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Bargaining over Nature:
Formal and Causal Analyses on Environments and Conflict

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Bargaining over Nature:
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Abstract

Bargaining over Nature:
Formal and Causal Analyses on Climate and Conflict

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Despite the growing attention to environmental changes and their consequences on conflict, we still do not know the roles of human responses and strategic interactions. This dissertation is composed of three essays that address this void of knowledge. The central argument is that natural environments do not only directly affect conflict, but their effects are intermediated by human responses, political institutions, and strategic opportunities. In each essay, I elaborate this argument by using formal models, causal inference methods, and geospatial data. The analyses indicate that natural environments do not automatically cause or inhibit conflict, but human's actions can critically shape the relationship.

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Chapter 1. Introduction

With growing attention to environmental changes, an increasing number of studies have examined the relationship between natural environments and conflicts. Summarizing 60 studies about climate changes and human conflicts across diverse fields, Hsiang et al. (2013; 2011) conclude that there exists "strong causal evidence linking climatic events to human conflict" (2013, 1213). Despite the plethora of studies, however, extant studies focus on how natural environments affect conflict, dismissing *human responses and strategic interactions that can shape the causal relationship*. Since our behaviors are not completely determined by natural environments, the paths from natural environments to conflict are conditional on our strategic choices. Three essays that constitute my dissertation are attempts to examine the roles of strategic actions in the relationship between natural environments and conflict.

The central argument in the dissertation is that natural environments do not only directly affect conflict, but their effects are intermediated by human responses, political institutions, and strategic opportunities. For instance, from a perspective of bargaining theories, a large shift in natural environments, such as natural disasters, might shift the power balance between the stakeholders, which in turn could make the commitment to peaceful conflict resolution difficult and hence result in violent conflicts. A closer look at the commitment problem, however, indicates that the real cause of armed conflicts is not the environmental shift per se but the anticipated recovery from the shift. Because natural disasters are unpredictable ex ante, parties cannot foresee the disaster-driven power shift a priori. By contrast, parties can see the future of post-disaster reconstruction. If one party is more benefited from post-disaster reconstruction than the other parties, the disadvantaged parties can optimally initiate violent conflicts to prevent such an unfavorable power shift. Thus, from the viewpoint of commitment problems, the real cause of violence lies in not natural disasters itself but in human responses to natural disasters. The first essay examines the causal effect of post-disaster reconstruction on violent conflicts, taking the case of the 2004 Tsunami in Sri Lanka.

Although the commitment problems can explain the effect of large shifts in environments, they are less insightful for explaining the consequences of much smaller changes in environments. Although such a small shift may not influence bargaining in civil war, more institutionalized politics can be sensitive to such a small change. In the second essay, I explore this possibility by extending private-information models to explain how rainfall deviation can effect violence risks via its impacts on voter turnout. Although free and fair elections are supposed to provide reliable information about the support bases of an incumbent and oppositions and hence to lessen the risks of strategic miscalculation and inefficient violence, abnormal rainfall can affect voter turnout and hence the learning processes. In particular, my formal model indicates that, rather counterintuitively, *high* turnout "drowns-out" the voices of dissidents, creates uncertainties about the size and intensity of social discontents, and thus raises the risk of protests. Thus, it is possible that even a small shift in natural environments can cause conflicts through its effect on electoral politics. The second essay examines this possibility in the case of the Indian State Assembly elections.

While so far I focus on a single outcome of conflict in each of these two essays, in reality, actors have choices over multiple actions. This raises a possibility that natural environments may shape their strategic opportunities—the availability of alternative choices—which in turn can affect their conflict behaviors. For instance, rainfall may directly affect the costs and benefits of the use of violence during civil war; it can hinder military deployment, force rebel groups to march on muddy ground, and lower the moral of the armies. However, rainy weather often coincides with windy ocean conditions, which may make it difficult for the rebels to conduct maritime piracy, limit their strategic opportunities, and hence leave no choices other than resorting to violence. If this is the case, not only do weather conditions directly affect the use of violence, but also they can shape rebels' strategic opportunities and thus indirectly affect violence. In the third essay, I examine these direct and indirect effects of weather conditions on the use of violence and maritime piracy during civil war.

The focuses on the intervening factors, however, pose new empirical challenges. With the intervening factors, we can no longer simply rely on the exogeneity of natural environments in order to make a causal identification. Isolating the effects of a natural disaster or weather conditions themselves from their indirect effect through post-disaster reconstruction, elections, and strategic opportunities is not as straightforward as the estimation of the overall effects. The quality of data about natural environments also raises another challenge; while previous studies tend to rely on government reports on the tsunami and rainfall, they are subject to nonresponses and misreports.

The three essays are a collection of my efforts to address these empirical challenges as well. In these essays, I develop a series of causal identification strategies, including instrumental variable designs, in order to address the problems relating to the intervening factors. Using the environmental variables, including off-shore wave height and election-day rainfall deviation, as predictors for the corresponding explanatory variables, I can isolate the effects of the intervening factors from the endogenous relationship. The instrumental variable approach, for instance, can account for the fact that stakeholders are less likely to allocate reconstruction materials to the locations of higher risks of violent conflicts, and hence allow me to identify the causal effect of post-disaster reconstruction on violent conflicts. In all of the three essays, I also address the potential measurement problems by using satellite-based data sources, including those about tsunami wave heights, rainfall deviations, and ocean wind speed.

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Chapter 2. Post-disaster Reconstruction as a Cause of Intrastate Violence[1]

On December 26th 2004, the third largest earthquake since 1900 hit Sumatra Island and propelled a massive tsunami across the Bay of Bengal to Sri Lanka, a country undergoing a fragile peace process after a twenty-year separatist war (Bandarage 2009). The tsunami killed more than 35,000 people, and destroyed over 78,000 homes, leaving 800,000 people without shelter in the country. The tsunami wave was followed by another wave: disaster relief. International organizations disbursed more than one billion dollars through at least 710 projects (RADA 2006). The government of Sri Lanka, moreover, spent over 200 million dollars on recovery (RADA 2005). By May 2006, at least 40,000 houses had been constructed. These efforts, however, did not successfully build peace nationwide. Sinhalese nationalists opposed the post-disaster agreement with the Tamil Tigers. The rebels also resumed suicide terrorist attacks. Finally, in February 2006, the government launched a full-scale military offensive and the country officