led to a corruption epidemic (Quah 2008). Resources collected through corruption become politicians' de facto power. Regional autonomy creates opportunities for elites to capture and manipulate new institutions to produce de facto power and benefits at the expense of citizens.

Perhaps controversially, I assume that citizens fail to anticipate the extent to which the elite capture of decentralized institutions will accompany regional autonomy. Citizens know that there will be some elite capture, because corruption is expected. But they expect that regional autonomy will mitigate local capture by increasing accountability through better representation. However, citizens underestimate the level of elite capture caused by increased autonomy. In many settings, citizens are not perfectly informed about the consequences of policies or institutional changes (Jerit and Zhao 2020, Kuklinski et al. 2000, Madrid-Morales et al. 2021, van Kessel, Sajuria and Van Hauwaert 2021), and information about policies or institutional changes is influenced by the attitudes of political elites (Darmofal 2005, Gabel and Scheve 2007, Guisinger and Saunders 2017). If the citizens knew how bad elite capture of regional autonomy would be, they would not prefer regional autonomy in the first place.

Regional autonomy increases insurgent violence through elite manipulation of new institutions. Because elites have an information advantage, citizens become convinced that it is best for them to work with elites to gain regional autonomy. However, local governments can be captured by elites (Bardhan and Mookherjee 2000). Movements pushing for regional autonomy can partially mask elite interests and manipulations, so there should be no automatic assumption that the autonomy would lead to greater social or political justice (Mawdsley 2002). After gaining regional autonomy, elites become more autonomous and capture the benefits of regional autonomy for themselves at the expense of citizens.^{5 6}

⁴Elites can be either local or national political elites, as long as they can receive benefits from changing the current institution.

⁵See Ribot et al. (2002) and Treisman (2002) for similar arguments.

⁶One might wonder whether this implication is out-of-equilibrium because citizens can learn from past experiences and choose not to work with elites. This can certainly be the case when citizens have interacted with elites on similar institutional changes before. However, because these types of citizen interactions with elites happen only rarely, I assume inexperienced citizens are easily led to believe that the payoff of regional autonomy is high for both citizens and elites. However, the true consequences for citizens are even worse than the results of failing to gain regional autonomy; for elites, the true payoffs are much higher. Because citizens are misinformed by elites, they learn the true payoffs only after gaining regional autonomy.

When citizens fail to receive the benefits they expected from regional autonomy, they realize that elites have captured the process. Even worse, local capture is more rampant than it was before autonomy. Because citizens acted collectively to request autonomy, they are also able to engage in collective insurgency. When citizens realize that captured regional autonomy does not yield benefits, they become more likely to join an insurgency.⁷

I make three key theoretical assumptions. First, I assume political elites face incentives to be elected and re-elected, as well as to engage in corruption with the aim of rent-seeking. Second, I assume citizens can be misled by elites. Third, I assume the value of public resources is large enough such that elites are willing to betray citizens. Although these assumptions are consistent with dynamics in India, they are commonly met in other developing democracies as well. In Indonesia, for example, scholars have also expressed concerns about the extent to which elite capture has limited the effect of fiscal decentralization on civil conflict (e.g., Diprose 2009, Sugiyanto et al. 2018).

If the three key assumptions are relaxed, the theory yields different implications that can be generalized to even more settings. First, if political elites seek power only within the political system with no intention to seek rent through corruption, citizens and elites can work together to gain regional autonomy without elite capture. Such regional autonomy should alleviate civil conflict. Second, if citizens have sufficient information so that they are not easily misguided by elites, citizens choose not to work with elites and no regional autonomy is gained—the level of civil conflict thus stays the same. Third, if the value of resources is not large enough, elites will not be interested in gaining regional autonomy. In this case, elites will not work with citizens to gain regional autonomy and the level of civil conflict won't change. On the other hand, if the value of public resources is not only large enough, but also increasing to be larger, I expect elite-captured regional autonomy to happen. Also, as the value of elite-exploitable public resources increases, the level of elite capture increases, generating more grievances that lead to more violence. All these implications can be tested in different countries that meet the relaxed assumptions. In this paper, I first test the implication with three key assumptions. Then I test the implication with the assumption that the value of public resources is not only large enough, but increasingly to be larger.

Hypothesis 1. *Relative to regions that do not gain autonomy, when regions move from no autonomy to elite-captured autonomy, we expect to see an increase in insurgent violence.*

Hypothesis 2. *As the value of elite-exploitable public resources increases in all regions, the increase in insurgent violence in regions with elite-captured autonomy—relative to regions without regional autonomy—becomes larger.*

⁷In a democratic setting, citizens can choose to vote the “bad” local elites out of office instead of joining insurgent violence. Why don't citizens use their democratic system to solve the problem without costly violence? I argue that in a developing democracy, vote-buying is common. In the case of India, elite parties outsourced the task of mobilizing poor voters to non-party affiliates that provide basic services to appeal to poor communities (Thachil 2014). Vote-buying does not have to pacify all members of the citizenry because part of the marginalized group, combined with local elites' core supporters, is sufficient to keep the local elites in power. The citizens, on the other hand, are disunified by vote-buying and less likely to vote local elites out of office, and unsatisfied citizens are likely to join insurgent violence.

1.4 Research Design

On November 1, 2000, Jharkhand was carved out of Bihar and Chhattisgarh was carved out of Madhya Pradesh to form two new Indian states. Jharkhand was formed from 18 former districts in South Bihar, and Chhattisgarh was formed from 16 districts in southeast Madhya Pradesh. The statehood of each was made possible by a combination of influences: “bottom-up”—local people expressing their needs for a new state—and “top-down”—a large national party, in this case, the Bharatiya Janata Party (BJP), pushing for the statehood. In other words, regional autonomy was possible because of the collaboration between citizens and elites.

In the case of Jharkhand, the Jharkhand Mukti Morcha (JMM) party was formed in the 1970s to unite scheduled caste and scheduled tribe (ST/SC) workers against land transfers to industries and non-tribal people. JMM demanded statehood with the vision to protect the interests of ST/SC people, but it focused more on protests and movements for the rights of ST/SC people. Gradually, the idea of a Jharkhand state became widespread and was adopted by other political forces. In the 1990s, BJP supported the demand for statehood with a vision of a state that would support the development of industry and natural resource extraction and help to bring tribes into the economic mainstream and into Hinduism (Tillin 2011). The BJP used this vision of a state to win the support of both the BJP’s core supporters—higher castes and SC/ST people. Upper castes saw benefit in building industries to extract natural resources, and SC/ST people expected to benefit from fast economic development and better representation. With the support from higher castes and some of the ST/SC, the BJP helped make Jharkhand a new state. The case of Chhattisgarh is similar to that of Jharkhand, but the local people who expressed needs were not SC/ST. Organizations like Chhattisgarh Mines Shramik Sanph and its derivative, Chhattisgarh Mukti Morcha, protected SC/ST labor and other laborers, but were not interested in the demand for statehood. Leaders of another group, Other Backwards Classes (OBCs), representing the majority of non-SC/ST people, demanded statehood for reasons like garnering the wealth from natural resources and the neglect of agricultural development and infrastructure (Tillin 2013). These leaders joined BJP, and BJP used a similar development vision to attract supporters from different groups. Eventually, Chhattisgarh was made possible.

Instead of fulfilling the vision of benefiting everyone with fast development with natural resources, unfortunately, regional autonomy enabled elite capture. Unlike old states, new states need to create their own capitals, state parliaments, ministries, courts, and other institutions of government (Mawdsley 2002). Because state governments have regulatory and executive power, new states can create and implement new policies, rules, and regulations that differ from those in the old states regarding health, education, infrastructure, mining, and economic development. The opportunity to create new institutions in new states enables elites to shape those new institutions and tailor them to their own interests with considerable autonomy.

While there are many ways through which elites can use the new institutions to seek rents, one of the ways is taking control of the mining sector. In India, the Mines and Minerals Development and Regulation (MMDR) Act of 1957 regulates the power of central and

state governments in the mining sector. Although the central government has the power to make rules regulating prospecting licenses or mining leases, issuing directions, and amending the MMDR act, state governments have two important powers: (1) granting prospecting licenses and mining leases with respect to minerals; and (2) preventing illegal mining, transportation and storage of minerals.⁸ Because states have the power to grant mining licenses, in the process of creating new institutions, elites can legally manipulate the allocation of licenses by exerting greater control over the institution that grants licenses. Because states have the power to regulate illegal mining, elites can choose not to create new regulations preventing illegal mining—in other words, to permit mining activities that were previously illegal. In return, elites seek rent by taking bribes from mining companies. The connection between business and illegal political activities and corruption in the Indian mining sector is well-documented and analyzed (e.g., Lahiri-Dutt 2007, Miklian and Carney 2013, Padel and Das 2010).⁹ What's new here is the opportunity to create new institutions in the new states—an opportunity that turns the two powers of state governments over the mining sector into two new opportunities to legally or illegally grant local elites permission to engage in mining and profit from it.

With permission from elites, mining companies acquired more land for natural resources extraction, primarily in areas traditionally inhabited by SC/STs. This has led to more loss of tribal land, more displacement of tribal communities, and more damage to the livelihoods of indigenous people (Areeparampil 1996, Gomes 2015, Lahiri-Dutt, Krishnan and Ahmad 2012). Natural resource extraction fueled rapid development at the expense of SC/STs. In response, dissatisfied groups, especially SC/STs, turned to Maoists who claim to fight for the interests of disadvantaged groups, including SC/STs. Maoist conflicts in India started as an uprising against unjust land reform in Naxalbari, a village in West Bengal, in 1967. The uprising was repressed by force in about three months, but it inspired revolutions in rural southern and eastern India. The insurgents called themselves Naxalites and claimed to support Mao Zedong's political ideology, which urges peasants and lower-class groups to take over the government of the upper classes through violent means. Land rights for lower classes are at the core of Naxalite ideology. By 1972, the Naxalites seemed to have been successfully put down, and in the early 1990s the Indian government started granting mining licenses to private and multinational corporations.¹⁰ In early 2000, the Naxalites started to re-consolidate. Various Naxalite groups—the Communist Party of India (Marxist/Leninist), People's War (People's War Group), and the Maoist Communist Centre of India (MCCI)—merged to form the Communist Party of India (Maoist) on October 14, 2004. Since the Maoist group attracted landless and subsistence peasants whose livelihoods were threatened by mining and who were dissatisfied with the results of regional autonomy, more ST/SCs joined the Maoist insurgent group that represented their interests, which led to more Maoist

⁸The full text of the MMDR Act can be found at:
<https://mines.gov.in/writereaddata/UploadFile/MMDR%20Act,1957.pdf>

⁹See also: <https://www.bbc.com/news/world-south-asia-14486290>; <https://www.reuters.com/article/india-coal-jharkhand-dhanbad-coalindia/special-report-coal-mafia-stokes-indias-power-crisis-idINDEE94D00B20130514>

¹⁰See <https://www.aljazeera.com/indepth/features/2017/04/india-maoist-rebels-explainer-170426132812114.html>

insurgent violence. Landless and small peasants formed the social base of and SC/STs formed the caste base of Maoist insurgency (Bhatia 2005, Guha 2007).¹¹

One might argue that conflicts are not the only solution to disagreement between the citizens and the governments. Empirically, there were only two negotiations or direct talk between state government leaders and Maoists leaders before 2011: one between West Bengal government and Maoists in 2011 and one between Andhra Pradesh government and Maoists in 2004.¹² Violence, not negotiation, was the main consequence of disagreement between the citizens and the governments.

Because granting regional autonomy creates new opportunities for elites to manipulate institutions in their own interests, it creates more political and economic grievances that fuel insurgent groups and eventually lead to more insurgent violence. After regional autonomy is granted, elites and mining companies use new institutions to seek rent in the mining sector at the expense of local people and this leads to more Maoist insurgent violence. I expect:

Operational Hypothesis 1. *Compared to old states, the creation of new states increases Maoist insurgent violence in new states.*

Operational Hypothesis 2. *As the value of elite-exploitable mining resources increases in all states, the increase in Maoist insurgent violence in new states—relative to old states—becomes larger.*

To estimate the effect of elite-captured regional autonomy on insurgent violence, I take advantage of the creation of new states in India in 2000 to use difference-in-difference as my identification strategy. To test my hypotheses, I use data on Maoist violence in India that covers 116 districts in two old states and two new states from the first quarter of 1990 to the last quarter of 2009 at district-quarter-year level (Dasgupta, Gawande and Kapur 2017).¹³ In this section, I first introduce the dependent variable and its measures: total incidents of Maoist violence. Then I discuss the independent variable and its measure: the creation of new states. After that, I discuss the identification strategy.

1.4.1 Operationalization

The dependent variable is insurgent violence. I use a count of violent Maoist incidents collected by Dasgupta, Gawande, and Kapur (2017) to measure Maoist insurgent violence in India. The dataset overcomes significant underreporting problems as well as urban and temporal reporting bias, using local language to code violent Maoist incidents. Notably, its temporal dimension is from 1990 to 2009. It covers the pre-2000 period, which enables me to use difference-in-difference as the identification strategy. Other sources of data—for

¹¹One alternative explanation is that the increased violence is due to Maoists moved to districts in these new states. The movement of Maoists can also be explained by increased dissatisfaction with the new states.

¹²See <https://www.idsa.in/idsacomments/>

NegotiatingwiththeMaoistsLessonsfromtheAndhraexperience_pvrman_131011

¹³New districts are carved out of old districts over the time frame of analysis. To have a balanced panel, the data are aggregated to the level of 2001 district boundaries (Dasgupta, Gawande and Kapur 2017).

example, the data from the South Asia Terrorism Portal (SATP)—include only observations starting in 2004.¹⁴ I use the number of incidents related to Maoist conflicts because elite-captured regional autonomy is likely to increase the magnitude of insurgency rather than its severity. Elite behaviors such as seizing the land of citizens for new mining activities are likely to cause more conflict incidents in different locations but not necessarily make conflicts more severe. The data set covers 116 districts in two old states and two new states between 1990 and 2009. The incidents range from 0 to 69 per quarter in a district.

The independent variable is elite-captured regional autonomy, measured by the creation of new states in India in 2000. As noted above, in November 2000, Chhattisgarh was carved out of Madhya Pradesh and Jharkhand was carved out of Bihar.¹⁵ Thus, the independent variable is coded as a binary variable that takes a value of 1 following the creation of the two new states for districts that are selected in the new state in each quarter. Districts that belong to Chhattisgarh and Jharkhand are coded as 1 after the fourth quarter of 2000.

1.4.2 Identification Strategy

Given that the creation of new states happened in some districts but not others, I use difference-in-difference as the identification strategy. The independent variable is coded as a binary variable that takes a value of 1 following the creation of the two new states for districts that are selected into the new state in each quarter.¹⁶ With this research design, I am able to compare the changes in Maoist conflict incidents in new state districts before and after the creation of the new states to the changes in Maoist conflict incidents in old state districts before and after the creation of the new states.

I use panel linear models to test the hypotheses because it is easy to interpret, and the calculation of clustered standard errors of interaction effects of the linear model is straightforward. Since the dependent variable, insurgent violence, is measured by Maoist incidents, which are count variables, quasi-Poisson maximum likelihood regression models are used as robustness checks for the main results.¹⁷ The regression equation is:

¹⁴SATP data can be found at <http://www.satp.org/>

¹⁵Another state, Uttarakhand, was separated from Uttar Pradesh in November 2000. This study focuses on Chhattisgarh and Jharkhand because those two states are heavily affected by Maoist conflicts. Studies of Maoist conflicts (Dasgupta, Gawande and Kapur 2017, Hoelscher, Miklian and Vadlamannati 2012) in general focus on the so-called “red corridor”, the six central and eastern states: Andhra, Pradesh, Bihar, Chhattisgarh, Jharkhand, Orissa, and West Bengal, in which more than 90 percent of Maoist conflict deaths happen. See <http://www.satp.org/satporgtp/countries/india/database/conflictmap.htm>

¹⁶district-fixed effects and quarterly year dummies are included in all specifications. District fixed effects control time-invariant omitted variables that could lead to biased estimates. Quarterly year dummies capture the influence of time-series trends that affect the estimation. This specification is generally used as a regression form of difference-in-difference with treatments in multiple periods.

¹⁷The use of fixed effects in MLE Poisson does not suffer from the problem of incidental parameters, unlike a fixed effects maximum likelihood estimation (MLE) logit model. Thus, its estimates are unbiased (Allison and Waterman 2002, Cameron and Trivedi 2013, Greene 2004). As shown in the appendix, the results are robust to using 1) a negative binomial model and 2) a quasi-Poisson model to address overdispersion. (Zeileis, Kleiber and Jackman 2008); the quasi-Poisson results (including standard errors) are the same as in this model as in the Poisson model with cluster standard errors.

$$Violence_{it} = \beta_0 + \beta_1 New\ State_{i,t} + \delta X' + \gamma_i + \eta_t + \epsilon_{i,t}$$

$Violence_{it}$ represents the incidents of Maoist conflict in district i in quarterly year t . $New\ State_{i,t}$ is the creation of new states for district i in quarterly year t . X' are controls for treatment assignment process I will discuss later and are included in some specifications as robustness checks. γ_i is district fixed effects that absorb the effect of unobserved time-invariant variables. η_t is quarterly year fixed effects that absorb the effect of common time shocks and trends to districts. All standard errors presented in the following results are robust standard errors clustered at the district level to account for the serial correlation within a district. Table 1.5 in the appendix shows descriptive statistics for the main variables in the analysis. The distribution of incidents is highly skewed.¹⁸

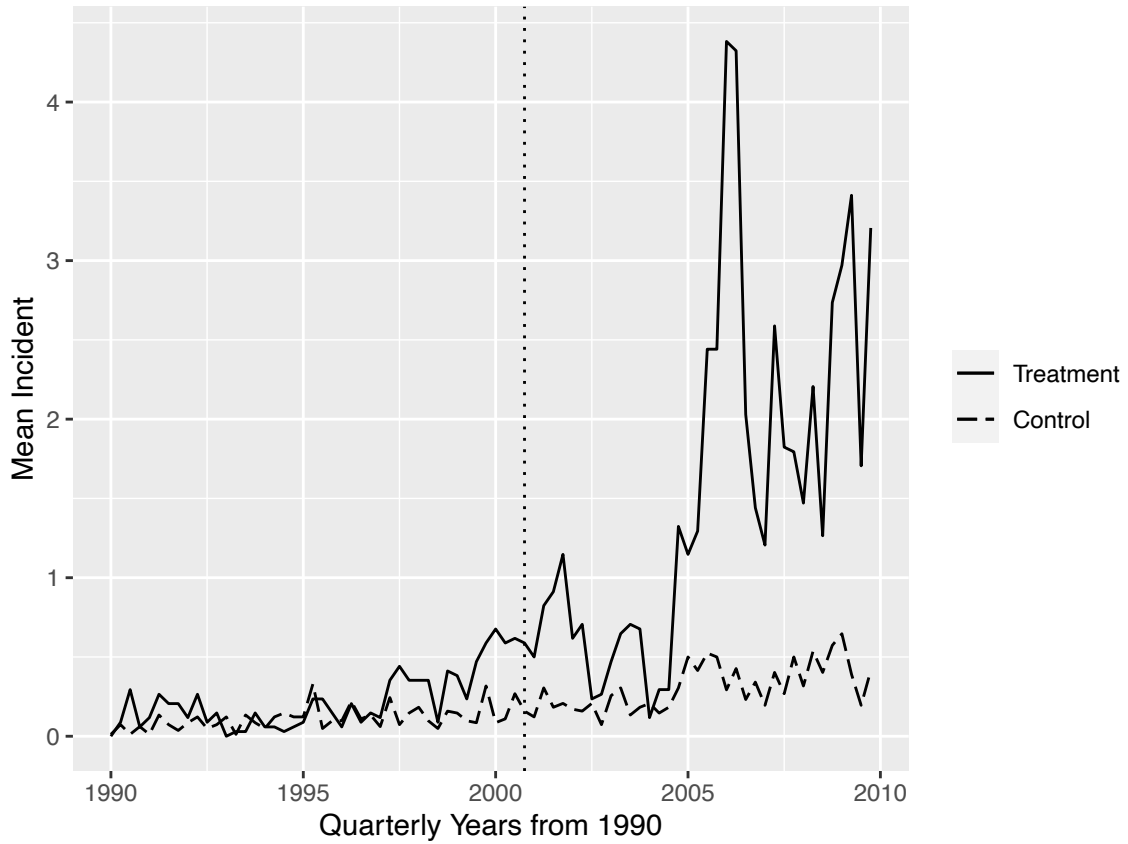
1.4.3 Parallel Trends Assumption

Difference-in-difference designs rely on the parallel trends assumption. The parallel trends assumption requires the difference between the treatment and control group in pre-treatment outcomes to be the same over time. To meet the assumption, it is optimal to have the treatment randomly assigned. In my analysis, the creation of new states is not random. To visually check the extent to which the assumption holds, I graphed violent Maoist incidents over time by treatment and control group. Figure 1 shows the parallel trends of incidents between the treatment group and the control group.¹⁹ In Figure 1.1, the trends in both groups are similar before the dotted vertical line, which represents the creation of new states. Although there seem to be increasing differences in incidents between the two groups preceding the creation of new states, the formal placebo tests, which are shown later in the paper, suggest that the differences are not statistically or substantively significant. With some level of increase in the pre-treatment periods, the dotted line continues to increase over time in the post-treatment periods. The solid line seems to be in parallel with the dotted line but with higher levels of violence in the pre-treatment periods. The treated districts before the treatment might inherently have higher levels of violence due to a higher baseline of elite control. Thus, the counterfactual is not the dotted line but an increasing line in parallel with the dotted line with higher levels of violence. Despite the baseline difference in violence, the graph and the placebo tests later in the paper suggest the treated and the control would increase in parallel in the post-treatment period and the parallel trends assumption holds. The treatment group and the control group start to show a marked difference after the dashed line. The figure suggests that the parallel trends assumption holds. More interestingly, it shows some preliminary evidence that the creation of new states increased Maoist violence.

¹⁸The distribution of incidents is highly skewed because there are many observations with zero incidents in the data. I conducted models logged dependent variable as robustness checks. The results are robust to logged dependent variable. Please see the appendix for results.

¹⁹Parallel trends by year are included in the appendix.

Figure 1.1: Trends in Incidents Between New States and Old States



1.5 Results

Table 1.1 shows the main results: the creation of new states leads to an increased number of violent Maoist incidents, supporting Hypothesis 1. The table shows the coefficient estimate of the effect of new states on incidents using an OLS model. New states see an increase of 1.298 incidents in each district in each quarter; the effect is statistically significant at the 0.01 level.

Table 1.1: Main Results

Creation of New States	1.298*** (0.431)
District FE	Y
Quarterly Year FE	Y
District	116
N	9280

* *Note:* P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1; **p<0.05; ***p<0.01.

To interpret the substantive results more directly, I use the data and the OLS empirical model to predict point estimates of the number of Maoist incidents in a hypothetical scenario in which no regional autonomy was granted. Without regional autonomy, there would be 2201 total incidents. Compared to the actual number of incidents (3562), this simple calculation suggests that the region would have experienced 1361 fewer Maoist incidents had no regional autonomy been granted in 2000. Poisson predictions show similar magnitudes. The results strongly support the hypothesis that when regions move from no autonomy to elite-captured autonomy, we expect to see an increase in insurgent violence, relative to regions that do not gain autonomy.²⁰

1.5.1 Controls for Treatment Assignment Process

As mentioned above, the creation of new states is not randomly assigned to districts. To better understand the treatment assignment process, it is crucial to understand why some districts in Madhya Pradesh were selected into Chhattisgarh and why some in Bihar became Jharkhand. I argue there are two major reasons: ST/SC population ratio and the need for development. The first reason some districts become the new states is they are ST/SC-populated. ST/SC groups requested the creation of new states through social movement (Tillin 2011), thinking that new ST/SC-populated states would better serve their needs. With the help of large political parties in India, the two new states were created with a large ST/SC population in each. Chhattisgarh has a 50 percent SC/ST population, and Jharkhand has a 40 percent SC/ST population. ST/SC population ratio in districts might be associated

²⁰I also conducted models with Quasi-Poisson, Negative Binominal, and logged dependent variable as robustness checks. The results are robust to all specifications except that the coefficient of treatment is not statistically significant in the Quasi-Poisson model with controls. Please see the appendix for results.

both with the assignment of the treatment to new states and the level of Maoist conflict. Although ST/SC population ratio is a largely time-invariant factor that has been controlled by the inclusion of district fixed effects, it does vary year by year. So in some specifications and as robustness checks, I control it using data for the ST and SC populations from India Census, collected by the Socioeconomic High-resolution Rural-Urban Geographic Dataset on India (SHRUG) (Asher and Novosad 2020, Asher et al. 2020).

The second reason some districts become the new states is that they are underdeveloped. It has been argued that the new states are created to better promote advancement within underdeveloped districts (Kumar 2000). Indeed, poverty ratios in Chhattisgarh and Jharkhand are very high: around 50 percent in Chhattisgarh and 40 percent in Jharkhand. Poverty might be a factor associated both with the assignment of the treatment to new states and the level of Maoist conflict. Although poverty, similar to ST/SC population, is largely a cross-sectional time-invariant difference that has been controlled by the inclusion of district fixed effects, it does vary year by year. I include the non-farm employment rate as a proxy of poverty in some specifications as a robustness check. The data on non-farm employment rate come from the Economic Census collected by SHRUG (Asher and Novosad 2020, Asher et al. 2020).

Table 1.2 shows the coefficient estimate of the effect of new states on incidents using an OLS model with the ST/SC population and non-farm employment rate controlled. New states saw an increase of 1.411 incidents in each district in each quarter; the effect is also statistically significant at the 0.01 level. Overall, I find support for the hypothesis that when regions move from no autonomy to elite-captured autonomy, we expect to see an increase in insurgent violence, relative to regions that do not gain autonomy.²¹

²¹Another concern is that the treatment assignment is also impacted by the distribution of pre-treatment era Maoists or potential Maoists in districts. Also, one might argue SC/ST population ratios do not vary much over time and hence their coefficients are likely to be statistically imprecisely estimated. If these are the cases, the creation of new states is endogenous and correlated with potential outcomes. To address these concerns, I include pre-treatment incidents (incidents in the fourth quarter of 1999), SC population ratio in 1999, ST population ratio in 1999, and non-farm employment rate in 1999, and their interactions with the treatment variable $\beta_1 New\ State_{i,t}$. The results are robust to the inclusion of pre-treatment incidents, SC population ratio and non-farm employment rate. Please see the appendix for results.

Table 1.2: Results with Controls

Creation of New States	1.411*** (0.464)
SC Population Ratio	-16.466* (9.332)
ST Population Ratio	1.259 (4.524)
Non-farm Employment Rate	2.462 (2.316)
District FE	Y
Quarterly Year FE	Y
District	116
N	9280

* *Note:* P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1; **p<0.05; ***p<0.01.

1.5.2 Placebo Tests

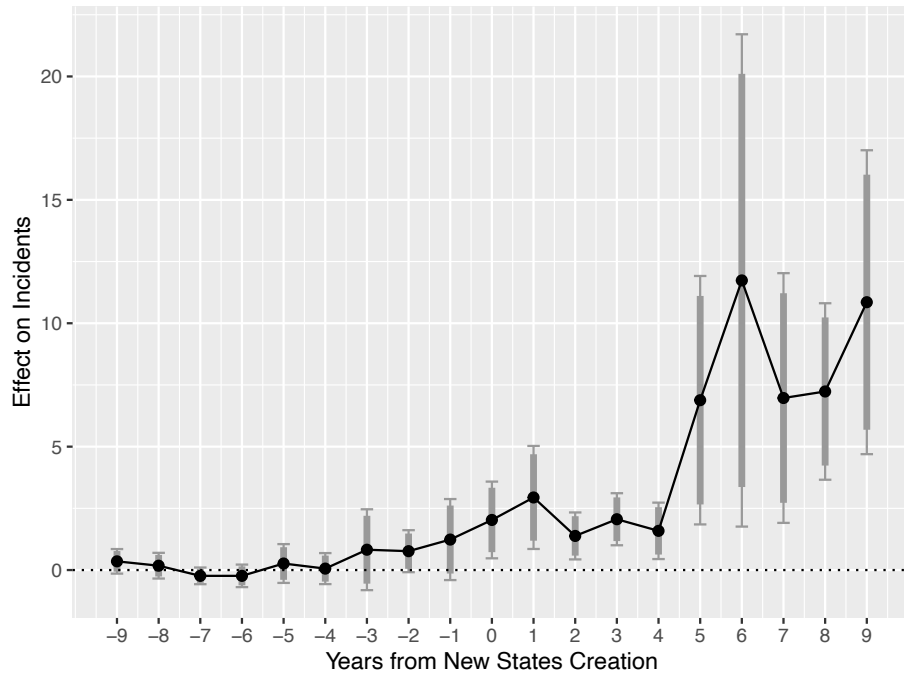
In this section, I conduct an additional test of the parallel trends assumption to examine whether the treatment group and the control group have similar trends in incidents before the creation of the new states, using an OLS model and data at the district-year level.²² To do so, I include hypothetical leads in the placebo test, i.e., hypothetical new state creation in each of the eight years prior to the actual year in which new states were created. If the treatment group and the control group had similar trends in incidents before the new states were created, the hypothetical creation prior to the actual creation would have no effect on incidents. Eight lags are included to test whether the effect of the creation lasts.

Figure 1.2 shows the placebo test for incidents. The bars are 95 percent confidence intervals. The bold lines are 90 percent confidence intervals for the estimate of the effect of the creation of the new states. As Figure 1.2 shows, the effects of the creation of new states on Maoist violence are not significant in years prior to the new states' creation. On

²²The district-year level results are similar to district-quarterly-year level results. Please see the appendix. The use of district-year level data reduces the number of lags and leads of new states creation, thus reducing multi-collinearity problem.

and after the year of the new states' creation, the effects of the creation of new states are statistically and substantively significant. The magnitude of the effects largely differ from those in years prior to the creation of new states. The results show that the trends of incidents in the treatment group and the control group are similar prior to new states creation and that the creation of new states has long-term effects on Maoist violence.

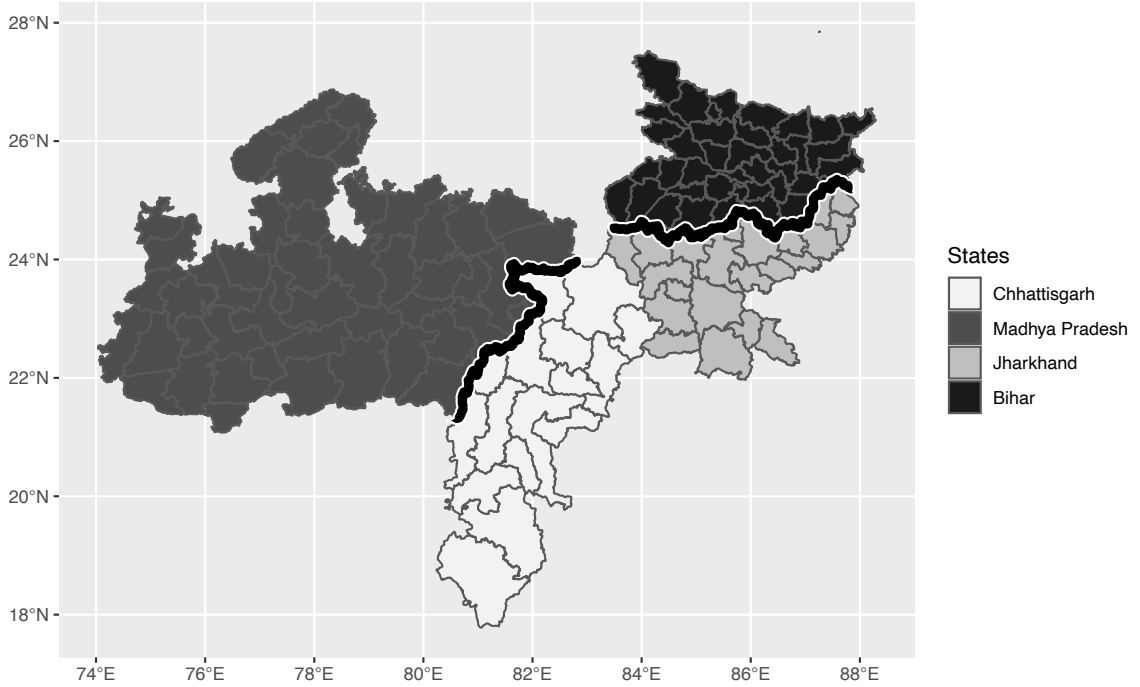
Figure 1.2: Changes in Maoist Violent Incidents Before and After New States Creation



1.6 Geographic Regression Discontinuity

I conducted auxiliary tests using a Geographic Regression Discontinuity Design (RDD) (Imbens and Lemieux 2008, Lee and Lemieux 2010) to investigate the effects of elite-captured regional autonomy on insurgencies in districts that are closer to the border. Figure 1.3 shows the shared border between two new states and two old states. Using an RDD design, I am able to compare the number of Maoist conflict incidents in new state districts with old state districts along the shared border after the creation of new states. Geographic RDD results work as a check of whether the difference-in-difference results are driven by districts that are far away from the border, which might be different even without new state creation.

Figure 1.3: Shared Border Between New and Old States



The district-level running variable is the distance from the centroid of each district to the shared border between two new states and two old states. For districts in old states, the distance is coded as negative and for districts in new states, positive. I use a nonparametric approach and local linear regression (Imbens and Lemieux 2008) to estimate the models. Following Imbens and Kalyanaraman (2012), I use a triangular kernel. Observations are at the quarter-year level, and I include only post-treatment observations. Panel data can be used in RDD designs as long as the serial correlations are properly controlled (Lee and Lemieux 2010). To account for serial correlation at the district level, robust standard errors are clustered at the district level. Since regression discontinuity designs are very sensitive to the choice of bandwidth, I run models with bandwidth from 25 to 100, incrementally increasing by 1.²³ The regression equation is:

$$Violence_i = \beta_0 + \beta_1 New State_i + \beta_2 Distance_i + \beta_3 New State_i * Distance_i + \delta X' + \epsilon_i$$

$Violence_i$ is the incidents of Maoist conflict in district i . $New State_{i,t}$ is the creation of new states for district i . $Distance_i$ is the distance to shared border for district i . X' are controls to be discussed below. All standard errors are robust standard errors clustered at the district level, to account for the serial correlation within a district.

²³RDD specifications by year and bandwidth are included in the appendix.

Table 1.3: Balance Tests

Variable	Full sample	<= 25 km	<= 50 km	<= 100 km
Population(/10000)	-35.181*** (12.614)	-38.353* (19.786)	-30.246* (17.925)	-40.097** (16.395)
Literacy	0.008 (0.022)	0.000 (0.033)	0.009 (0.031)	0.023 (0.026)
SC Population Ratio	-0.044*** (0.013)	-0.014 (0.027)	-0.017 (0.022)	-0.025 (0.016)
ST Population Ratio	0.194*** (0.043)	0.062 (0.070)	0.085 (0.058)	0.143*** (0.048)
Non-farm Employment Rate	0.039 (0.024)	0.062 (0.044)	0.056 (0.037)	0.057* (0.031)
Incident in 1999	1.030 (0.928)	1.250 (1.949)	1.263 (1.631)	0.557 (1.244)
N	116	32	42	64

* Note: P-values are two-tailed. *p<0.1; **p<0.05; ***p<0.01.

One important assumption of an RDD design is that observations close to the cutpoint should be similar. To check whether districts close to the border are similar to variables that might confound the relationship between new state creation and Maoist incidents, I conducted balance tests of pre-treatment population, literacy rate, ST population ratio, and SC population ratio between new state districts and old state districts. I used t-tests at different bandwidths because these variables are related to SC/ST and development, which are two possible determinants of new state creation, as discussed above. Data for population, literacy, and ST/SC population are from the 1991 India Census. Non-farm employment is from 1998 Economic Census. Data are collected by SHRUG (Asher and Novosad 2020, Asher et al. 2020). I also test the pre-treatment violence level by including incidents in 1999. All data are aggregated at the district level. Table 1.3 shows balance tests results, which, as expected, show that new state districts and old state districts are different in population, ST population ratio and SC population ratio, and non-farm employment rate. Thus, in the following RDD specifications, I include these variables as controls. ²⁴

²⁴Similar to DiD design, one concern is that the treatment assignment is also impacted by the pre-treatment covariates including incidents, SC/ST population ratio, and non-farm employment rate. To address the con-

Table 1.4: Geographic RDD Results

	<i>Bandwidth:</i>		
	<i>25 km</i>	<i>50 km</i>	<i>100 km</i>
	(1)	(2)	(3)
New States Creation	4.508*** (0.926)	1.870** (0.866)	0.591 (0.563)
Distance	-0.158*** (0.049)	-0.028 (0.018)	-0.003 (0.008)
New States Creation*Distance	0.069 (0.060)	0.018 (0.034)	0.007 (0.011)
SC Population Ratio	17.561*** (2.856)	15.350*** (3.127)	16.641*** (4.678)
ST Population Ratio	3.728*** (1.056)	2.952*** (0.994)	3.624** (1.450)
Non-farm Employment Rate	1.138 (9.434)	-7.802 (15.308)	-14.794 (11.110)
Population (/10000)	0.009*** (0.003)	0.013*** (0.002)	0.009*** (0.003)
Constant	-6.625*** (0.896)	-4.755*** (1.157)	-3.488*** (1.228)
N	432	972	1800

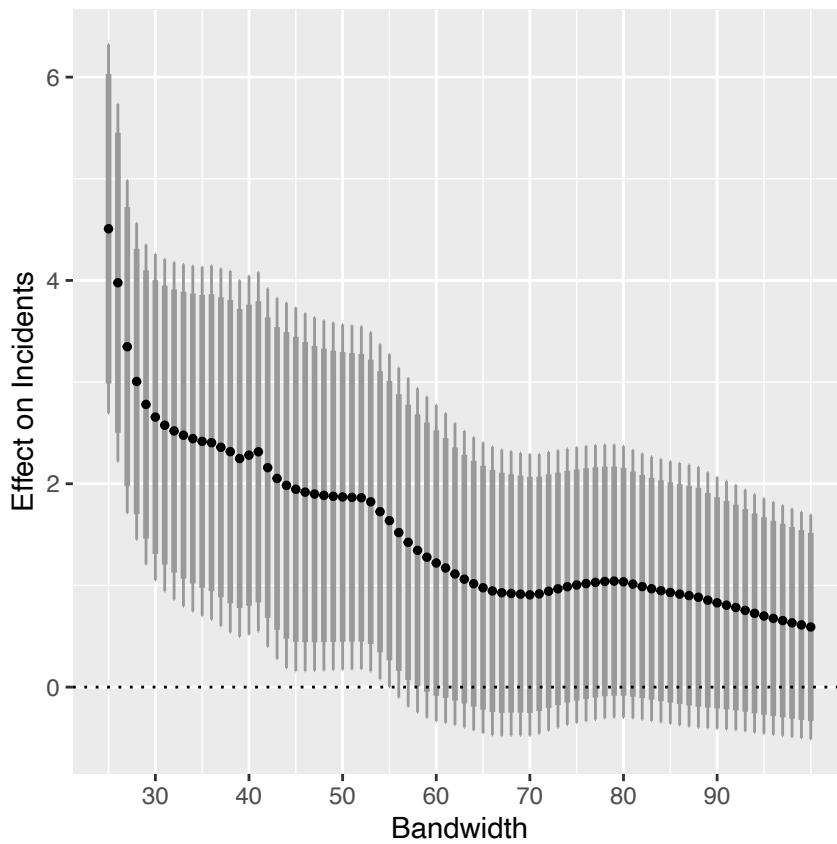
* *Note:* P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1; **p<0.05; ***p<0.01.

Table 1.4 shows the results of geographic RDD models with bandwidth at 25 km, 50 km, and 100 km. The results support the hypothesis that elite-captured regional autonomy increases insurgent violence. For districts that are 25km or less from the border, new state creation increases Maoist incidents by 4.508, and the result is statistically significant

cern, I also include pre-treatment these pre-treatment covariates and their interactions with the treatment variable $\beta_1 New State_{i,t}$ in RDD models. The results are robust to the inclusion of these new controls. Please see the appendix for the results.

at the 0.01 level. For districts 50km or less from the border, new state creation increases Maoist incidents by 1.870, and the result is statistically significant at the 0.05 level. For districts 100km or less from the border, new state creation increases Maoist incidents by 0.591, but the result is not statistically significant. Figure 1.4 plots the effects with bandwidth from 25km to 100km, incrementally increasing by one. The figure shows that as the bandwidth increases, the magnitude of the effect of new state creation on Maoist incidents decreases. The effect loses its statistical significance at 0.05 level when bandwidth is at 56km. The results show a strong border effect, such that the effect of new state creation is larger for districts closer to the border. These findings support that the results of the difference-in-difference design are not driven by districts that are far away from the border.

Figure 1.4: Geographic RDD Results with Different Bandwidths



1.7 Elite Exploitable Resources

I hypothesized that that as the value of elite-exploitable resources increases, elite-captured regional autonomy will increase insurgent violence. The value of elite-exploitable resources is measured by two variables: (1) resource abundance and (2) international resource price. Resource abundance is a dummy variable coded as 1 for districts with commercially viable reserves of iron ore, bauxite ore (a primary source of aluminum), and coal; I code

resource abundance for iron ore, bauxite ore, and coal separately. Studies have shown the connection between mining activity related to these minerals and insurgency (Hoelscher, Miklian and Vadlamannati 2012, Kennedy 2015). The variable is coded using the Ministry of Mines report titled State-wise Mineral Scenario 2010-2011.²⁵ There are a few concerns with this measurement. First, because I was able to only locate official reports published after 2010, the resource abundance variable is measured post-treatment. Second, the variable is only spatial-variant but not time-variant and therefore subject to unobservable confounding variables. International resource price is the international quarterly price of iron, aluminum, and coal, drawn from IMF Primary Commodity Prices.²⁶ The original data are at the month level. I aggregate the data to the quarter level by taking the average of the three-month price index for each quarter from 1990 to 2009. One of the benefits of using this measurement is exogeneity. Because India is not an influential exporter of these minerals, the international prices can be treated as exogenous to the mining activities affected by insurgency in India. One of the concerns is that this measurement is time-variant but not spatial-variant.

Theoretically, for each district, the value of elite-exploitable resources is subject to (1) the presence of important resources and (2) the price of resources. To better capture the concept of the value of elite-exploitable resources, I interact resource abundance and international resource price in the following tests. This approach also brings an empirical advantage because the measurements have both spatial variation and time variation. To test the effect of elite-captured regional autonomy on insurgent violence, conditioned by the value of elite-exploitable resources, I interact both resource abundance and international resource price with the main IV, new state creation, in the difference-in-difference design with OLS as the estimation method.²⁷ I conduct tests for iron, bauxite (aluminum), and coal separately to examine whether the effects hold across different resources. The regression equation is:

$$\begin{aligned} Violence_{i,t} = & \beta_0 + \beta_1 New\ State_{i,t} + \beta_2 Price_t + \beta_3 New\ State_{i,t} * Abundance_i + \\ & \beta_4 New\ State_{i,t} * Price_t + \beta_5 Abundance_i * Price_t + \\ & \beta_6 New\ State_{i,t} * Abundance_i * Price_t + \delta X' + \gamma_i + \eta_t + \epsilon_{i,t} \end{aligned}$$

Abundance_i is district *i* that has abundant iron, bauxite, or coal. *Price_t* is the international price index of iron, aluminum in quarterly year *t*. Other variables are the same as in the difference-in-difference design above. All standard errors are robust standard errors clustered at district level, to account for serial correlation within a district.

²⁵See https://www.mines.gov.in/writereaddata/UploadFile/Statewise_Mineral_Scenario.pdf

²⁶Please see: <https://www.imf.org/en/Research/commodity-prices> for the data source. For coal price, I used Coal Price Index, 2016 = 100, includes Australian and South African Coal. For aluminum price, I used Aluminum, 99.5% minimum purity, LME spot price, CIF UK ports, US\$ per metric ton. For iron price, I used China import Iron Ore Fines 62% FE spot (CFR Tianjin port), US dollars per metric ton.

²⁷As robustness checks, I also conducted tests with interactions between new state creation and resource abundance and interactions between new state creation and international resource price. All results, including triple interaction, are robust to inclusion of controls. Please see the appendix for tables of results.

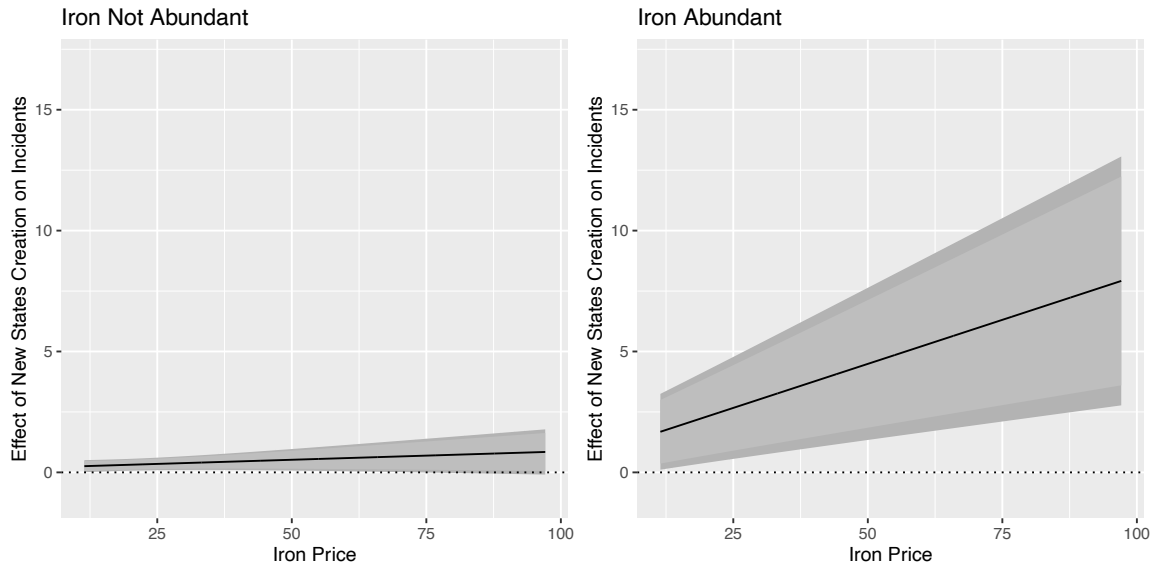
Because the results table of triple interactions is hard to interpret, I plot the effects of new state creation on Maoist incidents conditioned by resource abundance and international resource price.²⁸ Figure 1.5 shows the conditional effects of iron values.²⁹ In the figure, the left plot shows the effect of new state creation under no abundant iron resource conditions, and the right plot shows the effect under abundant iron resource conditions. Each plot shows how the effect of new state creation changes according to the international iron price. The value of the international iron price is constrained by real-world minimum and maximum prices of iron to present realistic scenarios.

The figure supports the hypothesis that as the value of elite-exploitable resources increases, elite-captured regional autonomy increases incidents of insurgent violence. For districts with abundant iron ore, new state creation substantively increases Maoist incidents as the international price of iron increases. When the iron price is at its minimum, i.e., \$11.45, new state creation increases Maoist incidents by 1.68. When iron price is at its maximum, i.e., \$97.10, new state creation increases Maoist incidents by 7.92. For districts without abundant iron ore, new state creation does not increase Maoist incidents as much as the international price of iron increases them. When the iron price is at its minimum, new state creation increases Maoist incidents by 0.26. When the iron price is at its maximum, new state creation increases Maoist incidents by 0.84. Putting the two plots together, the results show that as the value of elite-exploitable public resources increases in all regions, the increase in insurgent violence in regions with elite-captured autonomy—relative to regions without regional autonomy—becomes larger. Equally importantly, it shows that there is a baseline capture of regional autonomy by elites. When the value of exploitable resources is at its lowest, new state creation still increases Maoist incidents.

²⁸Please see the appendix for tables of results. All results including triple interaction are robust to the inclusion of controls.

²⁹Bauxite and coal results are included in the appendix. Bauxite and coal results have large confidence intervals compared to iron results because there are fewer variations over time.

Figure 1.5: The Effect of New States Creation Conditioned by Iron Abundance and International Iron Price



1.8 Conclusion

To explain the role of elite capture in regional autonomy and civil conflict, I built a general theory with three key assumptions: First, political elites are motivated to engage in corruption to capture rent. Second, citizens can be misled by elites. Third, the value of public resources is large enough such that elites are willing to betray citizens. Under these assumptions, I argue that granting regional autonomy creates new opportunities for elites to manipulate institutions in their own interest, consequently creating more political and economic grievances that fuel insurgent groups and lead to more insurgent violence. Using a difference-in-difference research design that exploits cross-sectional and temporal variation in the creation of new states and a geographical regression discontinuity design using geographic characteristics of new state creation in India, I show that the creation of two new states is statistically and substantively associated with increases in incidents related to Maoist insurgency. I further tested the implication of increasingly valuable public resources by showing that as the value of elite-exploitable minerals increases, the creation of these two new states leads to even more incidents of Maoist insurgency.

Although the assumptions of my theory are consistent with dynamics in India, and I test the theory using data on the creation of new states and Maoist insurgency in India, my theory about elite capture in regional autonomy and political violence is generalizable to other developing democracies where these assumptions are commonly met. In Indonesia, for example, scholars have also expressed concerns about the extent to which elite capture has limited the effect of fiscal decentralization on civil conflict. These findings have important policy implications: Democratization should be implemented carefully to avoid elite capture if governments hope to improve citizens' political rights and reduce violent conflicts.

My theory suggests that regional autonomy should be able to alleviate civil conflict when political elites with no intention to seek rent through corruption are selected. Also, government transparency, along with institutional designs that increase the costs or reduce the benefits of elite capture, can help to avoid elite-captured regional autonomy to take place and increase civil conflict.

This paper suggests two different directions for future research. First, future studies might empirically test my theory in other developing democracies where the three key assumptions are commonly met. These studies could be country-specific, examining whether civil conflict increases, when regions move from no autonomy to elite-captured autonomy, relative to regions that do not gain autonomy. Second, future studies might theorize more on the implications of relaxing any or all of the three key assumptions of my theory and test them empirically. The empirics of these studies could be large N cross-country studies that examine the conditions under which elite-captured regional autonomy, regional autonomy without elite capture, or no regional autonomy is more likely, as well as the consequences of these different institutions on civil conflict.

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1.9 Appendix

Table 1.5: Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Quarterly Year	9,280	1,999.750	5.768	1,990.100	1,994.925	1,999.750	2,004.575	2,009.400
Incidents	9,280	0.384	1.880	0	0	0	0	69
New States Creation	9,280	0.293	0.455	0	0	0	1	1
Population	9,280	1,427,753.000	765,581.700	118,643	892,971	1,289,735	1,812,130.0	4,689,460
Literacy	9,280	0.383	0.119	0.126	0.287	0.362	0.476	0.651
SC Population	9,280	0.141	0.060	0.028	0.106	0.141	0.181	0.324
ST Population	9,280	0.178	0.210	0.00000	0.010	0.111	0.271	0.868
Employment Rate	9,280	0.050	0.066	0.008	0.026	0.038	0.053	0.792
Bauxite	9,280	0.198	0.399	0	0	0	0	1
Coal	9,280	0.138	0.345	0	0	0	0	1
Iron	9,280	0.121	0.326	0	0	0	0	1
Resource Abundance	9,280	0.336	0.472	0	0	0	1	1
Distance	9,280	-131.486	217.298	-651.323	-274.475	-83.798	22.975	298.384
Coal Price	9,280	64.007	36.496	34.794	43.484	52.243	74.223	249.700
Aluminum Price	9,280	1,665.224	456.796	1,073.601	1,356.319	1,515.931	1,813.325	2,948.060
Iron Price	9,280	21.956	18.222	11.450	12.655	13.615	19.320	97.097

Figure 1.6: Parallel Trends in Incidents

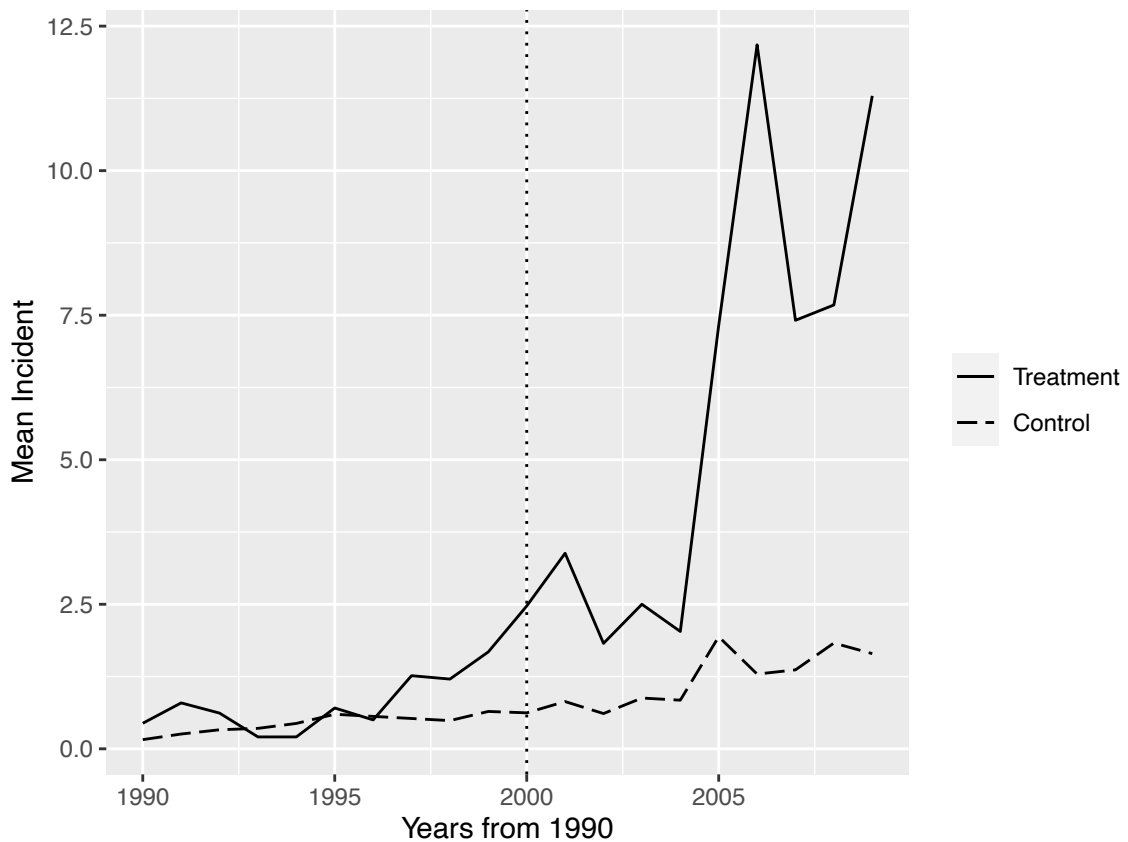


Table 1.6: Results with Interaction Controls

Creation of New States	0.892*** (0.324)	1.681 (1.147)	-0.335 (0.729)	1.569*** (0.516)
Creation of New States * Incidents 1999	0.987*** (0.235)			
Creation of New States * SC Population Ratio 1999		-3.452 (7.582)		
Creation of New States * ST Population Ratio 1999			5.204 (3.201)	
Creation of New States * Non-farm Employment Rate 1999				-3.262** (1.315)
District FE	Y	Y	Y	Y
Quarterly Year FE	Y	Y	Y	Y
District	116	116	116	116
N	9280	9280	9280	9280

* Note: P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1; **p<0.05; ***p<0.01.

Table 1.7: Year Results

	(1)	(2)
New States Creation	5.047*** (1.637)	5.300*** (1.693)
SC Population Ratio		-70.919* (39.251)
ST Population Ratio		-2.813 (17.001)
Employment Rate		5.380 (8.270)
District FE	Y	Y
Quarterly Year FE	Y	Y
District	116	116
N	2320	2320

* *Note:* P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1; **p<0.05; ***p<0.01.

Table 1.8: Quasi-Poisson, Logged DV, and Negative Binominal Results

	<i>Quasi-Poisson</i>		<i>Logged DV</i>		<i>Negative Binominal</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
New States Creation	0.918** (0.422)	0.609 (0.498)	0.369*** (0.071)	0.396*** (0.073)	0.866*** (0.310)	0.672* (0.362)
SC Population Ratio		-9.549 (15.012)		-3.292* (1.726)		0.309 (9.438)
ST Population Ratio		-29.509 (19.813)		0.034 (1.122)		-22.821* (13.462)
Employment Rate		-9.940 (12.561)		0.568 (0.512)		-9.511 (7.676)
District FE	Y	Y	Y	Y	Y	Y
Quarterly Year FE	Y	Y	Y	Y	Y	Y
District	116	116	116	116	116	116
N	9280	9280	9280	9280	9280	9280

* Note: P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1; **p<0.05; ***p<0.01.

Figure 1.7: Changes in Maoist Violent Incidents Before and After New States Creation with Controls

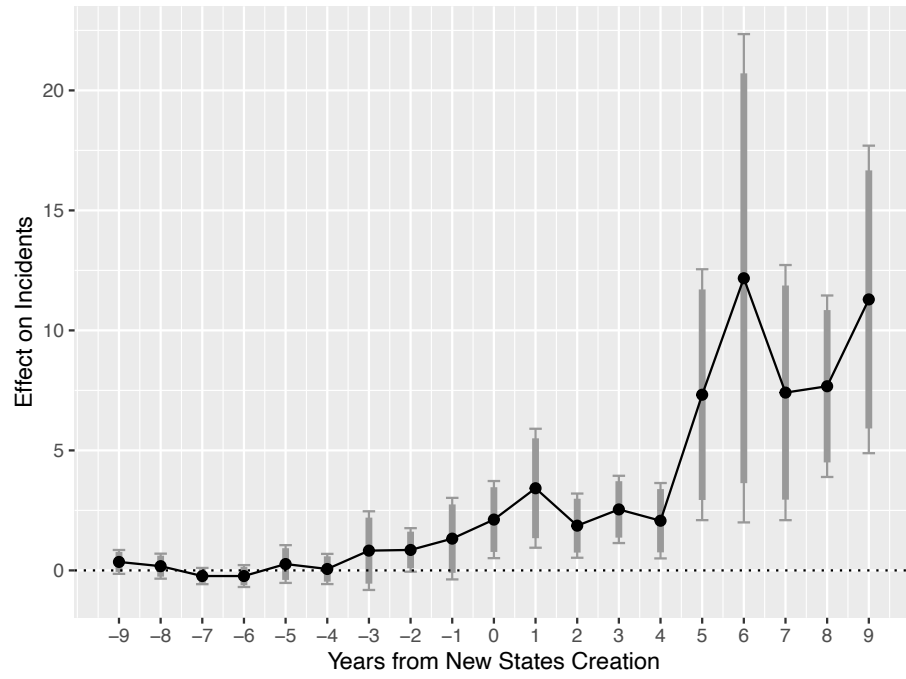


Table 1.9: Placebo Test Results

	(1)	(2)
tm9	0.353 (0.254)	0.353 (0.254)
tm8	0.176 (0.266)	0.176 (0.266)
tm7	-0.235 (0.173)	-0.235 (0.173)
tm6	-0.235 (0.233)	-0.235 (0.233)
tm5	0.265 (0.401)	0.265 (0.402)
tm4	0.059 (0.321)	0.059 (0.321)
tm3	0.824 (0.836)	0.824 (0.836)
tm2	0.765* (0.434)	0.853* (0.463)
tm1	1.235 (0.837)	1.323 (0.867)
t	2.029** (0.793)	2.117*** (0.820)
tp1	2.941*** (1.065)	3.423*** (1.265)
tp2	1.382*** (0.485)	1.864*** (0.682)
tp3	2.059*** (0.538)	2.541*** (0.715)
tp4	1.588*** (0.583)	2.070*** (0.801)
tp5	6.882*** (2.568)	7.320*** (2.667)
tp6	11.735** (5.089)	12.173** (5.191)
tp7	6.971*** (2.581)	7.409*** (2.712)
tp8	7.235*** (1.824)	7.673*** (1.929)
tp9	10.853*** (3.141)	11.291*** (3.269)
pop_sc.r		-66.467* (37.864)
pop_st.r		3.561 (17.882)
emp.r		9.180 (8.996)
District FE	Y	Y
Quarterly Year FE	Y	Y
District	116	116
N	2320	2320

Note: *p<0.1; **p<0.05; ***p<0.01

Table 1.10: Geographic RDD Results with Interaction Controls

	<i>Bandwidth:</i>		
	<i>25 km</i>	<i>50 km</i>	<i>100 km</i>
	(1)	(2)	(3)
New States Creation	11.733*** (0.693)	4.259*** (1.039)	1.150 (1.076)
Distance	-0.294*** (0.013)	-0.028 (0.018)	0.0005 (0.007)
New States Creation*Distance	-0.084*** (0.015)	-0.022 (0.018)	0.006 (0.007)
SC Population Ratio	30.426*** (1.342)	18.108*** (2.275)	15.793*** (4.198)
ST Population Ratio	8.397*** (0.988)	3.211*** (0.928)	2.450** (1.093)
Non-farm Employment Rate	-7.809 (12.583)	3.003 (11.112)	5.186 (10.538)
Population	0.001 (0.001)	0.012*** (0.002)	0.007*** (0.002)
Creation of New States * Incidents 1999	3.353*** (0.752)	1.009*** (0.211)	0.990*** (0.212)
Creation of New States * ST Population Ratio 1999	-8.232*** (0.816)	-2.701** (1.179)	-0.664 (1.525)
Creation of New States * SC Population Ratio 1999	-34.317*** (4.123)	-12.305*** (3.715)	-6.797 (5.428)
Creation of New States * Non-farm Employment Rate 1999		-9.656*** (1.694)	-4.517* (2.521)
Constant	-9.949*** (0.256)	-5.212*** (0.953)	-3.204*** (0.968)
N	432	972	1800

* *Note:* P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1; **p<0.05; ***p<0.01. Creation of New States * Non-farm Employment Rate 1999 in 25 km was dropped by the model automatically.

Figure 1.8: Geographic RDD Results with Different Bandwidths by Year

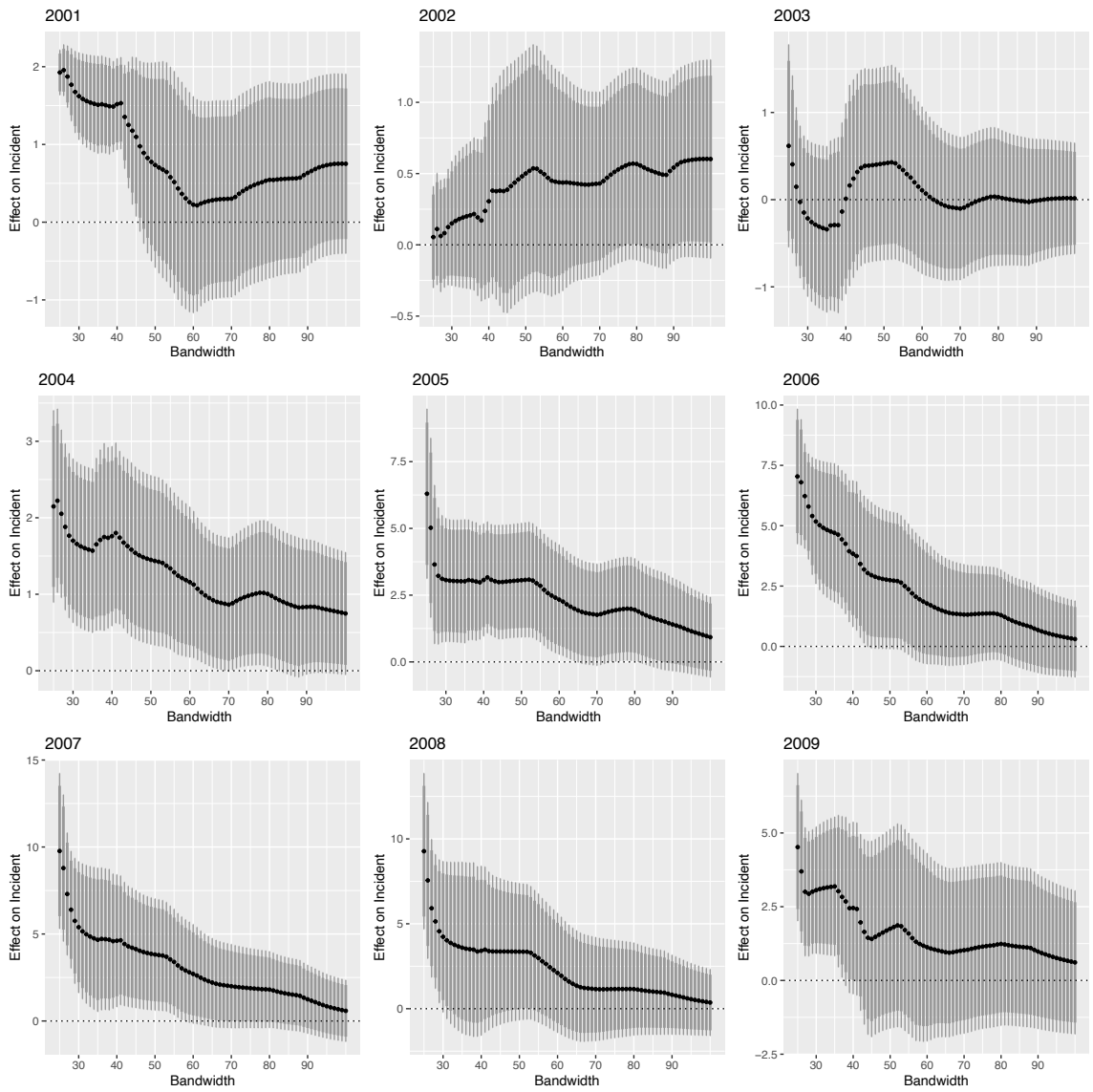


Table 1.11: Interaction with Resource Abundance Results

	<i>Dependent variable: Incident</i>	
	(1)	(2)
New State Creation	0.455* (0.236)	0.599** (0.304)
SC population		-15.193* (9.004)
ST population		-1.703 (5.226)
Employment Rate		1.122 (2.533)
New State Creation*Resource Abundance	1.103* (0.597)	0.993* (0.588)
District FE	Y	Y
Quarterly Year FE	Y	Y
District	116	116
N	9280	9280

Note: *p<0.1; **p<0.05; ***p<0.01

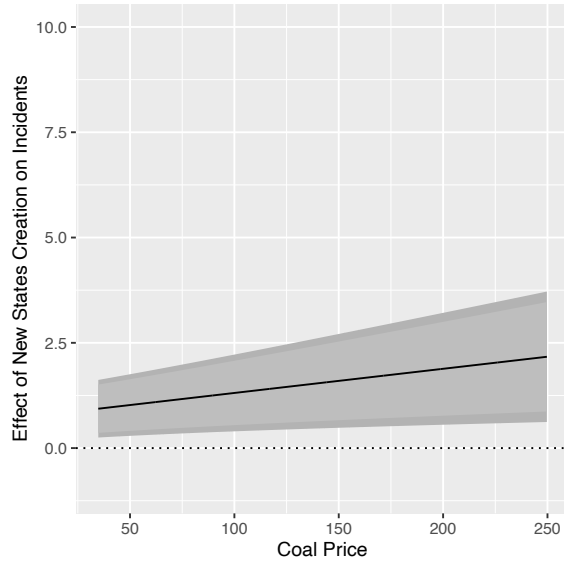
Table 1.12: Interaction with International Resource Price Results

	<i>Coal</i>		<i>Aluminum</i>		<i>Iron</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
New State Creation	0.736** (0.303)	0.855** (0.339)	-0.322 (0.489)	-0.202 (0.486)	0.393* (0.213)	0.508** (0.245)
Coal Price	0.002*** (0.001)	0.002*** (0.001)				
New State Creation*Coal Price	0.006** (0.002)	0.006** (0.002)				
Aluminum Price			0.0002*** (0.0001)	0.0002*** (0.0001)		
New State Creation*Aluminum Price			0.001* (0.0005)	0.001* (0.0005)		
Iron Price					0.005*** (0.002)	0.005*** (0.002)
New State Creation*Iron Price					0.025*** (0.009)	0.025*** (0.009)
SC Population		-15.916* (9.337)		-16.097* (9.402)		-15.957* (9.354)
ST Population		1.245 (4.450)		0.972 (4.392)		0.955 (4.365)
Employment Rate		2.545 (2.263)		2.397 (2.199)		2.414 (2.188)
District FE	Y	Y	Y	Y	Y	Y
Quarterly Year FE	Y	Y	Y	Y	Y	Y
District	116	116	116	116	116	116
N	9280	9280	9280	9280	9280	9280

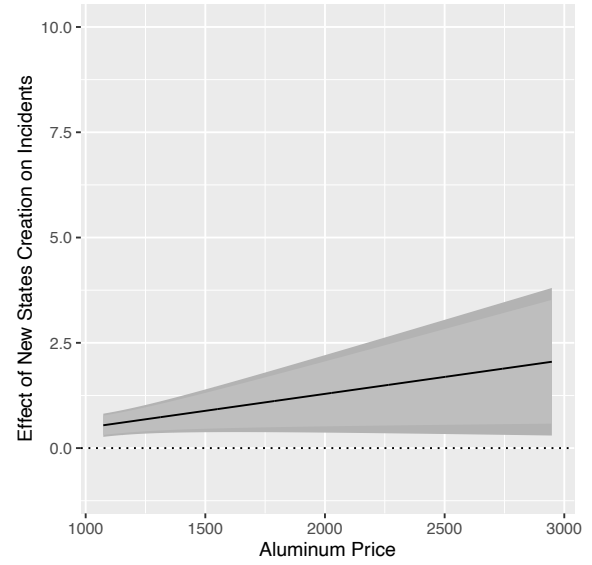
Note:

*p<0.1; **p<0.05; ***p<0.01

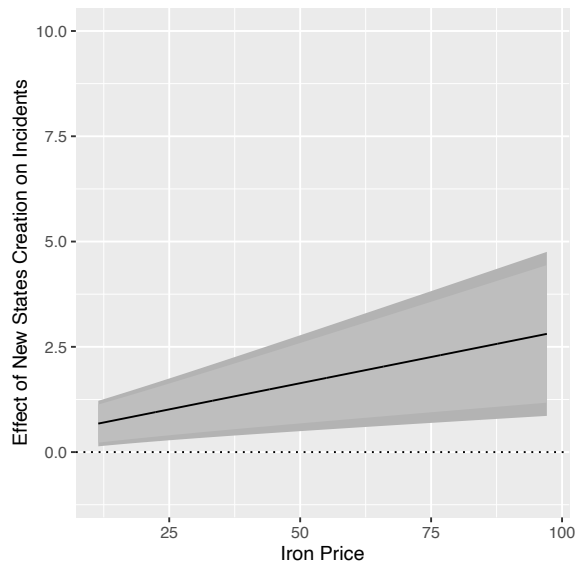
Figure 1.9: The Effect of New States Creation Conditioned by International Resource Price



(a)



(b)



(c)

Figure 1.10: The Effect of New States Creation Conditioned by International Resource Price with Controls

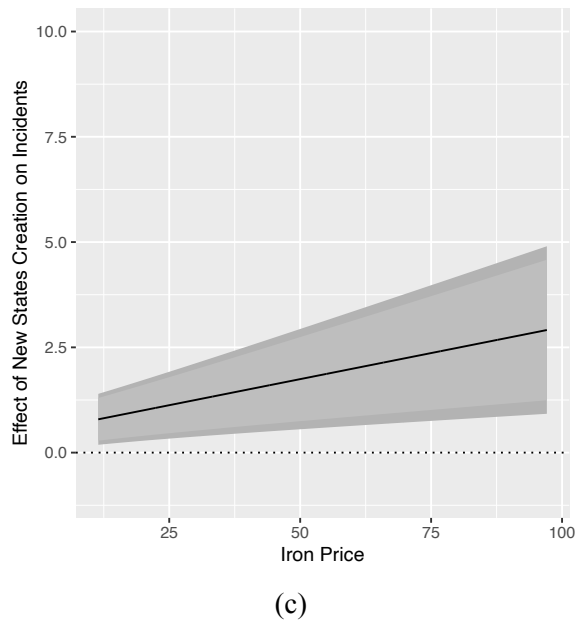
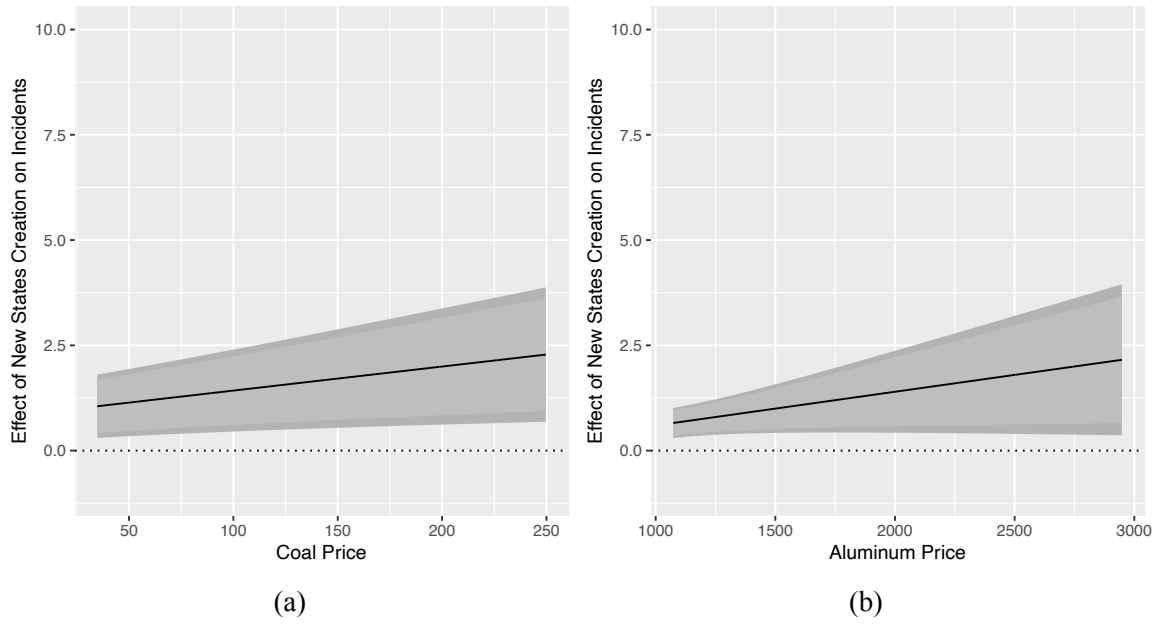


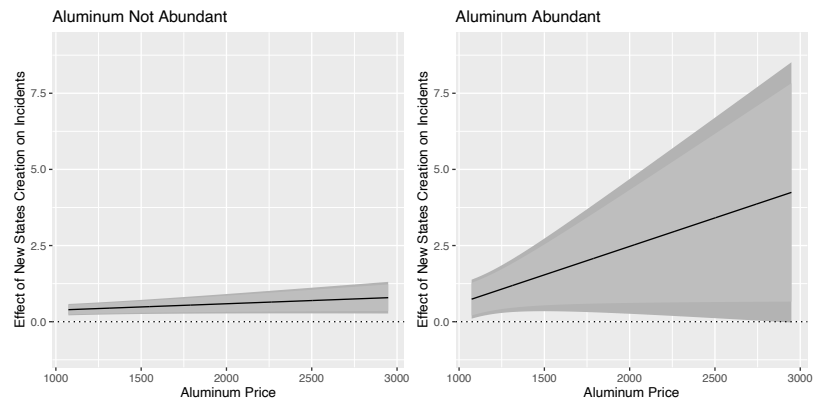
Table 1.13: The Effect of New States Creation Conditioned by Resource Abundance and International Resource Price

	(1)	(2)	(3)	(4)	(5)	(6)
New States Creation	0.178 (0.157)	0.228 (0.163)	0.169 (0.142)	0.187 (0.159)	0.929** (0.448)	1.045** (0.468)
Iron Price	0.006*** (0.002)	0.006*** (0.002)				
New States Creation*Iron Price	0.007 (0.006)	0.007 (0.006)				
New States Creation*Iron	0.667 (0.604)	0.552 (0.599)				
Iron Price*Iron	-0.006*** (0.002)	-0.006*** (0.002)				
New States Creation*Iron Price*Iron	0.066*** (0.023)	0.066*** (0.023)				
Aluminum Price			0.0002*** (0.0001)	0.0002*** (0.0001)		
New States Creation*Aluminum Price			0.0002* (0.0001)	0.0002 (0.0001)		
New States Creation*Aluminum			-1.437 (1.238)	-1.311 (1.244)		
Aluminum Price*Aluminum			-0.0002** (0.0001)	-0.0002** (0.0001)		
New States Creation*Aluminum Price*Aluminum			0.002 (0.001)	0.002 (0.001)		
Coal Price					0.003*** (0.001)	0.003*** (0.001)
New States Creation*Coal Price					0.005* (0.003)	0.005* (0.003)
New States Creation*Coal					-0.686 (0.534)	-0.703 (0.546)
Coal Price*Coal					-0.004** (0.002)	-0.004** (0.002)
New States Creation*Coal Price*Coal					0.005 (0.005)	0.005 (0.006)
SC Population		-8.699** (4.240)		-12.597 (8.588)		-17.026* (10.156)
ST Population		-1.199 (2.947)		-5.585 (6.146)		0.978 (4.342)
Employment Rate		0.270 (1.297)		0.954 (2.515)		2.303 (2.178)
District FE	Y	Y	Y	Y	Y	Y
Quarterly Year FE	Y	Y	Y	Y	Y	Y
District	116	116	116	116	116	116
N	9280	9280	9280	9280	9280	9280

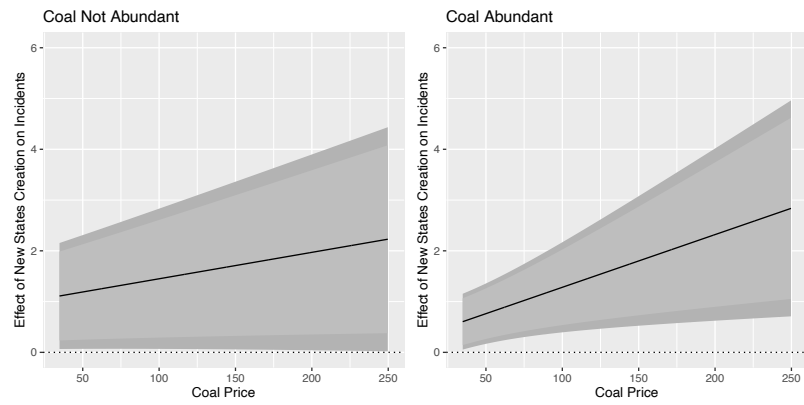
Note*

*p<0.1; **p<0.05; ***p<0.01

Figure 1.11: The Effect of New States Creation Conditioned by Resource Abundance and International Resource Price

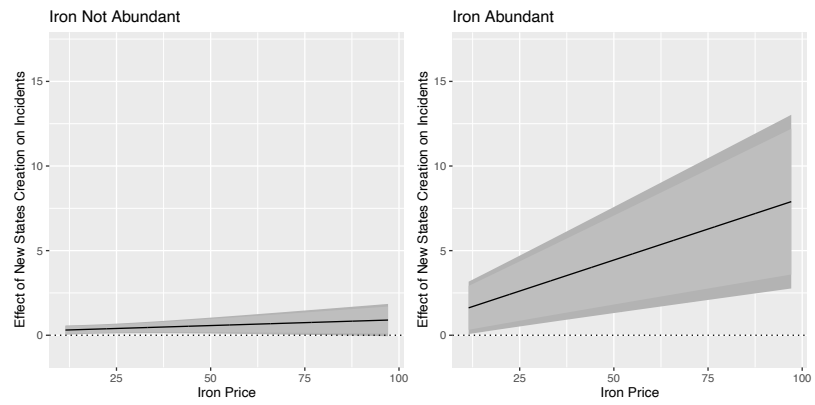


(a) Aluminum

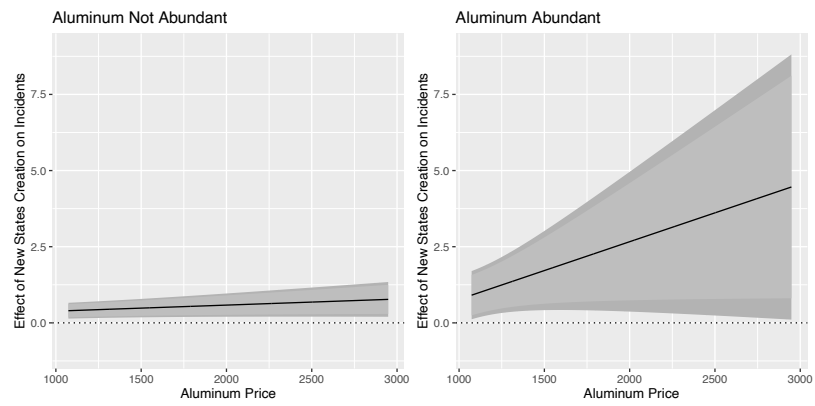


(b) Coal

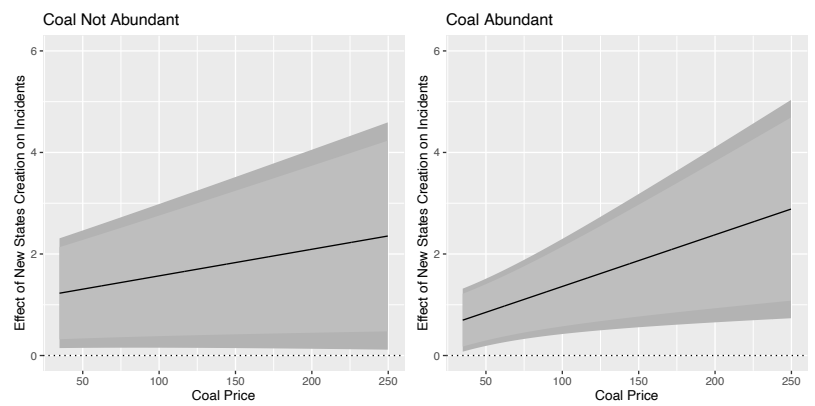
Figure 1.12: The Effect of New States Creation Conditioned by Resource Abundance and International Resource Price with Controls



(a) Iron



(b) Aluminum



(c) Coal

Part II

Heterogeneous Effects of ICC Investigation on Rebel Conflict

Abstract

Do investigations by the International Criminal Court of alleged human rights crises reduce non-state civil conflicts? To date, scholars interested in the effect of the ICC on civil conflicts have treated the “state” as a unitary actor. In this paper, I argue that the ICC’s investigations of government leaders and of rebel leaders have different effects on civil conflict. Because of its biased case selection process, when the ICC investigates government leaders, it constrains governments, provides incentives for the rebel groups to fight, and leads to more non-state conflicts—that is, conflicts between rebels. When the ICC investigates rebel leaders, it does not constrain rebel groups, strengthens the government’s control, and reduces non-state conflicts. I evaluate the implications of the theory with difference-in-difference tests using data on non-state conflicts cross-nationally and find support.

2.1 Introduction

The International Criminal Court began work in 2002 after the ratification of the Rome Statute, a treaty adopted by 120 states in 1990 and ratified by 60 states in 2002 to fight against war crimes. The International Criminal Court (ICC) investigates, tries, and prosecutes individuals charged with the gravest crimes of concern to the international community: genocide, war crimes, crimes against humanity, and the crime of aggression.³⁰ Since its establishment, the ICC has examined many ongoing conflicts. Although the ICC has a clearly stated goal of bringing war criminals to justice, the question of whether the court might advance peace by facilitating justice is a key topic of discussion among legal scholars and political scientists. Scholars debate whether the ICC's involvement in situations can impose the right costs to stop individuals from committing atrocities, thus promoting peace. Does the ICC reduce civil conflicts? More specifically, do the investigations of the ICC and their possible consequences, including warrants and trials in alleged human rights crises, reduce non-state civil conflicts?³¹

The ability of the ICC to improve human rights and advance peace has been disputed by academics. Some scholars argue that ICC can prevent human rights violations by prosecuting individuals, which increases the costs of carrying out future atrocities (Akhavan 2009, Appel 2018, Jo and Simmons 2016, Schabas 2011, Simmons and Danner 2010) and deterring future atrocities facilitates peace (Akhavan 2001, Gilligan 2006). Other scholars believe that the ICC's poor deterrent power and its need for third-party collaboration prevent it from advancing human rights and promoting peace (Ainley 2011, Cronin-Furman 2013, Drumbl 2007, Fish 2010, Ku and Nzalibe 2006). They think ICC cannot impose sufficient costs on individuals. A third group of academics also questions the ICC's ability to advance peace, but from a different angle. They argue that the ICC places excessive costs on individuals, who must continue fighting to escape consequences (Goldsmith and Krasner 2003, Nalepa and Powell 2016, Snyder and Vinjamuri 2003). Empirically, recent cross-country large-N studies have also shown conflicting results about the impact of the ICC on promoting peace and ending conflicts (Dancy and Wiebelhaus-Brahm 2018, Duursma 2020, Prorok 2017, Simmons and Danner 2010). Existing studies examine how the ICC affects state governments and rebel organizations without considering the different costs the ICC imposes on rival parties within states, leading to varied results. To understand the effects of the ICC on conflicts, we must differentiate its actions and the costs it imposes on various actors.

In this article, I argue that the ICC's investigations of government leaders and of rebel leaders have different effects on civil conflict. When the ICC investigates government leaders, it constrains governments, provides incentives for rebel groups to fight, and leads to more non-state conflicts; however, when the ICC investigates rebel groups, it does not constrain rebel leaders, strengthen government's control, or reduce non-state conflicts. The

³⁰See: <https://www.icc-cpi.int/about>

³¹I focus on non-state civil conflicts for several reasons. First, conflicts between rebels are overlooked in the literature. Second, as I will mention later, the implications of the ICC's investigation on state-rebel conflicts are not clear.

difference in deterrent and constraining power of the ICC's investigation is caused by the difference in ICC's situation selection process. Situations that are referred by state governments resulted in ICC's investigation of rebel groups. Such investigations are seen as less legitimate and biased, thus reducing domestic and international support for rebel groups. Situations referred by the UNSC or brought up as the Prosecutor proprio motu resulted in ICC's investigation of state leaders. Such investigations are seen as more legitimate and unbiased, thus reducing domestic and international support for the government. Using difference-in-difference research designs, I show that the ICC's investigation of governments and of rebel groups have heterogeneous effects on civil conflict: Whereas ICC investigations of government leaders increase deaths in non-state conflicts, ICC investigations of rebel group leaders do not reduce deaths in non-state conflicts.

The paper addresses whether the ICC can improve human rights and promote peace by facilitating justice given the costs the ICC can put on actors. The ICC puts different costs on state leaders and rebel groups, and these differences produce varied consequences in conflicts. More generally, the paper highlights the implications of biases in international intervention in conflicts. It shows biases in the procedure of international interventions have impacts on domestic politics/civil conflicts. Equally importantly, it highlights the importance of looking at multiple domestic actors and how their power relationship changes when studying international intervention. When international intervention targets different domestic actors, it changes the power relationship between state governments and rebel groups.

2.2 The Deterrence Power of the ICC

Scholars have debated whether the ICC can improve human rights and promote peace by facilitating justice. Scholars have argued that ICC can deter individuals from committing human right violations through prosecution, which increases the costs of committing atrocities (Akhavan 2009, Appel 2018, Jo and Simmons 2016, Schabas 2011, Simmons and Danner 2010). By deterring future atrocities, international prosecution facilitates peace (Akhavan 2001, Gilligan 2006). In this stream of thought, the ICC can tie individual state or rebel leaders' hands by imposing costs on individuals, thus deterring them from committing atrocities and continuing conflicts.

Another school of thought holds that the ICC's ineffective deterrence power prevents it from advancing human rights and fostering peace (Ainley 2011, Cronin-Furman 2013, Drumbl 2007, Fish 2010, Ku and Nzelibe 2006). They contend that the ICC is dependent on third-party assistance because it lacks independent enforcement authority. It either depends on states' willingness to collaborate (Ku and Nzelibe 2006) or on the support of international organizations. Because third parties may not be able or willing to cooperate, the requirement of third-party collaboration undermines the deterrence effect of the ICC. Due to the lack of severe punishment and the low likelihood of indicting wanted individuals (Cronin-Furman 2013), the Court has little enforcement power. The main disagreement between scholars who believe the ICC can deter future atrocities and researchers who believe it cannot is how they think about whether the ICC can impose enough costs on individuals.

Another group of scholars also think ICC cannot promote peace, but for a different reason. They argue that the ICC's pursuit of justice leaves little room for amnesty for peace (Goldsmith and Krasner 2003, Nalepa and Powell 2016, Snyder and Vinjamuri 2003). Studies have found the Rome Statute is ambiguous on the recognition of amnesty for peace deals (Clark 2005, Goldsmith and Krasner 2003, Majzub 2002, Newman 2004, Roht-Arriaza 2000, Scharf 1999). When ICC prosecutions are in place, indicted individuals might be forced to choose to continue the conflict to avoid the consequences of being prosecuted and tried. Similarly, Prorok (2017) argues that "active ICC involvement in a conflict increases the threat of punishment for rebel and state leaders, which, under certain conditions, generates incentives for these leaders to continue the conflict as a way to avoid capture, transfer to the Hague, and prosecution." These scholars think that the ICC put too high a cost on individuals, forcing them to find alternative ways to avoid the consequences.

Recent cross-country large-N research has shown contradictory empirical results about the impact of the ICC on promoting peace and ending conflicts. In states that are least likely to be able to make credible commitments to reduce atrocities, ratification of the ICC has been linked to decreased violence and advanced peace, according to Simmons and Danner (2010). The ICC's trials, according to Dancy and Wiebelhaus-Brahm (2018), are endogenous to other international initiatives even though they seem to be weakly associated with conflict termination. Prorok (2017) discovered that when domestic punishment threats are at relatively low levels, the ICC's involvement reduces the likelihood of conflict termination. Duursma (2020) found that the chances of resolving a dispute through negotiation are undermined by the arrest warrants issued by the ICC.

Existing studies have examined how the ICC affects state governments and rebel organizations without considering the dynamics between rival parties within states, such as state authorities and rebel organizations, leading to varied results. To understand the effect of the ICC on conflicts, we must distinguish ICC actions and the costs they impose on different actors. This paper offers a new perspective in studying the heterogeneous effects of ICC investigation of government leaders and rebel group leaders on non-state conflicts.

2.3 ICC and Selection of Situations

It's vital to introduce the ICC and how it decides which cases to explore before I develop my theory. The history and goals of the ICC are laid forth here, because these will be crucial presumptions in defining the ICC as an actor with a distinct purpose. As one of the fundamental assumptions that determines that determine the heterogeneous effects of the ICC on civil conflict, I also discuss the different selection procedures of situations for state leaders and rebel leaders here.

The ICC began its work in 2002, as its foundational treaty (the Rome Statute) took effect. The Rome Statute was adopted by 120 states in 1998 and ratified by 60 states in 2002 as a way to fight against war crimes. The ICC is a "permanent international court established to investigate, prosecute and try individuals accused of committing the most serious crimes of concern to the international community as a whole, namely the crime of

genocide, crimes against humanity, war crimes and the crime of aggression.”³² The ICC is the first permanent treaty-based supranational body dedicated to investigating atrocity crimes.

Given the number of war crimes conducted or on-going in the world, how does the ICC decide which situations to investigate? Theoretically, how the ICC selects situations has effects on the implications of its actions. Empirically, understanding how the ICC selects situations helps to identify the selection process, avoids selection biases, and comes up with a proper research design to identify causality.

There are three main ways situations can be brought to the ICC: (1) referral by a state government; (2) referral by the UNSC; (3) the Prosecutor *proprio motu*, that is on his or her impulse.³³ The ICC then selects from the pool of potential situations to investigate. Given limited resources, the ICC cannot possibly look into every situation brought to its attention, so the Prosecutor selects situations to investigate. Four criteria determine situations to investigate: (1) the scale of the crimes; (2) the nature of the crimes; (3) the manner of commission of the crimes; (4) the impact of the crimes.³⁴ In short, the ICC focuses on the most serious crimes with the worst consequences.

Table 2.1 is a list of situations under the ICC’s investigation. The investigation is defined in a broad sense so includes such possible actions as issuing warrants and trials. Investigations of government leaders and rebel group leaders differ even in the selection process. Situations resulting in the indictment of individuals are most often referred by state governments against rebel leaders. I define this as investigation of rebel leaders. Situations that are referred by the UNSC or brought up by the Prosecutor *proprio motu* result in indictments of state leaders. I define this as investigation of state leaders. The only exception is Darfur, Sudan. Darfur, Sudan was referred by UNSC. So far, the ICC investigation has resulted in indictments of four state leaders and three rebel leaders. Such differences in the situation selection process have implications for the deterrence power of the ICC and on the power relations between the state and rebels. I will explain the implications of the situation selection process in the theory section.

³²See publication Understanding the International Criminal Court by ICC at <https://www.icc-cpi.int/iccdocs/PIDS/publications/UICCEng.pdf>. Publication year unknown.

³³Two new situations: Venezuela I and Ukraine was referred by a group of states, respectively in 2021 and 2022. This is a new way in which situations can be brought to the ICC.

³⁴See: Policy Paper on Preliminary Examinations 2010 at https://www.icc-cpi.int/NR/rdonlyres/9FF1EAA1-41C4-4A30-A202-174B18DA923C/282515/OTP_Draftpolicypaperonpreliminaryexaminations04101.pdf

Table 2.1: List of Situations Under Investigation

Situation	Referral Source	Individuals indicted	Region	Referral Time
Uganda	State Government	Rebel Leaders	Africa	Jan 2004
Democratic Republic of the Congo	State Government	Rebel Leaders	Africa	Apr 2004
Central African Republic	State Government	Rebel Leaders	Africa	Dec 2004
Darfur, Sudan	UNSC	State Leaders and Rebel Leaders	Africa	Mar 2005
Kenya	proprio motu	State Leaders	Africa	Mar 2010
Libya	UNSC	State Leaders	Africa	Feb 2011
Ivory Coast	proprio motu	State Leaders	Africa	Oct 2011
Mali	State Government	Rebel Leaders	Africa	Jul 2012
Central African Republic II	State Government	Rebel Leaders	Africa	May 2014
Palestine	State Government	Not Yet	Middle-East	Jun 2014
Georgia	proprio motu	Not Yet	Europe	Jan 2016
Burundi	proprio motu	Not Yet	Africa	Oct 2017
Bangladesh/Myanmar	proprio motu	Not Yet	Asia	Nov 2019
Afghanistan	proprio motu	Not Yet	Middle-East	Mar 2020
Venezuela I	Group of States Parties	Not Yet	South America	Nov 2021
Ukraine	Group of States Parties	Not Yet	Europe	Apr 2022

2.4 Theory

There are three actors in my theory: state government leaders, multiple rebel leaders, and the ICC. I assume state government leaders and rebel leaders want to gain more power. Note this assumption is slightly different from the conventional assumption that leaders want to stay in power (De Mesquita et al. 2005, 1999, 2002, Gandhi and Przeworski 2007). The assumption that leaders want to stay in power implicitly assumes a goal of survival, whereas the assumption that leaders want to gain more power assumes a goal not only of survival but also expansion. For example, leaders who want to stay in power might not care about rebel groups that do not pose immediate threats (more defensive), but leaders who want to gain more power are more resolved to destroy rebel groups (more offensive). I assume the state government is stronger than rebel groups. There are assumed to be (or potentially be) multiple rebel groups in the state.

The third actor is the ICC. Just like the state leaders and the rebel leaders, the ICC is an actor with its own goals. As I mentioned, the ICC investigates, tries, and prosecutes individuals charged with the gravest crimes of concern to the international community: genocide, war crimes, crimes against humanity, and the crime of aggression.³⁵ The ICC aims to bring war criminals to justice (Ford 2019, Jessberger and Geneuss 2012, Mullins and Rothe 2010). The ICC's stated goal is neither directly improving human rights (perhaps deter potential future serious human rights violations) nor promoting peace. Thus, I assume the ICC's goal is justice. I also assume the ICC has limited resources (Burke-White 2008). Even for a domestic court, resources are limited. For an international institution that has no enforcement capability, the resources are even more limited.

Although the ICC has been argued it does not have enforcement capabilities to arrest and punish indicted persons, I assume that there are several costs the ICC's investigation can put on individuals.³⁶ The ICC's investigation can decrease the domestic support for

³⁵See: <https://www.icc-cpi.int/about>

³⁶Jo and Simmons (2016), Simmons and Danner (2010) use the ratification of Rome Statute as independent

the investigated party. Scholars argue that international law can constrain governments by influencing domestic support among elite actors and citizens (Kim and Sikkink 2010, Simmons 2009). It is costly to the investigated party because when domestic support decreases, it reduces the de facto power of the investigated party. Second, the ICC's investigation can decrease international support for the investigated party. The ICC's investigation sends the signal that the actor who is being investigated violates international laws to the international community (Nouwen and Werner 2010). It is costly to the investigated party because being spotlighted by the ICC could potentially lead to such consequences as sanctions and military actions from the international community. Just like state governments, rebels also benefit from external supports from other states (Huang and Sullivan 2021, Salehyan 2007, 2010) and are affected by international military actions against them (Gent 2008, Sullivan and Karreth 2015).

Given limited resources, the ICC tends to investigate individuals from the party deemed most responsible for the war crimes in the conflict instead of investigating everyone responsible. The indictment of leaders from the side that committed more serious war crimes will fulfill the goal of bringing at least some war criminals to justice while satisfying international communities and audiences. For example, in the situation of Uganda, the Ugandan military and the rebel Lord's Resistance Army both committed atrocities, but the ICC only indicted rebel leaders so far. While the Lord's Resistance Army has committed more horrific crimes against civilians in northern Uganda, soldiers in Uganda's national army have also raped, assaulted, arbitrarily jailed, and killed citizens in camps (Watch 2005). The ICC investigation resulted only in the indictment of leaders of Lord's Resistance Army, including its founder Joseph Kony. One of the parties whose leader is investigated loses both domestic support and international support. Losing both supports also means losing legitimacy at both levels.

The costs brought by the ICC differ for state leaders and rebel leaders given the unique selection process. Situations referred by state governments result in investigations of rebel groups, whereas situations referred by the UNSC or brought up as the Prosecutor proprio motu resulted in investigations of state leaders. The difference in the selection process reflects hidden biases in politics. Although an ICC investigation decreases both domestic support and international support, the referral by state government makes the ICC's investigation less legitimate, thus reducing domestic support and international support compared to the referral by UNSC or the Prosecutor proprio motu. In the case of Uganda, for example, the Ugandan government cynically referred the ongoing conflict to the ICC in an effort to gain international support for its militarization and prolong rather than end the war (Branch 2007). In the case of Libya, the UNSC referral offered the world community a chance to undermine the legitimacy of the Qadhafi dictatorship, but it also offered the ICC a chance to legitimize itself (Peskin and Boduszynski 2016). Similarly, the Prosecutor proprio motu, as it is more independent, reduces more international and domestic support, compared to the referral by state government. Domestic and international support varied as a result of the different selection process. When the state moves against rebel leaders, the ICC put lower costs on the rebel group as compared to situations referred by UNSC or Prosecutor proprio

variable. This is theoretically and empirically different from the ICC's investigations

motu against state leaders. Thus, the ICC's investigations are more effective in curbing the behavior of state governments than rebel groups.

In a world in which the ICC's investigations impose the same or similar levels of costs on state leaders and rebel groups, the ICC's investigations of state leaders or rebel leaders have heterogeneous impacts on conflicts. If the ICC investigates government leaders, the government loses both domestic support and international support so is more likely to lose control over the state. If the government loses control over the state, the rebel groups are more likely to fight with each other to get control over the state. Conversely, if the ICC investigates a rebel leader, the rebel leader loses both domestic support and international support, but the government is more likely to control the state. Thus, rebels are less likely to fight with each other.

However, the ICC's investigations impose different levels of costs on state leaders or rebel groups. If the ICC investigates government leaders, because the cost ICC's investigations can impose on state leaders is high, the government loses high levels of both domestic and international support. Then the government is more likely to lose control over the state, and the rebel groups are more likely to fight with each other to get control over it. If the ICC investigates rebel leaders, because the cost the ICC's investigation can impose on rebel groups is low, the rebel group loses low levels of both domestic and international support. In contrast to the world in which the ICC's investigations impose the same or similar levels of costs on state leaders or rebel groups, the government is not more likely to better control the state. Thus, rebels are not more or less likely to fight with each other.

What about the implications of the ICC's investigation on state-rebel conflicts? The implications are not clear because the ICC's investigation of state leader can go both ways. When the ICC investigates state leaders, it can impose costs on state leaders. When state government is somewhat constrained, the rebel group might choose to attack state government more. When government is very constrained, the rebel group might choose to not attack state government anymore.

The testable hypotheses are:

Hypothesis 1. *ICC investigations of government leaders increase non-state conflicts.*

Hypothesis 2. *ICC investigations of rebel group leaders do not increase or decrease non-state conflicts.*

2.5 Research Design

It is expected that ICC investigations of government leaders increase non-state conflicts and ICC investigations of rebel group leaders do not decrease or increase non-state conflicts. To test the heterogeneous effect of investigations, I use a difference-in-difference design that utilizes the variation in ICC's investigations over time and across states. For each hypothesis, I conduct tests using different samples. The sample for ICC investigations of government leaders covers 30 African states from 1997 to 2018, and the sample for ICC investigations of rebel group leaders covers 28 African states from 1997 to 2018. I will dis-

cuss the sample selection later. The unit of observation is state-year. In the following, I first discuss sample selection and the operationalization of the main dependent and independent variables. Then, I discuss the identification strategy.

2.5.1 Sample Selection

As I mentioned above, the samples are different for the test of the effect of ICC investigations of government leaders and rebel group leaders, because I constructed comparable samples by selecting states in which ICC's investigation could happen. To select states into the samples, I use civilian death by state or rebels: the number of civilians killed by the use of armed force by the government of a state or by a formally organized group against civilians. The data is also collected by UCDP (Eck and Hultman 2007, Pettersson, Högladh and Öberg 2019). Civilian killing is one of the war crimes that the ICC was designed to punish and deter. Most people that the ICC has publicly indicted were charged with killing civilians. The sample for the test of the effect of ICC investigations of government leaders and rebel leaders includes all African states that had at least one civilian killing by the government from 2002 to 2018. By using civilian death as the rule to select states into samples, I can have more comparable cases as counterfactual. The sample for ICC investigations of government leaders covers 30 African states from 1997 to 2018 and the sample for ICC investigations of rebel group leaders covers 28 African states from 1997 to 2018. Note that although not every country in my sample has more than one rebel group, 21 out of 27/28 states do.

2.5.2 Dependent Variable

The dependent variable is non-state conflict. I use the number of death by rebels in non-state conflicts in UCDP Georeferenced Event Dataset (Sundberg and Melander 2013) as my measure.³⁷ A non-state conflict is defined by UCDP as “the use of armed force between two organized armed groups, neither of which is the government of a state, which results in at least 25 battle-related deaths in a year” (Sundberg and Melander 2013). I aggregate event data to state-year level and for state-year that does not have any event, I code them as 0, in order to compare with other empirical studies of the ICC. The number of deaths by rebels in non-state conflict is suitable to capture the severity of conflicts between rebel groups. It is worth noting that UCDP data are coded based on media reports. It subjects to a possible media effect: ICC investigation leads to more media attention to the country, which leads to an increase in reported killings.

2.5.3 Independent Variable

The independent variable is the ICC investigations that include investigations and possible following activities such as warranty issuing, and trial Prorok (2017). I use ICC investiga-

³⁷One might vote for ACLED data (Raleigh et al. 2010). Although ACLED has more observation for each country, Eck (2012) argues that UCDP data is more accurate.

tion to measure it. I use the information on the start date of ICC investigation in situations under investigation to code it.³⁸ ICC investigation is coded as a binary variable that takes a value of 1 on and after the year when the ICC started to investigate for each state. I consider ICC's investigation as the start of investigation. ICC investigation is a conservative measurement that only codes whether the ICC is involved, ignoring the degree of investigation. Another potential way to code the ICC investigation as a variable is to count the number of ICC actions including investigation, warranty issuing, and trials of a state leader or a rebel group. Although this variable captures the degree of ICC investigation, it assumes each action weights the same. Thus, I use ICC investigation, which is the minimum level of ICC investigation to capture the concept.

To test the heterogeneous effects of ICC investigations of government leaders and ICC investigations of rebel group leaders, ICC investigations of government leaders and rebel group leaders are coded separately and used in two different tests with different samples. ICC investigations in Sudan, Kenya, Libya, Ivory Coast, and Burundi are coded as against government leaders.³⁹ ICC investigations in DR Congo (Zaire), Uganda, Central African Republic, and Mali are coded as of rebel group leaders. Table 2.2 shows the details of each situation under investigation in the data.

Table 2.2: ICC Investigations

<i>Of Government Leaders</i>	
Country	ICC Investigation Started in
Sudan	June 2005
Kenya	march 2010
Libya	march 2011
Ivory Coast	October 2011
Burundi	October 2017

<i>Of Rebel Group Leaders</i>	
Country	ICC Investigation Started in
DR Congo (Zaire)	June 2004
Uganda	July 2004
Central African Republic	May 2007
Mali	January 2013

2.5.4 Identification Strategy

I adopt generalized difference in differences (DiD) as the identification approach since the dataset is cross-sectional time-series data that includes pre-ICC investigation and post-ICC

³⁸See: <https://www.icc-cpi.int/pages/situation.aspx>

³⁹I code Sudan as against government leaders because the investigation resulted in more indicted government leaders.

investigation as well as countries or rebel groups that had ICC investigations in different years. The two-way fixed effects model, commonly referred to as generalized DiD, eliminates selection biases brought on by common time trends and time-invariant covariates. To use it as the treatment in the generalized DiD design, ICC investigation is coded as a binary variable that takes a value of 1 on and after the year in which the ICC started to involve for each state. States that had ICC investigation are “selected” into treatment group and states that had no ICC investigation are “selected” into control group.

Given the dataset is cross-sectional time-series data that covers pre-ICC investigation and post-ICC investigation and countries or rebel groups that had ICC investigations in different years, I use generalized difference in differences (DiD) as the identification strategy. Generalized DiD, also known as the two-way fixed effects model, removes selection biases caused by time-invariant unobservables and common time trends. To use it as the treatment in the generalized DiD design, ICC investigation is coded as a binary variable that takes a value of 1 on and after the year in which the ICC started to involve for each state. States that had ICC investigation are “selected” into treatment group and states that had no ICC investigation are “selected” into control group. The specifications contain year-fixed effects and country-fixed effects. Time-invariant omitted variables that could cause selection bias are controlled by country-fixed effects. The influence of common time-series patterns is captured by year-fixed effects. This specification is generally used as a regression form of difference-in-differences with treatments throughout many periods. We are comparing the changes non-state conflicts in the treatment group before and after the ICC investigation to the changes in non-state conflicts in the control group before and after the ICC investigation using this research design.

I use Ordinary Least Square (OLS) to estimate the models because it is easy to interpret and the calculation of clustered standard errors of OLS is straightforward. Since the dependent variable - the number of deaths in non-state conflicts, which is a count variable, Quasipoisson maximum likelihood regression model is used as robustness checks for the main results.⁴⁰ For both hypothesis tests, the regression equation is,

$$Non - state Conflicts_{it} = \beta_0 + \beta_1 ICC\ investigation_{i,t} + \delta X' + \gamma_i + \eta_t + \epsilon_{i,t}$$

$Non - state Conflicts_{it}$ is the number of death in non-state conflicts in state i in year t . $ICC\ investigation_{i,t}$ is the ICC investigation against government/rebel group in state i in year t . X' are covariates I shall discuss in the next section. γ_i is state-fixed effects and η_t is year-fixed effects. $\epsilon_{i,t}$ is the error term in OLS. All standard errors presented in the following results are robust standard errors clustered at state level, to account for the serial correlation within units. The unit of observation is state-year.

Recent literature in DiD found that a two way fixed effects multi-period difference-in-differences model estimates a variance-weighted average of treatment effect sometimes

⁴⁰The use of fixed effects in MLE Poisson does not suffer from the problem of incidental parameters, unlike a fixed effects maximum likelihood estimation (MLE) logit model. Thus, its estimates are unbiased (Allison and Waterman 2002, Cameron and Trivedi 2013, Greene 2004). As shown in the Appendix, the results are robust to using the quasi-Poisson models.

with some observations having negative weights (Goodman-Bacon 2021, Imai and Kim 2021, Sun and Abraham 2021) and this limits our ability to draw causal inferences from the results. Imai and Kim (2021), Imai, Kim and Wang (2019) proposed matching and weighting methods with generalized DiD to avoid negative weights. Goodman-Bacon (2021) identified the root cause of negative weights in the estimator is treatment effects that are not constant over time. Goodman-Bacon (2021) found the two-way fixed effects estimator is a weighted average of all possible combination of two-group/two-period DiD estimators in the data. When previously treated units are used as the controls, changes in their results are subtracted, and these changes may include time-varying treatment effects, causing the negative weights problem. To check whether the treatment effects in the model are constant over time, I conduct a placebo test that includes lags of the treatment. The lags also decompose the average treatment effect and help us better make causal claims when the treatment effects in the model are not constant over time.

2.5.5 Control Variables

The difference-in-differences design is dependent on parallel trends assumption that is the treatment group and the control group has similar trends in the dependent variable before the treatment. To meet the assumption, it is optimal to have the treatment to be randomly assigned; however, ICC investigation is not randomly assigned to countries, and there are selection biases caused by confounders. What are the factors that could both lead to the investigation of the ICC and the increased violence of state or rebel group? In the following, I discuss these factors used as control variables to eliminate selection biases.

The first main factor is the level of violence against civilians by governments or rebel groups. Violence against civilians by government is associated with both the investigation of the ICC of a government and the level of violence by rebel groups. Similarly, violence against civilians by rebel groups is also associated with both the investigation of the ICC of rebel and the level of violence by rebel groups. So, I include violence against civilians by the government in the test of the effect of ICC investigation on a government and the violence against civilians by rebel groups in the test of the effect of ICC investigation on a rebel group. Violence against civilians is measured by civilian death by state or rebels collected by UCDP (Eck and Hultman 2007, Pettersson, Högladh and Öberg 2019).

The second main factor is whether or not the state has ratified the Rome Statute. The ratification of the Rome Statute by a state makes the investigation of ICC more likely (Prorok 2017), and it also could be related to violence by rebel groups because the state is more constrained by the ICC. I include the ratification of the Rome Statute in both tests of the effect of ICC investigation on a government and the effect of ICC investigation on rebel groups. The ratification of the Rome Statute is coded as 1 on and after the year in which the state ratifies the Rome Statute and 0 otherwise based on the info on the ICC's website.⁴¹

The third main factor is democracy. Non-democracies are more likely to have the ICC involved and a higher level of violence by rebel group. I use POLITY2 in Polity V dataset

⁴¹Please see:https://asp.icc-cpi.int/en_menus/asp/Pages/asp_home.aspx

to measure democracy.⁴² POLITY2 is a revised combined policy score that facilitates the use of the POLITY regime measure in time-series analyses. It ranges from +10 (strongly democratic) to -10 (strongly autocratic).⁴³

2.6 Results

Table 2.3 shows the results for ICC investigations of government leaders, which strongly support the hypothesis that ICC investigations of government leaders increase non-state conflicts.⁴⁴ ICC investigations of government are positively associated with non-state conflict deaths. Column (1) shows that coefficient estimates of the effect of ICC investigations of a government on non-state conflict deaths is 193.894, and the estimate is statistically significant at 0.01 level. Thus, ICC investigations of a government leads to 194 more deaths in non-state conflict on average. Column (2) shows the results are robust to the inclusion of all three control variables. The table shows ICC investigations of government leaders increase non-state conflicts.

Table 2.4 shows the results for ICC investigations of rebel group leaders, which support the hypothesis that ICC investigations of rebel group leaders do not increase or decrease non-state conflicts.⁴⁵ ICC investigations of rebel groups are negatively associated with non-state conflict deaths, but the results are not statistically significant. Column (1) shows the coefficient estimate of the effect of ICC investigation of government on non-state conflict deaths is -179.029. It is statistically insignificant. Column (2) shows the results are robust to the inclusion of all three control variables. The table shows ICC investigations of rebel group leaders do not increase or decrease non-state conflicts.

⁴²Please see: <http://www.systemicpeace.org/inscrdata.html>

⁴³Alternative explanations are explained in the appendix.

⁴⁴As shown in the Appendix, the empirical results are robust to quasipoisson models, simple OLS, Fixed Effects, and different specifications of control variables.

⁴⁵As shown in the Appendix, the empirical results are robust to quasipoisson models, simple OLS, Fixed Effects, and different specifications of control variables.

Table 2.3: The Effect of ICC Investigation of Governments on Non-state Conflict Deaths

	(1)	(2)
ICC Investigation	193.894*** (71.013)	223.325*** (82.024)
Civilian Death by Government, $t-1$		0.071 (0.087)
Rome Statute		-43.014 (38.706)
Polity V		-8.787 (5.422)
State FE	Y	Y
Year FE	Y	Y
	State = 30 N = 660	State = 29 N = 616

Note: p-values are two-tailed. *p<0.1; **p<0.05; ***p<0.01.
Robust standard errors are clustered by state.

Table 2.4: The Effect of ICC Investigation of Rebel Groups on Non-state Conflict Deaths

	(1)	(2)
ICC Investigation	-179.029 (117.762)	-203.827 (134.774)
Civilian Death by Rebel, <i>t-1</i>		-0.012 (0.054)
Rome Statute		-22.708 (47.157)
Polity V		-7.345 (6.651)
State FE	Y	Y
Year FE	Y	Y
	State = 28 N = 616	State = 27 N = 572

Note: p-values are two-tailed. *p<0.1; **p<0.05; ***p<0.01. Robust standard errors are clustered by state.

Placebo Tests

Difference-in-difference design requires the data to satisfy the parallel trends assumption: the treatment group (states that had ICC investigation) and the control group (states that did not have ICC investigation) have similar trends in deaths in non-state conflicts prior to the treatment (ICC investigation). If ICC investigations are randomly assigned, the treatment group and control group should have similar trends in deaths in non-state conflicts. As I discussed, however, the ICC selects situations to investigate; that is, ICC investigations are not random. Thus, it is important to further investigate the extent to which the data meet the parallel trends assumption required by difference-in-difference design for causal inference. To test the parallel trends assumption, I include five hypothetical leads of ICC investigation. If the data meet the assumption, five hypothetical leads should have no effects on non-state conflicts. I also included four lags of ICC investigation to test whether ICC investigation has long-term effects on non-state conflicts and decompose the average treatment effect. In addition, I included five lags of ICC investigation in the models to test whether the treatment has a lasting effect on non-state conflicts. Note, ICC investigation is coded only on the year in which the ICC investigation occurred.

Figure 2.1: Placebo Test: The Effect of ICC Investigation of Governments

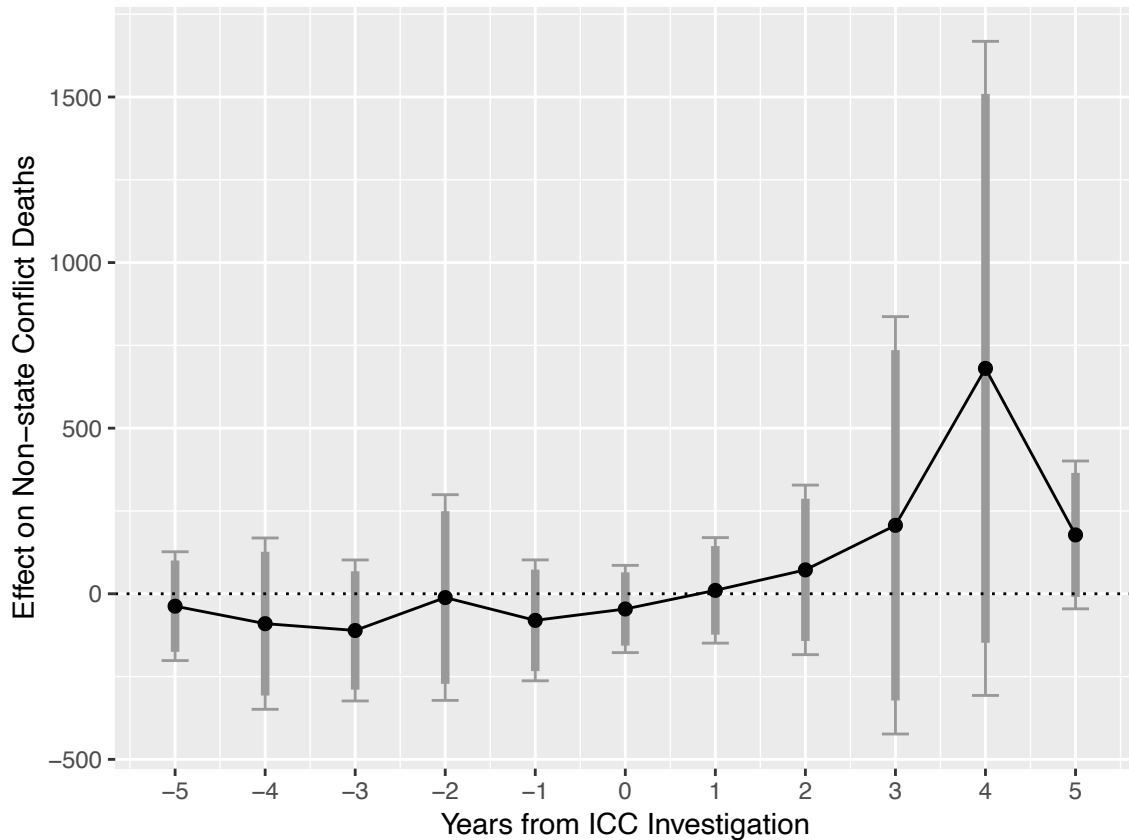


Figure 2.1 shows a plot of the coefficients of ICC Investigation of government leaders (at time, $t = 0$) and the associated leads and lags.⁴⁶ Black dots represent point estimates; bars show 95% confidence intervals, and bold lines show 90% confidence intervals. As we can see from the figure, the effect of the hypothetical ICC investigation of government leaders fails to reach traditional levels of statistical significance in years prior to the actual ICC investigation, and the magnitude of the effects is very close to zero. It indicates that the treatment group and the control group have similar trends in non-state conflicts before ICC investigation. The magnitude of the effects of five lags of ICC investigation is above zero and increasing in the first four years, showing ICC investigations of government leaders have some long-term effects on increasing deaths in non-state conflict, although they also failed to reach traditional levels of statistical significance.

⁴⁶A full table of results is provided in the Appendix.

Figure 2.2: Placebo Test: The Effect of ICC Investigation of Rebel Groups

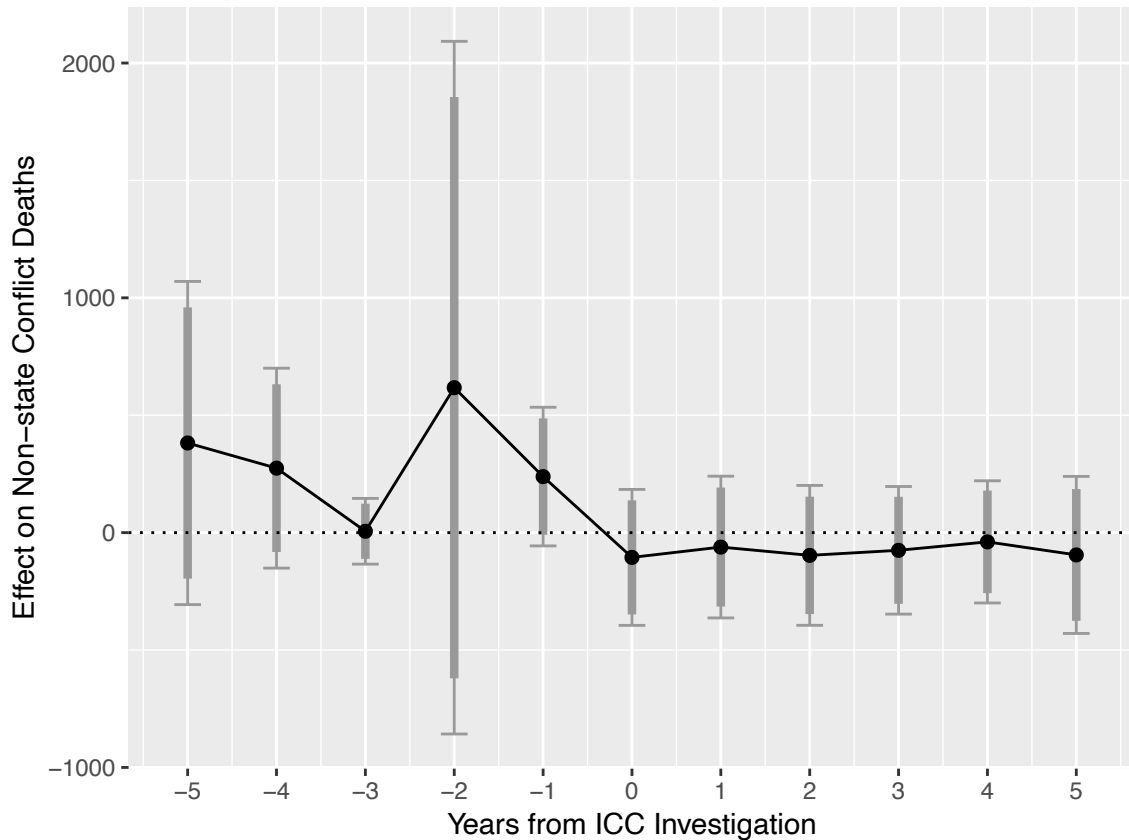


Figure 2.2 shows a plot of the coefficients of ICC investigation of rebel group leaders (at time, $t = 0$) and the associated leads and lags.⁴⁷ Black dots represent point estimates; bars show 95% confidence intervals, and bold lines show 90% confidence intervals. As we can see from the figure, the effect of the hypothetical ICC investigation of rebel group leaders fails to reach traditional levels of statistical significance in years prior to the ICC investigation, but the magnitude of the effects is not very close to zero. It shows some support that the treatment group and the control group have similar trends in non-state conflicts before ICC investigation. The magnitude of the effects of five lags of ICC investigation is constantly below zero but very close to zero, showing ICC investigations of government leaders have no long-term effects on reducing or increasing deaths in non-state conflict.

2.7 Conclusion

Scholars have debated whether the ICC's pursuit of justice can reduce conflicts and promote peace. In this article, I argue that the ICC's investigations of governments and rebel groups have different effects on civil conflict. When the ICC investigates a government,

⁴⁷A full table of results is provided in the Appendix.

it constrains the government, providing incentives for the rebel groups to fight and leading to more non-state conflicts. When the ICC investigates rebel groups, however, it does not constrain those groups, so strengthens a government's control and reduces non-state conflicts. The difference in deterrent and constraining power of the ICC's investigations was caused by the difference in the ICC's situation selection process. Situations referred by state governments resulted in the ICC's investigating rebel groups. Such investigations are seen as less legitimate and biased, so reduce domestic and international support for rebel groups. Situations referred by the UNSC or brought up by the Prosecutor proprio motu resulted in the ICC's investigation of state leaders. Such investigations are seen as more legitimate and unbiased, thus they reduce domestic and international support for state government. Using difference-in-difference research designs, I show that the ICC's investigations of governments and rebel groups have heterogeneous effects on civil conflict: Whereas ICC investigations of government leaders increase deaths in non-state conflicts, investigations of rebel group leaders do not reduce deaths in non-state conflicts.

The paper contributes to the literature of international intervention and civil conflicts in two ways. First, the paper highlights the implications of biases in international intervention on conflicts. In terms of ICC investigations, situations referred by state governments against rebel leaders are less legitimate and more biased, compared to situations referred by UNSC or brought by the Prosecutor proprio motu against state leaders. The situation selection process might be unique to the ICC, but it shows how biases in the process of international interventions can affect domestic politics and civil conflicts. Second, it highlights the importance of looking at multiple domestic actors and how their power relationship changes when studying international intervention. ICC investigations of government leaders and rebel group leaders are theoretically different and have theoretically different effects on conflicts. It shows that when international interventions target different domestic actors, it changes the power relationship between state government and rebel groups differently.

For future research, there are three directions in which this paper might be extended. First, future studies might test my theory by looking at cases qualitatively and test the causal chain from situation selection to support reduction, then to changes in conflict. Second, scholars might be able to extend the framework in this paper to study the effect of the ICC on curbing human rights violations. The unique situation selection process and the different costs it puts on state leaders and rebel leaders should create heterogeneous effects on deterring human rights violations as well. Last but not the least, scholars can extend this to look at the effects of other international interventions that are biased towards different actors on conflicts or human rights conditions.

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2.8 Appendix

2.8.1 Descriptives

Table 2.5: Descriptive Data for the Effect of ICC Investigation of Governments

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Year	660	2,007.500	6.349	1,997	2,002	2,007.5	2,013	2,018
ICC Investigation	660	0.062	0.242	0	0	0	0	1
Treatment Year	660	0.008	0.087	0	0	0	0	1
Non-state Conflict Death	660	111.477	305.559	0	0	0	43	2,969
Civilian Death by Government	660	59.862	292.978	0	0	0	9	4,120
Civilian Death by Government, <i>t-1</i>	660	63.156	302.822	0	0	0	8	4,120
Rome Statute	660	0.455	0.498	0	0	0	1	1
Polity V	616	0.591	4.590	-7.000	-3.000	-1.000	5.000	9.000

Table 2.6: Descriptive Data for the Effect of ICC Investigation of Rebels

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Year	616	2,007.500	6.349	1,997	2,002	2,007.5	2,013	2,018
ICC Investigation	616	0.078	0.268	0	0	0	0	1
Treatment Year	616	0.006	0.080	0	0	0	0	1
Non-state Conflict Death	616	119.469	314.926	0	0	0	52.2	2,969
Civilian Death by Rebel	616	105.442	451.722	0	0	0	20	5,063
Civilian Death by Rebel, <i>t-1</i>	616	157.560	1,349.950	0	0	0	19	31,661
Rome Statute	616	0.425	0.495	0	0	0	1	1
Polity V	572	0.503	4.400	-7.000	-3.000	0.000	5.000	9.000

2.8.2 Alternative Specifications

Table 2.7: The Effect of ICC Investigation of Governments on Non-state Conflict Deaths: Specifications

	Simple OLS	Fixed Effects	Civilian Death	Rome Statute	Polity V
ICC Investigation	285.814*** (76.527)	193.645*** (63.723)	190.384*** (71.964)	194.812*** (70.715)	228.637*** (80.252)
Civilian Death by Government, $t-1$			0.079 (0.086)		
Rome Statute				-42.497 (38.515)	
Polity V					-9.027* (5.345)
State FE	N	Y	Y	Y	Y
Year FE	N	N	Y	Y	Y
	State = 30 N = 660	State = 30 N = 660	State = 30 N = 660	State = 30 N = 660	State = 29 N = 616

Note: p-values are two-tailed. *p<0.1; **p<0.05; ***p<0.01. Robust standard errors are clustered by state.

Table 2.8: The Effect of ICC Investigation of Rebels on Non-state Conflict Deaths: Specifications

	Simple OLS	Fixed Effects	Civilian Death	Rome Statute	Polity V
ICC Investigation	54.259 (50.788)	-161.225 (118.996)	-194.046 (127.580)	-174.235 (120.917)	-191.249 (120.797)
Civilian Death by Rebel, $t-1$			-0.011 (0.055)		
Rome Statute				-28.861 (42.660)	
Polity V					-7.750 (6.139)
State FE	N	Y	Y	Y	Y
Year FE	N	N	Y	Y	Y
	State = 28 N = 616	State = 28 N = 616	State = 28 N = 616	State = 28 N = 616	State = 27 N = 572

Note: p-values are two-tailed. *p<0.1; **p<0.05; ***p<0.01. Robust standard errors are clustered by state.

2.8.3 Quasipoisson Results

Table 2.9: The Effect of ICC Investigation of Governments on Non-state Conflict Deaths: Quasipoisson

	(1)	(2)
ICC Investigation	1.152*** (0.363)	1.621*** (0.489)
Civilian Death by Government, $t-1$		0.0001 (0.0002)
Rome Statute		-0.087 (0.458)
Polity V		-0.098** (0.047)
State FE	Y	Y
Year FE	Y	Y
	State = 30 N = 660	State = 29 N = 616

Note: p-values are two-tailed. *p<0.1; **p<0.05; ***p<0.01.
Robust standard errors are clustered by state.

Table 2.10: The Effect of ICC Investigation of Rebel Groups on Non-state Conflict Deaths: Quasipoisson

	(1)	(2)
ICC Investigation	-0.802 (0.516)	-1.041 (0.687)
Civilian Death by Rebel, <i>t-1</i>		-0.00003 (0.0001)
Rome Statute		-0.145 (0.558)
Polity V		-0.058 (0.044)
State FE	Y	Y
Year FE	Y	Y
	State = 28 N = 616	State = 27 N = 572

Note: p-values are two-tailed. *p<0.1; **p<0.05; ***p<0.01. Robust standard errors are clustered by state.

2.8.4 Placebo Tests Results

Figure 2.3: Placebo Test: The Effect of ICC Investigation of Governments with Controls

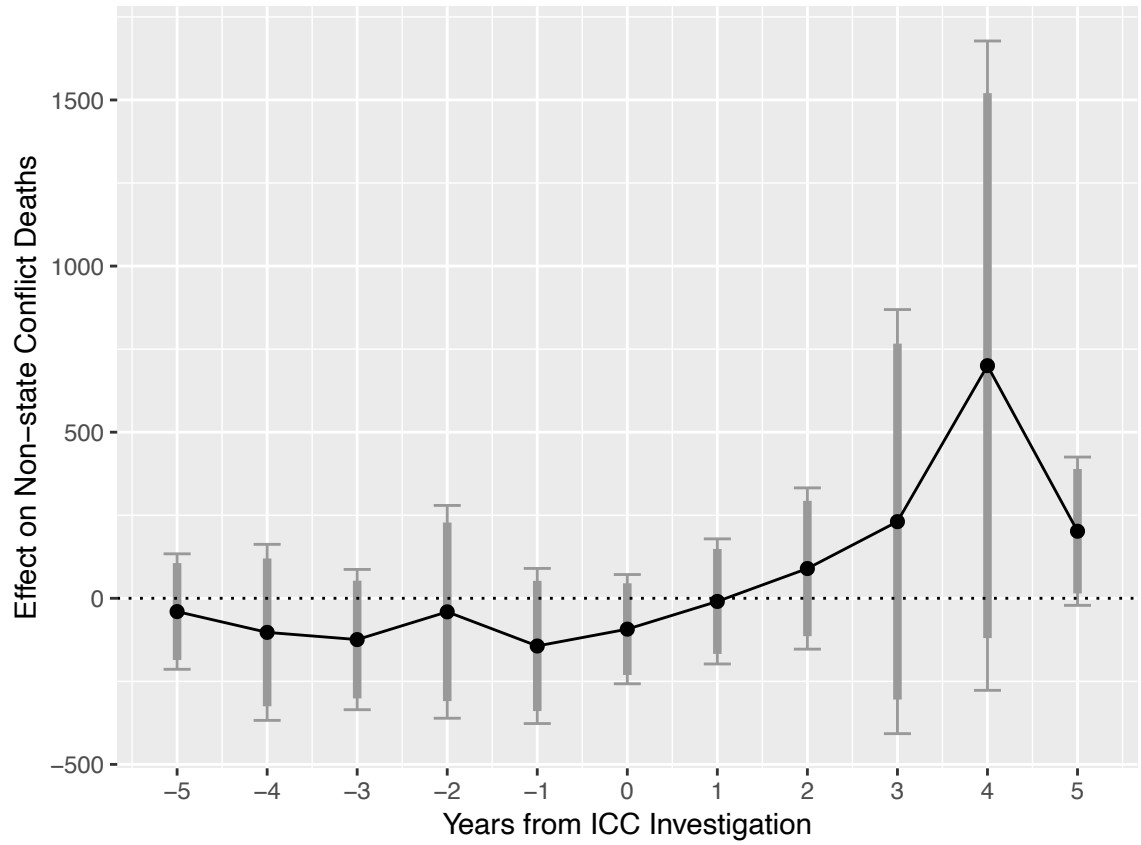


Figure 2.4: Placebo Test: The Effect of ICC Investigation of Rebel Groups with Controls

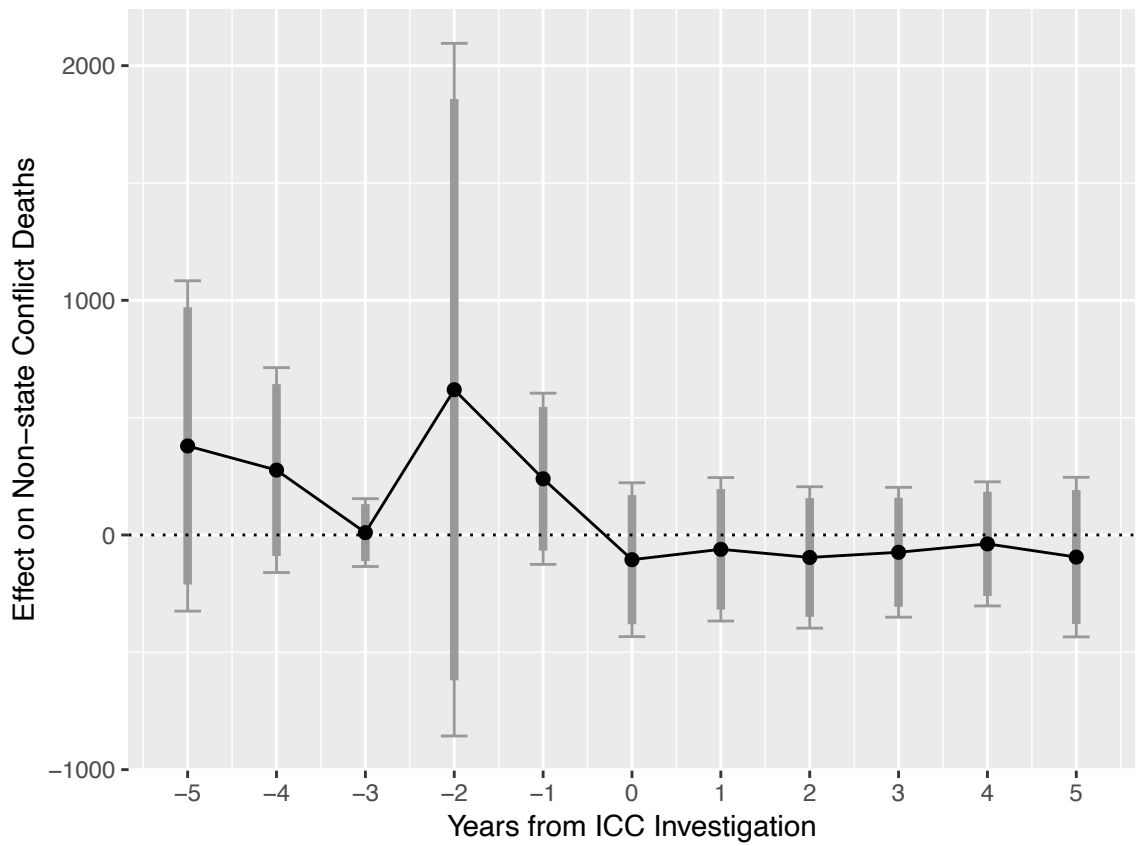


Table 2.11: The Effect of ICC Investigation of Governments on Non-state Conflict Deaths

	(1)	(2)
t-5	-37.413 (83.796)	-39.977 (88.681)
t-4	-90.173 (131.959)	-102.582 (135.202)
t-3	-110.787 (108.617)	-124.212 (107.711)
t-2	-11.368 (158.427)	-40.751 (163.406)
t-1	-80.261 (93.135)	-143.548 (119.120)
t	-45.949 (67.214)	-92.883 (83.899)
t-1	10.305 (81.292)	-9.457 (96.059)
t-2	72.196 (130.584)	89.597 (123.830)
t-3	206.719 (321.501)	230.815 (325.654)
t-4	680.540 (503.825)	700.265 (498.642)
t-5	177.727 (113.871)	201.890* (113.875)
Civilian Death by Government		0.083 (0.089)
Rome Statute		-53.712 (40.030)
Polity V		-11.058** (5.233)
State FE	Y	Y
Year FE	Y	Y
	State = 28 N = 616	State = 27 N = 572

Note: p-values are two-tailed. *p<0.1; **p<0.05; ***p<0.01.
Robust standard errors are clustered by state.

Table 2.12: The Effect of ICC Investigation of Rebel Groups on Non-state Conflict Deaths

	(1)	(2)
t-5	381.773 (351.126)	379.358 (359.149)
t-4	274.670 (217.139)	276.544 (222.738)
t-3	5.841 (71.428)	10.102 (73.732)
t-2	617.402 (752.577)	619.177 (753.038)
t-1	238.727 (150.636)	239.434 (186.128)
t	-105.421 (147.691)	-105.369 (167.288)
t-1	-61.433 (153.977)	-61.109 (155.955)
t-2	-96.788 (151.994)	-95.803 (153.757)
t-3	-75.272 (138.629)	-73.726 (141.143)
t-4	-39.327 (132.756)	-37.946 (134.913)
t-5	-94.918 (170.629)	-94.192 (173.489)
Civilian Death by Rebel		0.003 (0.102)
Rome Statute		7.939 (42.466)
Polity V		1.819 (3.935)
State FE	Y	Y
Year FE	Y	Y
	State = 28	State = 27
	N = 616	N = 572

Note: p-values are two-tailed. *p<0.1; **p<0.05; ***p<0.01. Robust standard errors are clustered by state.

Part III

Transparency and Citizens' Collective Action: The Effect of Freedom of Information Laws on Protests

Abstract

Do freedom of information (FOI) laws increase or reduce protests? While more transparency in government should theoretically increase citizens' satisfaction and make them less likely to participate in protests, I argue that providing citizens with information about the wrongdoings of lower-tier officials without giving them ways to punish those officials leads to more protests. Using a difference-in-difference design and cross-country data, I find that adopting FOI laws is associated with statistically and substantively significant increases in the number of protests. More importantly, I found the relationship between the adoption of FOI law and the number of protests is conditioned by electoral democracy, a measure of ways to punish government officials. When there are ways to punish those officials, FOI laws do not lead to more protests.

3.1 Introduction

In 2005, the Parliament of India passed the right to information (RTI) act, which is a freedom of information (FOI) law. Under this act, any citizen of India may request information from any government authority or institution. The RTI act made India one of the countries with the most functional right to information laws.⁴⁸ Although it increased the transparency of the government, some local government officials who were revealed as being corrupt were not being punished. This led to protests against local officials.⁴⁹ It seems that RTI was not functioning and caused more problems. Do FOI laws lead to more or fewer protests? What conditions affect this?

By 2018, 119 countries had adopted freedom of information laws to give citizens access to government information. Most of them adopted the law after 1990; the number of countries with FOI laws peaked at 52 during the 2000s.⁵⁰ FOI laws are expected to boost openness, improve the government's image, minimize corruption, and better serve the needs of the citizens. Governments are expected to become more responsive, effective, and efficient due to increased transparency (Ackerman and Sandoval-Ballesteros 2006, Cucciniello, Porumbescu and Grimmelikhuisen 2017, Florini 2000, Hood and Heald 2006, Kosack and Fung 2014). But scholars have discovered that FOI rules are not as effective as anticipated: It has been discovered that FOI is linked to higher levels of public sector corruption in developing nations (Escaleras, Lin and Register 2010). After an FOI law was passed, perceived corruption increased, and the quality of government decreased (Costa 2013). Government decision-making, public understanding, participation, and trust in government did not improve as a result of FOI laws (Worthy 2010). More government information is made available to the public through the FOI process. Providing citizens with information on their government has been crucial for organizing them in mass actions such as protests (Hollyer, Rosendorff and Vreeland 2015, Pierskalla and Hollenbach 2013, Rød and Weidmann 2015, Zheng and Wu 2005). In theory, FOI laws as a form of releasing government information should affect protests. However, no studies have looked at the effect of FOI laws on protests.

In this paper, I study that effect, tying together literature about the effectiveness of FOI laws and the impact of disclosing government information on protests. I argue that FOI laws provide citizens with information about the wrongdoings of lower-tier officials without giving them ways to punish those officials, which leads to more protests. In most settings, the government may not be able to punish such officials through legal means, and citizens may not be able to punish the official through elections after the release of requested information about major wrongdoings. Information about wrongdoing serves as a focal point where citizens can mobilize when they learn that the official is not punished and they have no way to punish the official. Using difference-in-difference designs and cross-country data, I find that adopting FOI laws is associated with statistically and substantively significant increases

⁴⁸<https://timesofindia.indiatimes.com/india/rTI-rank-india-slips-a-spot-to-no-6/articleshow/66172060.cms>

⁴⁹One example: <https://www.tribuneindia.com/news/ludhiana/rTI-activist-protests-against-br-branch-over-%E2%80%98corrupt-practices%E2%80%99-97585>

⁵⁰Table 3.1 later in the research design part shows the distribution of adoption of FOI laws over time.

in the number of protests. More importantly, I found the relationship between the adoption of FOI law and the number of protest changes by the level of electoral democracy, which is a measure of whether there are ways to punish government officials. When there are ways to punish those officials, FOI law does not lead to more protests.

Although government information has theoretically been crucial for mobilizing citizens in mass actions such as protests, this article is the first to examine the relationship between FOI laws and protests. This paper took an initial step toward building a theory connecting FOI laws and protests with corruption and tested it empirically with cross-country data. More importantly, this paper highlights the significance of the distinction between detecting corruption and punishing corruption. FOI laws might be helpful in detecting corruption and useful in identifying corruption. However, if corruption is simply detected and not punished in the system, citizens would get dissatisfied and act outside the rules. Without more means of enforcement, freedom of information acts will not be able to reduce corruption.

3.2 FOI Laws, Transparency, and Protests

FOI laws are expected to increase government transparency, reduce corruption, improve government image, and address citizens' needs. Moreover, transparency in governments should theoretically make governments more responsive, effective, and efficient (Ackerman and Sandoval-Ballesteros 2006, Cucciniello, Porumbescu and Grimmelikhuijsen 2017, Florini 2000, Hood and Heald 2006, Kosack and Fung 2014). This theoretical expectation of FOI laws is supported by large-N cross-country studies. Vadlamannati and Cooray (2017) found FOI laws are associated with increases in the perception of government corruption, suggesting that they lead to greater reporting and more observed corruption. Vadlamannati and Cooray (2017) also found the effects are amplified with a higher degree of media freedom, presence of NGOs, and competitiveness in political participation. Islam (2006), using the existence of an FOI law and the length of its existence, measured transparency and found that countries with better transparency measured by these indices are associated with better governance, that is, less corruption. There is also evidence in country-specific studies supporting the idea that FOI laws lead to better governance. In a state-level large-N study of the U.S., Cordis and Warren (2014) found switching from a weak to a strong FOI law doubled the corruption conviction rates of state and local officials, suggesting that FOI law leads to the detection of more corruption. As the time between switching from a weak to a strong FOI law increases, conviction rates decline, suggesting an FOI law reduces underlying corruption levels in the long term. In two studies about the RTI (right to information) act in India, Peisakhin and Peisakhin and Pinto (2010) and Peisakhin (2012) found the use of FOI laws is almost as effective as bribery in securing access to basic public services, suggesting FOI laws make the government more responsive.

However, some scholars have found FOI laws are not as effective as people expected. Bac (2001) argues that while a higher level of transparency leads to an increase in the probability of corruption being detected, it may also lead to an increase in corruption because it provides outsiders with more information about the identities of key decision-makers within government to target for bribes. Large-N cross-country studies also found FOI laws might

not reduce corruption and increase the quality of governance. Escaleras, Lin and Register (2010) found there is no significant relationship between FOI and corruption except that FOI has been found to be associated with more public sector corruption in developing countries. Costa (2013) found that perceived corruption rose, and the quality of governance fell following the adoption of an FOI law. In a study that examines the effects of the United Kingdom's FOI act, Worthy (2010) used official literature, interviews with officials, an online survey of FOI requesters, and media analysis and found FOI did not improve decision-making by the government, public understanding, participation, and trust in government.

An FOI law is a means to provide transparency; that is, more government information is publicly available to citizens. Information about the government has been essential for mobilizing citizens in collective actions such as protests. Hollyer, Rosendorff and Vreeland (2015) argue that because transparency makes information about underperforming leadership publicly available and widely shareable, it eases protests. The free flow of information over the Internet promotes collective actions and protests (Lynch 2011, Zheng and Wu 2005). Cell phones as a communication technology aid the flow of information and the coordination of beliefs that helped facilitate collective actions in Africa (Pierskalla and Hollenbach 2013).

So far, no studies have looked at the effect of FOI laws on protests, although FOI laws as a form of releasing government information should have an impact on protests. If FOI laws are effective at increasing government transparency, reducing corruption, improving government image, and addressing citizens' needs, citizens should be more satisfied and less likely to protest. However, what would happen if FOI laws are not effective? In this paper, I study the effect of FOI laws on protests, connecting the literature about the effectiveness of FOI laws and the impact of releasing government information on protests. In what follows, I first build a theory that connects FOI laws, corruption, democracy, and protests. Then I test the implications of my theory empirically with data on protests.

3.3 Theory

My theory has three actors: government leaders, government officials, and citizens. I assume the government leader wants to stay in power. This is a common assumption used in political science regarding the preference of government leaders (De Mesquita et al. 2005, 1999, 2002, Gandhi and Przeworski 2007). Staying in power makes rent-seeking possible. I assume that, to stay in power, the government leader wants the government to be more transparent to detect corruption at lower levels of government and satisfy the citizens. But the government leader doesn't want wrongdoings of his or hers known by the citizens. A government official is a lower-tier government agent who works for the government leader. The official wants to stay in power, and I also assume the government official wants to seek rent through illegal channels. Corruption generally exists in most settings, less democratic or more democratic (Chang and Golden 2007, 2010, Rock 2009). Staying in power enables them to seek illegal rents, but seeking too much illegal rent endangers their job. Citizens want the government to be more transparent and accountable at all levels. The citizens are assumed to want to reveal information about wrongdoings of the government leaders and

officials and the correction of wrongdoings.

When the government provides platforms for all citizens to request information, citizens will request information, expecting the revealed information leads the government to correct wrongdoing by its leaders or officials. Theoretically, citizens can punish leaders or officials through elections when elections are available (De Vries and Solaz 2017, Winters and Weitz-Shapiro 2016). However, when the requested information is related to the leader, the leader can use strategies to avoid the release of that information. Roberts (2005) found that, while the Canadian FOI law was intended to constrain executive power, officials developed administrative routines and technologies to minimize its disruptive potential to leaders. Given a leader's power to avoid the release of information in a democratic country, I assume leaders in less democratic countries also have the power to do so. In most settings, when the requested information is related to serious wrongdoings of an official, the government may not be able to punish the official through legal means, and the citizens cannot punish the official through elections because some officials have networks and resources to avoid punishment and stay in power. Vuković (2020) found because of personal connections and mutual dependence on a rent-extracting relationship formed between firms and political elites, politicians can engage in corruption and still win elections. When the citizens find out that the official is not punished or they have no means to punish the official, the information about wrongdoing serves as a focal point for citizens to mobilize collectively (Hollyer, Rosendorff and Vreeland 2015, Tucker 2007). When the government reveals more information about its wrongdoing, but citizens have little means to punish the government through the system, they are likely to protest (Hollyer, Rosendorff and Vreeland 2015, 2019). Only in countries with a very high level of democracy is the government likely to punish the official through legal means or the citizens are likely to punish the official through elections.

One puzzle might be why the leaders in authoritarian countries adopt platforms for all citizens to request information in the first place if they expect there will be more protests. I argue, first, that because some less powerful lower-tier officials are identified and punished, it helps the leader to stay in power. Not all lower-tier officials are punished. The leader selects officials outside his or her power networks to consolidate his or her power (Carothers 2022, Pei 2018, Zhu and Zhang 2017). Even though some out-of-network lower-tier officials are punished, officials are less likely to be punished in an authoritarian regime compared to an electoral democracy. Second, a protest against lower-tier officials is not a bad thing for the leader. It will make the government more responsive (Chen, Pan and Xu 2016) and therefore help the leader to govern the country. Third, because the protests are against the government, not the leader, citizens' attention is moved away from the leader. The testable hypotheses are:

Hypothesis 1. *Adopting freedom of information laws increases the number of protests.*

Hypothesis 2. *Adopting freedom of information laws in countries without a high level of democracy increases the number of protests.*

Hypothesis 3. *Adopting freedom of information laws in countries with a high democracy level does not increase the number of protests.*

3.4 Research Design

I expected the adoption of FOI laws to lead to more protests. To test the hypothesis, I use data from the Armed Conflict Location and Event Dataset (ACLED) (Raleigh et al. 2010), which provides daily violence event count data that include protests. Because FOI laws data ends in 2018, I trimmed ACLED data to 2018. The data show protest events in 151 countries between 1997 and 2018. To create a balanced sample, I include all 202 countries in the data set of analysis for years from 1997 to 2018. The unit of analysis is country-year.⁵¹ In this section, I first introduce the main variables, and then I introduce identification strategies.

3.4.1 Dependent Variable

The dependent variable is protests, measured by the number of protests aggregated at the country-year level.⁵² A protest is defined as “a public demonstration in which the participants do not engage in violence, though violence may be used against them” (Raleigh et al. 2010). Protests include individuals and groups who peacefully demonstrate against a political entity, government institution, policy, group, tradition, business, or other private institutions (Raleigh et al. 2010). As I mentioned, ACLED data record protests as event data. To measure the number of protests at a country-year level, protest events are aggregated to country-year level, and a country-year with no protests is labeled as 0.

3.4.2 Independent Variables

The independent variable is the adoption of FOI laws. As of 2018, 119 countries had adopted FOI laws.⁵³ Table 3.1 shows the distribution of the adoption of FOI laws over time. Most countries adopted such a law after the 1990s. In the 1990s, 20 countries adopted FOI laws. The number of countries that adopted FOI laws peaked at 52 during the 2000s and was at 38 from 2010 to 2018. Following the practice of (Banisar 2006), the variable is coded as 1 on and after the country-year the FOI law was adopted and 0 otherwise. FOI law adoption coded here is de jure rather than de facto. De jure adoption means that the country has publicly announced adoption of an FOI law; that is, on paper the law is adopted. It does not necessarily mean the infrastructure has been built completely, and the law is fully

⁵¹Both ACLED and FOI laws data are event data that has exact date. However, V-dem data I use to measure democracy is at country-year level.

⁵²ACLED data does not have size or other measurements about the magnitude of protests. It does have fatalities, but fatalities is not a good measurement of protests.

⁵³As far as my knowledge goes, no FOI law has been repealed yet. In 2001, the Paraguayan government repealed a controversial new information access law that limited journalists’ access to public records. However, this law was not a freedom of information law because it contained restrictions. Please see <https://ifex.org/president-repeals-controversial-information-law/> for more details.

functioning. Thus, de jure adoption is considered the minimal level of FOI law adoption. Because de jure adoption is used to measure FOI law adoption in the tests, the tests will be conservative. I use the data from freedominfo.org to code the variable.⁵⁴

Table 3.1: The Distribution of Adoption of FOI Laws over Time

Years	Number of Countries adopted FOI law
Before1980	3
1980 - 1989	6
1990 - 1999	20
2000 - 2009	52
2010 - 2018	38

To test the effect of FOI laws conditioned on democracy levels (H2 and H3), I use the electoral democracy index from the V-dem dataset (Coppedge et al. 2021, Lindberg et al. 2014) to measure the level of democracy. The electoral democracy index partially fits my theory because it measures to what degree an election is clean and to what degree an election can make rulers accountable. In my theory, I argue that only in countries with a very high level of democracy is the government likely to punish officials through legal means or are the citizens likely to punish officials through elections. Citizens are more likely to use elections to punish the official in a country with a high electoral democracy index. The other part of democracy at work is to punish the official through legal means, which is the rule of law principle. Since I could not find a good measure of the rule of law and none of the other four democracy indices (liberal, participatory, deliberative, and egalitarian democracy) in V-dem is related to the rule of law, I only use the electoral democracy index to measure the level of democracy.⁵⁵ The index ranges from 0 to 1, with 1 as the maximum level of electoral democracy. I interact the electoral democracy index with FOI law to test H2 and H3.⁵⁶

3.4.3 Identification Strategy

Because the dataset contains cross-sectional time-series data covering pre-FOI law adoption and post-FOI law adoption periods and countries adopted FOI laws in different years, I use generalized difference-in-differences (DiD) as the identification strategy. Generalized DiD, also known as the two-way fixed effects model, removes selection biases caused by time-invariant unobservables and common time trends. Country-fixed effects and year-fixed effects are included in all specifications. Country-fixed effects control time-invariant omitted variables that could lead to selection bias. Year-fixed effects capture the influence of common time-series trends that affect the estimation. This specification is generally used

⁵⁴See the data at <http://www.freedominfo.org/?p=18223>

⁵⁵Electoral democracy index should be highly correlated with levels of rule of law.

⁵⁶Descriptive statistics of variables are in the appendix.

as the regression form of difference-in-differences with treatments in multiple periods. With this research design, I am comparing the changes in protests in countries that adopted FOI before and after FOI adoption to protests in countries that did not adopt before and after FOI adoption. I use panel linear regression to estimate the results because interaction effects are easy to calculate and interpret.⁵⁷ The regression equations are:

$$Protests_{i,t} = \beta_0 + \beta_1 FOI\ law_{i,t} + \delta X' + \gamma_i + \eta_t + \epsilon_{i,t}$$

$$Protests_{i,t} = \beta_0 + \beta_1 FOI\ law_{i,t} + \beta_2 Electoral\ democracy\ index_{i,t} + \beta_3 FOI\ law_{i,t} * Electoral\ democracy\ index_{i,t} + \delta X' + \gamma_i + \eta_t + \epsilon_{i,t}$$

Protests is the number of protests in country *i* in year *t*. *FOI law_{i,t}* is FOI adoption in country *i* in year *t*. *Electoral democracy index_{i,t}* is the electoral democracy index in country *i* in year *t*. γ_i is country-fixed effects and η_t is year-fixed effects. All standard errors shown in the following results are robust standard errors clustered at country level, to account for the serial correlation within units. Since FOI laws are not randomly assigned to countries, I include controls in some specifications. X' are control variables that could be correlated with both FOI adoption and protests: different types of democracy indices and GDP per capita. To see the heterogeneous effects of FOI adoption conditioned on the democracy level, I interact *FOI law_{i,t}* with *Electoral democracy index_{i,t}*.

3.4.4 Controls for Treatment Assignment Process

Difference-in-difference designs rely on the parallel-trends assumption, which is that the difference between the treatment and control group in pre-treatment outcomes is the same over time. If the treatment is randomly assigned to the treatment and control group, the parallel-trends assumption is met. However, the adoption of FOI law is not random, and there are selection biases caused by confounders. What might be the factors that determine the treatment assignment process of the adoption of FOI laws and also correlates with protests? I argue there are two factors: democracy and economic development. In the following, I discuss why these two factors matter and how I use them as controls in the models to eliminate selection biases. Also, later in the paper, I conduct placebo tests to formally test the parallel-trends assumption.

The first reason some countries adopt FOI laws is they are more democratic. Democratic countries are more likely to have FOI laws because an FOI law is an organic part of democracy. Having an FOI law matches a lot of democratic values and processes. Politicians in democratic countries support FOI laws because their symbolic value makes it difficult to oppose them (Worthy 2017). In a democracy that emphasizes accountability, protection of people's rights, participation by citizens in political processes, public reasoning of policy, and equal rights, recourses, and political power (Coppedge et al. 2021, Lindberg et al. 2014),

⁵⁷I use Logged dependent variable, Negative Binomial, Poisson, and Quasi-Poisson models as robustness checks. Please see the appendix for results

FOI can help meet these goals by sharing information and curbing the government's behavior. Democracy is also associated with fewer protests because citizens' needs are generally met in democracies. Therefore, in some specifications and as robustness checks, I control five democracy indices (electoral, liberal, participatory, deliberative, and egalitarian) from V-dem data (Coppedge et al. 2021, Lindberg et al. 2014).

The second reason some countries adopt FOI laws is they are more developed economically. The level of economic development is highly correlated with the level of democracy and is thus correlated with the adoption of FOI laws. Economically developed countries have fewer protests because citizens are more economically satisfied with the government.⁵⁸ To control for economic development, in some specifications and as robustness checks, I include GDP per capita as newly estimated by Fariss et al. (2022).

3.5 Results

Table 3.2 shows the results of the effect of adopting FOI laws on the number of protests. The adoption of FOI laws increases the number of protests, supporting the main hypothesis. Column (1) shows the adoption of FOI law increases 93.863 protests on average, and the estimate is statistically significant at 0.01 level. Column (2) shows the result is robust to the inclusion of electoral, liberal, participatory, deliberative, and egalitarian democracy indices from V-dem. After controlling for the five democracy indices, the adoption of FOI laws increases 101.308 protests on average, and the estimate is still statistically significant at 0.01 level. Surprisingly, none of the democracy indices has statistically significant effects on the number of protests, suggesting there is no correlation between protests and electoral democracy.⁵⁹ Column (3) shows the result is robust to the inclusion of the five democracy indices from V-dem and GDP per capita. After controlling for the five democracy indices, the adoption of FOI law increases 100.455 protests on average, and the estimate is still statistically significant at 0.01 level. GDP per capita also has no statistically significant effects on the number of protests, suggesting there is no correlation between protests and economic development. Overall, I found support that the adoption of FOI law increase protests. The adoption of FOI law is associated with 100 more protests per country-year.

⁵⁸Related to economic development, McClean (2010) argues that in coordinated market economies, trade associations negotiates with the government so they have privileged access to information about the government. FOI law provide public access to official files, thus threatens the privileged access of trade associations. So more highly coordinated country's economy, the less likely the country will have FOI law. While coordinated economy is related to protests because associations could better solve the collective action problem, I could not find cross-country data that measures coordinated economy.

⁵⁹I conducted regressions to investigate the correlation between electoral democracy and FOI law and the correlation between electoral democracy and protests. The results shows electoral democracy is positively associated with FOI law, as expected. However, electoral democracy is not associated with protests. Please see the appendix for full results.

Table 3.2: Main Results

	(1)	(2)	(3)
FOI law	93.863*** (35.807)	101.308** (41.995)	100.455** (42.200)
Electoral democracy index		981.425 (677.931)	991.449 (665.095)
Liberal democracy index		1,762.453 (1,158.990)	1,885.230 (1,267.334)
Participatory democracy index		-846.662 (796.763)	-825.489 (796.948)
Deliberative democracy index		-835.190 (743.977)	-867.082 (759.966)
Egalitarian democracy index		-2,309.957 (1,493.813)	-2,503.586 (1,615.424)
GDP per capita			0.001 (0.001)
State FE	Y	Y	Y
Year FE	Y	Y	Y
State	202	173	161
N	4444	3780	3535

* Note: P-values are two-tailed. Robust standard errors are clustered by country. *p<0.1; **p<0.05; ***p<0.01.

3.5.1 Placebo Test

In this section, I conduct an additional test to examine the parallel-trends assumption on which difference-in-difference estimation depends: whether the treatment group, which includes countries that adopted FOI laws, and the control group, which includes countries that adopted FOI laws, have similar trends in protests before the adoption of FOI laws. To investigate the extent to which the data meet the parallel-trends assumption, I include hypothetical leads to the previous models in the placebo test, i.e., hypothetical FOI law adoption

in each of the five years before the actual year of FOI law adoption. Including hypothetical leads allow me to test the parallel-trends assumption formally. If the treatment group and the control group had similar trends in incidents before the FOI laws were adopted, the hypothetical adoption before the actual adoption would have no effect on protests. I include 15 lags to test whether the effect of the FOI law adoption lasts.⁶⁰

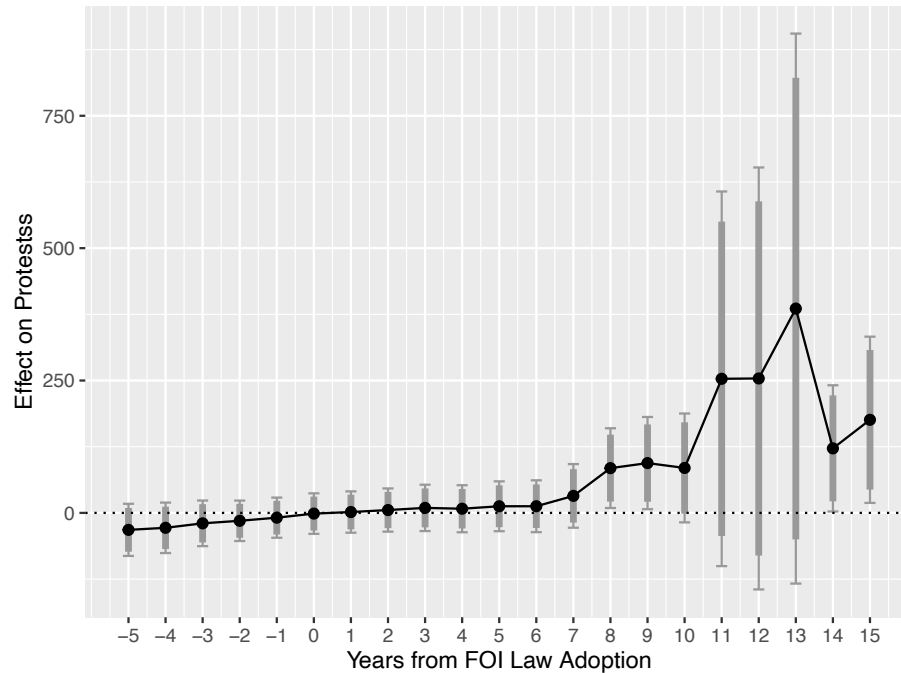
Figure 3.1 shows a graph of the placebo test for protests. The graph plots the coefficients of FOI law adoption ($t = 0$) and its leads and lags. Black dots represent point estimates. The bars are 95 percent confidence intervals. The bold lines are 90 percent confidence intervals. As Figure 3.4 shows, the effects of the hypothetical adoption of FOI laws on protests are not significant in years before the new states' creation, with the point estimates as small negative numbers below 0. This suggests that the protest trends in the treatment group and the control group were not statistically different from one another before the actual adoption of FOI laws.⁶¹

Starting one year after the adoption of an FOI law ($t = 1$), the point estimates show FOI laws start to increase protests. However, the effects are not statistically significant until 8 years after the adoption ($t = 8$). At 9 years after the adoption of an FOI law, the point estimates show that such adoption increases protests by a lot, and the effects reach statistical significance at 14 and 15 years after the adoption. Given that including many lags and leads causes a multi-collinearity problem; that is, the increase of uncertainties, the results suggest that creating an FOI law increases protests in both the short and long term.

⁶⁰I chose 5 leads because 19 countries that adopted FOI laws on or before 1997, which means they don't have any pre-treatment period in data. There are 20 countries adopted FOI laws between 1997 and 2002 with equal or less than 5 pre-treatment period in data.

⁶¹Table of full results is included in the appendix.

Figure 3.1: Changes in Protests Before and After FOI law



3.5.2 Sensitivity Test

One might think the long-term effects shown in the placebo tests are too good to be true, and the results showing that adopting FOI laws increases protests might be driven by some particular country. I conducted a sensitivity test to show the results are robust to the exclusion of any one country. In the sensitivity test, I deleted one of the 119 countries that adopted FOI laws at a time and re-ran the main model. In total, I conducted 119 main models, with each model excluding one of the 119 countries that adopted FOI laws. The results show that the deletion of any one of the 119 countries that adopted FOI laws does not change the results much.

Table 3.3 shows five selected sensitivity test results that are most different from the main effect in terms of magnitude. As we can see from the table, the deletion of India from the data set reduces the effect from 93.863 to 64.554. India’s RTI Act was utilized by the citizens a lot to reveal government information and led to more protests regarding government officials’ wrongdoing. But the result is still substantively large and statistically significant, suggesting India does not solely drive the results. The same applies to Paraguay. Deleting Belgium, Nigeria, or Tunisia did not change the effect much, suggesting that no one particular country is driving the result.⁶²

⁶²Full sensitivity results of 116 tests are included in the appendix.

Table 3.3: Selected Sensitivity Test Results

FOI Law Adoption Year	Country Deleted	Estimate	Std. Error	T value	P value
2010	India	64.554	21.158	3.051	0.002
2014	Paraguay	75.566	31.184	2.423	0.015
1994	Belgium	90.591	36.178	2.504	0.012
2011	Nigeria	90.716	36.151	2.509	0.012
2011	Tunisia	91.039	36.177	2.516	0.012

3.5.3 Generalized Synthetic Control

Potential unobserved time-varying confounders are one of the main concerns with DiD design. To address this concern, Generalized synthetic control(GSC) is utilized as a robustness check here. SC models unobserved time-varying confounders(factors) with an interactive fixed effects (IFE) model and it uses a cross validation scheme that automatically selects the number of factors of the IFE model (Xu 2017). Additionally, it breaks down the treatment effect over time so that I can compare the findings with the placebo test. In a linear factor model, GSC assumes the outcome is explained by the treatment effect, time variant variables and their effects, unobserved common factors (time-varying confounders) times factor loadings (unit-specific intercepts), and stochastic error. It is worth to note that in all specifications, I force the model to include two way fixed effects, which are special cases of unobserved common factors times factor loadings. Since no covariates were included, the model has the function form shown below:

$$Protests_{i,t} = \delta FOI\ laws_{i,t} + \lambda'_i f_t + \epsilon_{i,t}$$

To put it simply, the model first estimates an interactive fixed effects model, obtaining estimates of f_t - a fixed number of latent factors, using only the control group data. These can be obtained using only information from control units because f_t is only time-variant. After f_t is estimated, then factor loading λ'_i can be calculated for the treated units by minimizing the mean squared error of the predicted treated outcome in the pre-treatment periods. Because λ'_i is only unit-specific, we got factor loading for the treatment unit. Then, treated counterfactual can be computed based on the estimated factors f_t and factor loading $\lambda'_i f_t$.

$$Pro\hat{t}ests_{i,t}(0) = \hat{\lambda}'_i \hat{f}_t$$

As a final step, δ can be estimated by taking the difference between observed $Protests_{i,t}(1)$ and estimated $Pro\hat{t}ests_{i,t}(0)$. In sum, we can get the effect of FOI laws on protests using observations of controlled units and the pre-treatment treated unit in the panel data.

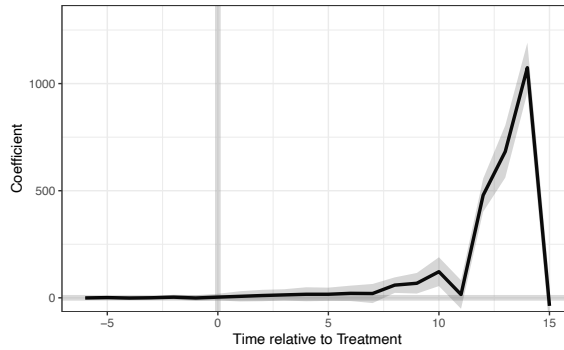
Figure 3.2 shows the results of GSC models. One limitation of GSC is that it requires a certain number of pre-treatment periods to estimate the number of factors. Because not all treated units in the data have pre-treatment units, to use GSC, I have to drop some treated observations. In the first specification, I limit the minimum number of pre-treatment pe-

riods to be 7.⁶³ The model can select the number of factors from 0 to 5 and it selected 0, indicating the inclusion of two way fixed effects is enough. Subfigure (a) shows the results. In pre-treatment periods, the effect of FOI laws on protest is around 0 with no statistical significance, showing that the model did a good job creating the synthetic control unit. 8 years after the treatment, FOI laws started to increase the number of protests with the number peaked at year fourteen. The results suggest that FOI laws increase protests in the long term, similar to what the placebo test suggests. In the second and third specifications, the minimum number of pre-treatment periods is 5 and 3, respectively. The models can select the number of factors from 0 to 3 and 0 to 1, respectively, and both of them selected 0. Subfigure (b) shows the results of the second specification and Subfigure (c) shows the results of the third specification. Both of them show similar trends as the first specification, with small magnitudes in the effects. The results suggest that FOI laws increase protest in the long term.

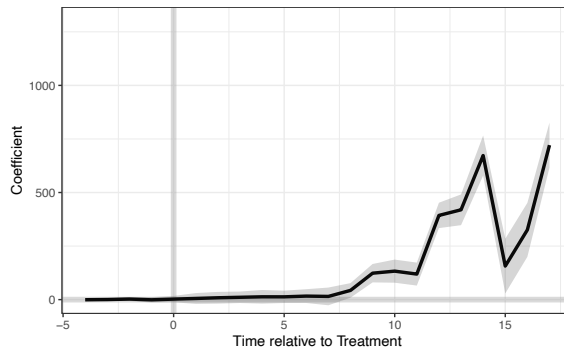
⁶³The details of dropped treated units can be found in the appendix.

Figure 3.2: GSC Results

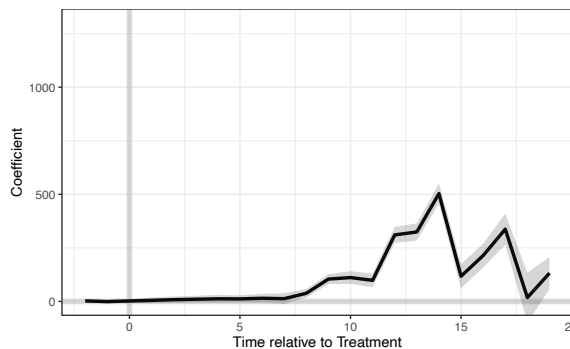
(a) Minimum value of pre-treatment periods: 7;
The number of factors: 0 selected from 0 to 5



(b) Minimum value of pre-treatment periods: 5;
The number of factors: 0 selected from 0 to 3



(c) Minimum value of pre-treatment periods: 3;
The number of factors: 0 selected from 0 to 1



3.5.4 Conditional Effects

Table 3.4 shows the effect of adopting FOI laws on the number of protests conditioned on the electoral democracy index. The adoption of FOI law increases the number of protests when the electoral democracy index is low, and it does not increase the number of protests when the electoral democracy index is high, supporting H2 and H3. Column (1) shows the adoption of FOI law increases 231.712 protests on average when the electoral democracy index is 0, and the estimate is statistically significant at 0.01 level. When the electoral democracy index is 1, the point estimate is 2.69 (231.712 - 229.022), almost no effect. Column (2) shows the result is robust to the inclusion of electoral, liberal, participatory, deliberative, and egalitarian democracy indices from V-dem. When the electoral democracy index is 0, the adoption of FOI law increases 200.421 protests. When the electoral democracy index is 1, the point estimate is the adoption of FOI law increases 22.093 protests. Column (3) shows the result is robust to the inclusion of the five democracy indices from V-dem and GDP per capita. When the electoral democracy index is 0, the adoption of FOI law increases 205.738 protests. When the electoral democracy index is 1, the point estimate is the adoption of FOI law increases 15.115 protests.

Table 3.4: The Effect of FOI Law Conditioned on Electoral Democracy Index Results

	(1)	(2)	(3)
FOI law	231.712** (93.166)	200.421*** (73.909)	205.738*** (75.217)
Electoral democracy index	-131.276 (284.870)	928.862 (664.907)	950.367 (654.464)
Liberal democracy index		1,800.360 (1,164.378)	1,921.296 (1,271.934)
Participatory democracy index		-832.092 (794.511)	-807.614 (793.647)
Deliberative democracy index		-843.822 (742.549)	-880.010 (758.815)
Egalitarian democracy index		-2,197.435 (1,471.020)	-2,389.451 (1,590.990)
GDP per capita			0.001 (0.001)
FOI law:Electoral democracy index	-229.022** (93.512)	-178.328** (70.624)	-190.623** (74.116)
State FE	Y	Y	Y
Year FE	Y	Y	Y
State	202	173	161
N	4444	3780	3535

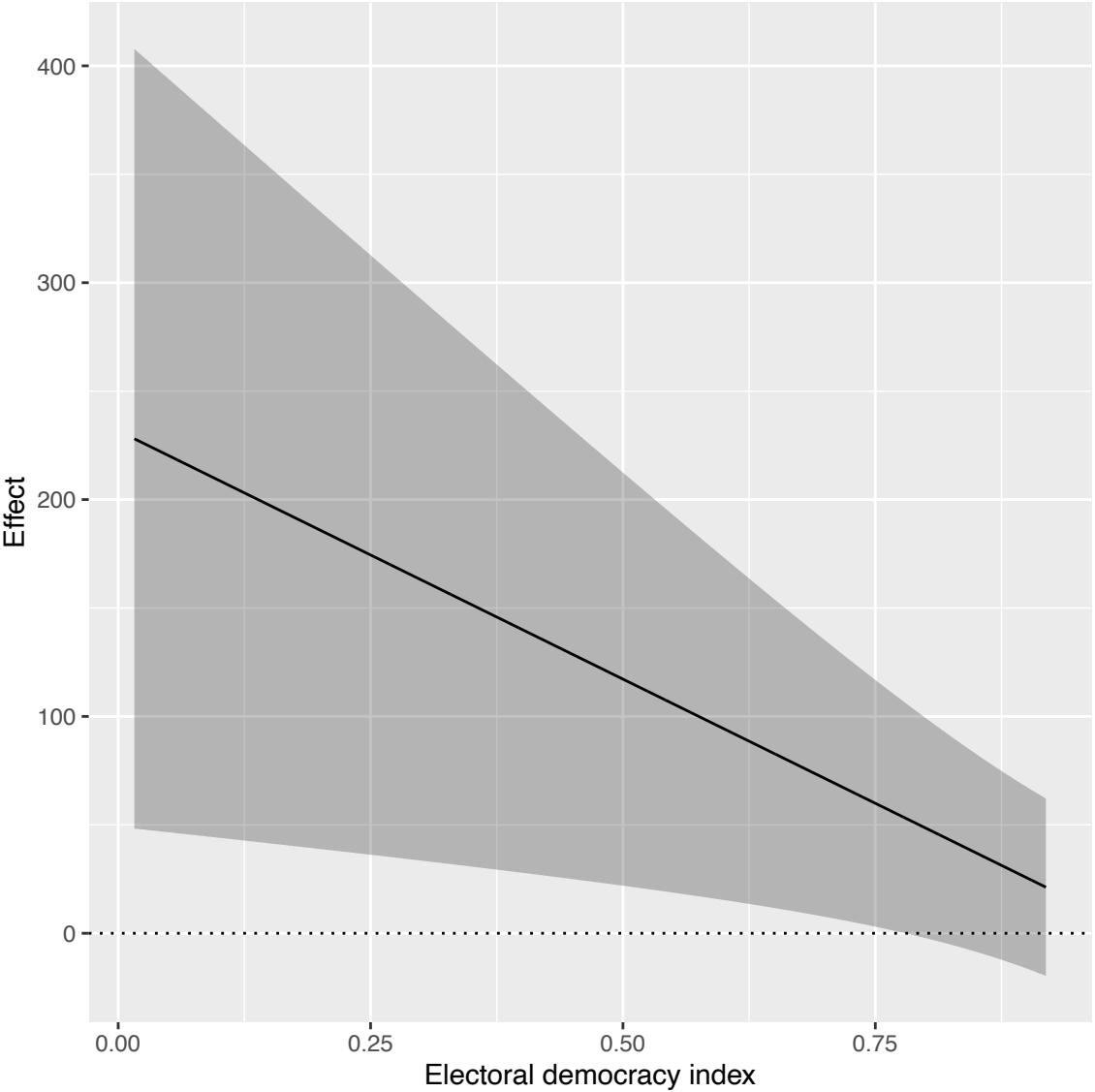
* Note: P-values are two-tailed. Robust standard errors are clustered by country. *p<0.1; **p<0.05; ***p<0.01.

Because the electoral democracy index is a continuous variable and it is hard to see how the standard error varies when the value of the electoral democracy index changes, to better interpret the interaction effect, I plot the effect of FOI law adoption on the number of protests conditioned on the electoral democracy index.⁶⁴ Figure 3.3 shows the conditional effect

⁶⁴I also explored conditional effect of GDP per capita. Please see the appendix for the graph.

with a 95% confidence interval. The plot shows that when the electoral democracy index is low or medium, FOI law adoption increases the number of protests. When the electoral democracy index is high, FOI law adoption does not increase the number of protests. When the electoral democracy index is below 0.78, the point estimate is positive and statistically different from 0. When the electoral democracy index is above 0.78, FOI law adoption no longer has a statistically significant effect on protest. One caveat here is that the CI is large. The number of protests when the electoral democracy index is 0 might not be statistically different from the number of protests when the electoral democracy index is 1, making the results less conclusive. The large CI at the lower end of the electoral democracy index is partially driven by a smaller number of observations around 0. Looking at the lower bound of the CI, although CI is large, the lower bound still shows the effect decreases as the electoral democracy index increases, suggesting a conditional effect. Overall, the graph provides some support for H2 and H3.

Figure 3.3: The Effect of FOI Law Adoption Conditioned on Election Democracy Index



3.6 Conclusion

This paper found that adopting FOI law increases the number of protests rather than reducing them. The results are counter-intuitive because, conventionally, we think more transparency of the government makes citizens happier. It happens because in most settings, when the requested information is related to serious wrongdoings of an official, the government may not be able to punish the official through legal means, and the citizens cannot punish the official through elections. When the citizens find out that the official has not been punished and they have no means to punish the official, the information about wrongdoing serves as a focal point for citizens to mobilize collectively. Using a difference-in-difference

design, I find that adopting FOI law is associated with statistically and substantively significant increases in the number of protests using cross-country data. More importantly, I found the relationship between the adoption of an FOI law and the number of protests is conditioned by electoral democracy. When there are ways to punish those officials, FOI laws do not lead to more protests.

The paper makes two contributions to the literature about the flow of government information, corruption, and protests. First, it is the first paper that examines the relationship between FOI law and protests. Although information about the government has theoretically been essential for mobilizing citizens in collective actions such as protests, no studies focused on FOI law and protests. This paper took an initial step to build a theory about FOI law and protest and tested it empirically. Second, this paper highlights the importance of the difference between the detection and punishment of corruption. FOI law might be helpful in detecting corruption, but when there is only detection of corruption without punishment for it in the system, citizens will not be satisfied and will do something outside the system. FOI law cannot work independently without other means of enforcement to curb corruption.

Building on this paper, there are three directions future research might consider working on. First, it might be worth testing the causal chain mentioned in the theoretical framework. Although the data might be hard to collect, one might test the effect of FOI laws on not only the detection of corruption but also the punishment of corruption and then test the effect of punishment for corruption on protests to fully test the theory. Second, measuring the de jure and de facto power of FOI by country and over time might be worthwhile. In this paper, FOI is coded as a binary variable that has only two values, existing or not. There were efforts to better measure FOI. For example, Banisar (2006) did a global survey of access to government information laws. Lidberg (2009) created a freedom of information index for five countries and suggested that the FOI Index should be expanded to all countries that adopted FOI laws. So far, no study or data sources have done that. One might continue on this path and create an index that covers all countries that adopted FOI laws over time. With this data available, more scholars can use this to help us better understand FOI laws, their causes, and their consequences. Last but not least, scholars might study other conditionalities. Theoretically, the effect of FOI laws on protests is also changed by other socioeconomic conditions, such as the condition of free media, economics, or civil society. Studies of conditionality will help us better understand under what conditions FOI laws work better.

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3.7 Appendix

Table 3.5: Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Number of Protests	4,444	29.799	337.557	0	0	0	0	12,275
FOI law	4,444	0.354	0.478	0	0	0	1	1
Electoral democracy index	3,791	0.522	0.265	0.016	0.282	0.521	0.773	0.926
Liberal democracy index	3,783	0.409	0.271	0.005	0.154	0.376	0.657	0.896
Participatory democracy index	3,786	0.337	0.207	0.009	0.150	0.319	0.502	0.805
Deliberative democracy index	3,791	0.416	0.259	0.007	0.195	0.375	0.648	0.889
Egalitarian democracy index	3,791	0.399	0.244	0.029	0.193	0.331	0.614	0.880
GDP per capita	3,791	14.788	16.287	0.429	2.954	8.533	21.456	100.865
Log of Number of Protests	4,444	0.620	1.428	0.000	0.000	0.000	0.000	9.415

Table 3.6: The effect of Electoral Democracy Index on Protests

<i>Dependent variable: Protests</i>	
Electoral democracy index	-149.830 (317.349)
State FE	Y
Year FE	Y
State	202
N	4444
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01

Table 3.7: The effect of Electoral Democracy Index on FOI law

<i>Dependent variable: FOI law</i>	
Electoral democracy index	0.572*** (0.221)
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01

Table 3.8: The effect of FOI Law Conditioned on Electoral Democracy Index Results:
Logged DV

	(1)	(2)	(3)
FOI law	0.909*** (0.143)	106.057** (44.955)	100.000** (42.239)
Electoral democracy index		1,137.002 (768.413)	1,131.156 (766.443)
Liberal democracy index		1,600.157 (1,115.260)	1,612.773 (1,120.519)
Participatory democracy index		-685.610 (712.832)	-697.179 (718.482)
Deliberative democracy index		-841.052 (787.116)	-840.910 (786.873)
Egalitarian democracy index		-2,540.014 (1,662.382)	-2,538.949 (1,663.180)
GDP per capita			1.768 (1.344)
State FE	Y	Y	Y
Year FE	Y	Y	Y
State	202	173	161
N	4444	3780	3535

* *Note:* P-values are two-tailed. Robust standard errors are clustered by country. *p<0.1; **p<0.05; ***p<0.01.

Table 3.9: The effect of FOI Law Conditioned on Electoral Democracy Index Results:
Negative Binomial

	(1)	(2)	(3)
FOI law	0.632** (0.295)	42.487* (23.376)	0.462 (0.366)
Electoral democracy index		951.687 (748.927)	16.441*** (6.097)
Liberal democracy index		1,593.868 (1,139.596)	9.630 (6.902)
Participatory democracy index		-874.876 (807.157)	-8.150 (5.458)
Deliberative democracy index		-654.092 (746.199)	-4.311 (4.600)
Egalitarian democracy index		-2,373.985 (1,687.357)	-28.857*** (8.948)
GDP per capita			0.275** (0.125)
State FE	Y	Y	Y
Year FE	Y	Y	Y
State	202	173	161
N	4444	3780	3535

* *Note:* P-values are two-tailed. Robust standard errors are clustered by country. *p<0.1; **p<0.05; ***p<0.01.

Table 3.10: The effect of FOI Law Conditioned on Electoral Democracy Index Results: Poisson and Quasi-Poisson

	(1)	(2)	(3)
FOI law	0.078 (0.410)	0.570 (0.387)	0.462 (0.366)
Electoral democracy index		16.372*** (6.007)	16.441*** (6.097)
Liberal democracy index		8.179 (6.460)	9.630 (6.902)
Participatory democracy index		-8.080 (5.452)	-8.150 (5.458)
Deliberative democracy index		-4.964 (4.752)	-4.311 (4.600)
Egalitarian democracy index		-26.632*** (8.794)	-28.857*** (8.948)
GDP per capita			0.275** (0.125)
Constant	-0.464 (0.442)	0.040 (0.872)	0.278 (0.921)
State FE	Y	Y	Y
Year FE	Y	Y	Y
State	202	173	161
N	4444	3780	3535

* *Note:* P-values are two-tailed. Robust standard errors are clustered by country. *p<0.1; **p<0.05; ***p<0.01.

Table 3.11: Placebo Test Results

<i>Dependent variable:</i>		
	(1)	(2)
t5-	-32.051 (25.126)	-25.011 (25.126)
t-4	-28.186 (24.314)	-17.861 (24.314)
t-3	-19.710 (21.984)	-4.678 (21.984)
t-2	-14.917 (19.498)	0.118 (19.498)
t-1	-9.036 (19.336)	4.992 (19.336)
t	-1.326 (19.518)	15.986 (19.518)
t+1	1.613 (19.906)	16.578 (19.906)
t+2	5.392 (20.824)	16.302 (20.824)
t+3	9.458 (22.332)	21.359 (22.332)
t+4	7.869 (22.658)	24.360 (22.658)
t+5	12.517 (24.022)	29.300 (24.022)
t+6	12.576 (24.985)	23.755 (24.985)
t+7	32.116 (30.653)	35.520 (30.653)
t+8	84.537** (38.425)	96.598** (38.425)
t+9	94.064** (44.426)	106.815** (44.426)
t+10	84.882 (52.449)	96.993* (52.449)
t+11	253.277 (180.503)	264.261 (180.503)
t+12	253.899 (203.315)	261.470 (203.315)
t+13	385.886 (264.998)	388.493 (264.998)
t+14	121.833** (60.815)	120.306** (60.815)
t+15	175.833** (80.095)	171.826** (80.095)
State FE	Y	Y
Year FE	Y	Y
State	202	161
6 Controls	N	Y
N	4444	3535

* Note: P-values are two-tailed. *p<0.1;
p<0.05; *p<0.01.

Figure 3.4: Changes in Protests Before and After FOI law with Controls

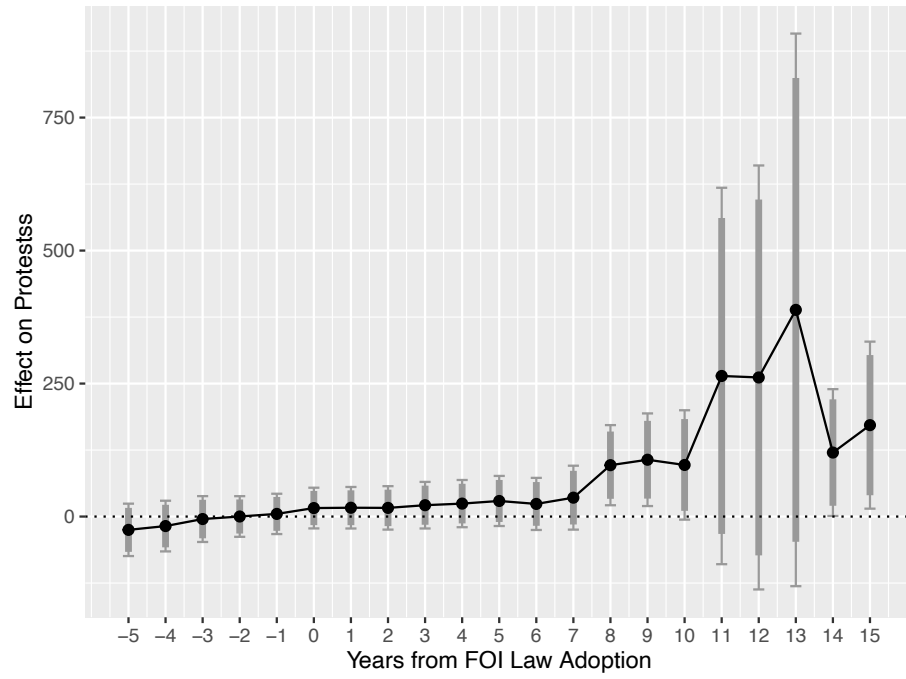


Table 3.12: Sensitivity Test Results Part 1

FOI Law Adoption Year	Country Deleted	Estimate	Std. Error	T value	P value
2010	IDN	64.554	21.158	3.051	0.002
2014	PRY	75.566	31.184	2.423	0.015
1994	BEL	90.591	36.178	2.504	0.012
2011	NGA	90.716	36.151	2.509	0.012
2011	TUN	91.039	36.177	2.516	0.012
2014	PLW	92.147	36.281	2.540	0.011
2018	LUX	92.180	36.012	2.560	0.011
2003	TUR	92.721	36.199	2.561	0.010
2005	IND	92.759	36.291	2.556	0.011
2006	MKD	92.946	36.274	2.562	0.010
2001	POL	92.965	36.039	2.580	0.010
2000	EST	93.167	36.318	2.565	0.010
2012	YEM	93.685	36.254	2.584	0.010
2016	KEN	93.809	36.048	2.602	0.009
1998	LVA	93.842	35.976	2.608	0.009
1766	SWE	93.863	35.807	2.621	0.009
1966	USA	93.863	35.807	2.621	0.009
1978	FRA	93.863	35.807	2.621	0.009
1982	AUS	93.863	35.807	2.621	0.009
1982	NZL	93.863	35.807	2.621	0.009
1985	COL	93.863	35.807	2.621	0.009
1987	AUT	93.863	35.807	2.621	0.009
1987	DNK	93.863	35.807	2.621	0.009
1991	NLD	93.863	35.807	2.621	0.009
1994	BLZ	93.863	35.807	2.621	0.009
1996	KOR	93.863	35.807	2.621	0.009
1997	IRL	93.863	35.807	2.621	0.009
1998	ISR	93.863	35.807	2.621	0.009
1999	CZE	93.863	35.807	2.621	0.009
1999	FIN	93.863	35.807	2.621	0.009
1999	GRC	93.863	35.807	2.621	0.009
1999	LIE	93.863	35.807	2.621	0.009
1999	TTO	93.863	35.807	2.621	0.009
2000	BIH	93.863	35.807	2.621	0.009
2000	BGR	93.863	35.807	2.621	0.009
2000	SVK	93.863	35.807	2.621	0.009
2000	ZAF	93.863	35.807	2.621	0.009
2000	GBR	93.863	35.807	2.621	0.009
2002	AGO	93.863	35.807	2.621	0.009
2002	PAK	93.863	35.807	2.621	0.009
2002	PAN	93.863	35.807	2.621	0.009
2002	TJK	93.863	35.807	2.621	0.009
2002	ZWE	93.863	35.807	2.621	0.009
2003	ARM	93.863	35.807	2.621	0.009
2003	XKX	93.863	35.807	2.621	0.009
2003	SRB	93.863	35.807	2.621	0.009
2004	DOM	93.863	35.807	2.621	0.009
2004	CHE	93.863	35.807	2.621	0.009
2005	DEU	93.863	35.807	2.621	0.009
2005	MNE	93.863	35.807	2.621	0.009
2005	TWN	93.863	35.807	2.621	0.009
2007	CHN	93.863	35.807	2.621	0.009
2007	KGZ	93.863	35.807	2.621	0.009
2007	NPL	93.863	35.807	2.621	0.009
2007	NIC	93.863	35.807	2.621	0.009
2008	BGD	93.863	35.807	2.621	0.009
2008	CHL	93.863	35.807	2.621	0.009
2008	GTM	93.863	35.807	2.621	0.009
2009	COK	93.863	35.807	2.621	0.009

Table 3.13: Sensitivity Test Results Part 2

FOI Law Adoption Year	Country Deleted	Estimate	Std. Error	T value	P value
2009	IRN	93.863	35.807	2.621	0.009
2011	BRA	93.863	35.807	2.621	0.009
2011	SLV	93.863	35.807	2.621	0.009
2011	HUN	93.863	35.807	2.621	0.009
2011	MLT	93.863	35.807	2.621	0.009
2011	MNG	93.863	35.807	2.621	0.009
2013	RWA	93.863	35.807	2.621	0.009
2013	CIV	93.863	35.807	2.621	0.009
2014	MOZ	93.863	35.807	2.621	0.009
2015	BFA	93.863	35.807	2.621	0.009
2015	SDN	93.863	35.807	2.621	0.009
2016	TGO	93.863	35.807	2.621	0.009
2016	VNM	93.863	35.807	2.621	0.009
2016	MWI	93.863	35.807	2.621	0.009
2017	LBN	93.863	35.807	2.621	0.009
2018	CYP	93.863	35.807	2.621	0.009
2003	HRV	93.875	35.895	2.615	0.009
1990	ITA	94.051	35.895	2.620	0.009
2014	MDV	94.096	35.894	2.621	0.009
2014	AFG	94.114	36.167	2.602	0.009
1996	LTU	94.307	35.972	2.622	0.009
2015	KAZ	94.307	35.972	2.622	0.009
2008	URY	94.307	36.045	2.616	0.009
2016	VUT	94.400	36.043	2.619	0.009
2005	UGA	94.496	36.042	2.622	0.009
1997	UZB	94.497	36.042	2.622	0.009
1999	GEO	94.497	36.042	2.622	0.009
2013	ESP	94.497	36.042	2.622	0.009
2007	JOR	94.599	36.104	2.620	0.009
2011	UKR	94.651	36.275	2.609	0.009
2005	AZE	94.684	36.205	2.615	0.009
2003	SVN	94.794	36.202	2.618	0.009
1997	THA	94.806	36.154	2.622	0.009
2002	JAM	94.811	36.154	2.622	0.009
2002	MEX	94.811	36.154	2.622	0.009
2003	PER	94.811	36.154	2.622	0.009
2007	PRT	94.811	36.154	2.622	0.009
2009	RUS	94.811	36.154	2.622	0.009
2006	HND	94.818	36.201	2.619	0.009
2013	SLE	94.840	36.201	2.620	0.009
2016	LKA	94.846	36.200	2.620	0.009
1999	ALB	94.869	36.153	2.624	0.009
2003	VCT	94.887	36.199	2.621	0.009
2010	LBR	94.891	36.290	2.615	0.009
2013	SSD	94.935	36.198	2.623	0.009
2016	PHL	94.935	36.198	2.623	0.009
2016	TZA	94.935	36.198	2.623	0.009
2008	ETH	94.935	36.198	2.623	0.009
2013	GUY	94.935	36.198	2.623	0.009
1999	JPN	95.024	36.298	2.618	0.009
1983	CAN	95.037	36.233	2.623	0.009
2004	ATG	95.037	36.233	2.623	0.009
2004	ARG	95.037	36.233	2.623	0.009
2004	ECU	95.037	36.233	2.623	0.009
2011	NER	95.049	36.263	2.621	0.009
2000	MDA	95.089	36.283	2.621	0.009
1996	ISL	95.116	36.261	2.623	0.009
2001	ROU	95.207	36.292	2.623	0.009
2006	NOR	95.207	36.292	2.623	0.009
2010	GIN	95.218	36.296	2.623	0.009

Table 3.14: List of Dropped Treated Countries in GSC Specifications

Spefication	Total Number	List
1	51	AGO ALB ARM AUS AUT BEL BGR BIH BLZ CAN COL CZE DNK EST FIN FRA GBR GEO GRC HRV IRL ISL ISR ITA JAM JPN KOR LIE LTU LVA MDA MEX NLD NZL PAK PAN PER POL ROU SRB SVK SVN SWE THA TJK TTO TUR USA UZB VCT ZAF ZWE
2	38	ALB AUS AUT BEL BGR BIH BLZ CAN COL CZE DNK EST FIN FRA GBR GEO GRC IRL ISL ISR ITA JPN KOR LIE LTU LVA MDA NLD NZL POL ROU SVK SWE THA TTO USA UZB ZAF
3	29	ALB AUS AUT BEL BLZ CAN COL CZE DNK FIN FRA GEO GRC IRL ISL ISR ITA JPN KOR LIE LTU LVA NLD NZL SWE THA TTO USA UZB

Table 3.15: Main Results National

	(1)	(2)	(3)
FOI law	56.075*** (19.654)	60.439*** (23.425)	60.874*** (23.555)
Electoral democracy index		577.402 (363.103)	583.529* (353.140)
Liberal democracy index		860.930 (620.152)	929.277 (678.489)
Participatory democracy index		-402.626 (441.090)	-390.245 (441.698)
Deliberative democracy index		-464.335 (407.278)	-482.100 (416.621)
Egalitarian democracy index		-1,229.740 (777.788)	-1,334.069 (841.557)
GDP per capita			0.0003 (0.0003)
State FE	Y	Y	Y
Year FE	Y	Y	Y
State	202	173	161
N	4444	3780	3535

* *Note:* P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1; **p<0.05; ***p<0.01.

Table 3.16: The Effect of FOI Law conditioned on Election Index Results National

	(1)	(2)	(3)
FOI law	130.707** (52.348)	113.407*** (42.815)	117.371*** (43.610)
Electoral democracy index	-70.834 (154.089)	549.312 (356.091)	561.484 (347.420)
Liberal democracy index		881.188 (622.982)	948.631 (680.934)
Participatory democracy index		-394.840 (439.791)	-380.653 (439.788)
Deliberative democracy index		-468.948 (406.372)	-489.038 (415.815)
Egalitarian democracy index		-1,169.607 (765.067)	-1,272.822 (828.032)
GDP per capita			0.0004 (0.0003)
FOI law:Electoral democracy index	-123.393** (54.395)	-95.300** (43.059)	-102.293** (44.929)
State FE	Y	Y	Y
Year FE	Y	Y	Y
State	202	173	161
N	4444	3780	3535

* Note: P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1;
p<0.05; *p<0.01.

Table 3.17: Main Results Non-national

	(1)	(2)	(3)
FOI law	37.789** (16.804)	40.868** (19.182)	39.581** (19.374)
Electoral democracy index		404.023 (322.764)	407.920 (324.749)
Liberal democracy index		901.523 (549.678)	955.953 (601.043)
Participatory democracy index		-444.035 (364.017)	-435.244 (364.066)
Deliberative democracy index		-370.856 (343.178)	-384.982 (350.143)
Egalitarian democracy index		-1,080.216 (719.622)	-1,169.516 (777.566)
GDP per capita			0.001 (0.001)
State FE	Y	Y	Y
Year FE	Y	Y	Y
State	202	173	161
N	4444	3780	3535

* *Note:* P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1; **p<0.05; ***p<0.01.

Table 3.18: The Effect of FOI Law conditioned on Election Index Results Non-National

	(1)	(2)	(3)
FOI law	101.005** (43.050)	87.014** (33.784)	88.367** (34.455)
Electoral democracy index	-60.442 (132.670)	379.550 (316.867)	388.884 (320.047)
Liberal democracy index		919.172* (552.121)	972.665 (603.133)
Participatory democracy index		-437.252 (363.074)	-426.961 (362.692)
Deliberative democracy index		-374.874 (342.701)	-390.973 (349.861)
Egalitarian democracy index		-1,027.827 (709.857)	-1,116.629 (766.948)
GDP per capita			0.001 (0.001)
FOI law:Electoral democracy index	-105.629** (42.819)	-83.027*** (32.209)	-88.330*** (33.879)
State FE	Y	Y	Y
Year FE	Y	Y	Y
State	202	173	161
N	4444	3780	3535

* Note: P-values are two-tailed. Robust standard errors are clustered by district. *p<0.1;
 p<0.05; *p<0.01.

Figure 3.5: The effect of FOI Law conditioned on GDP per capita

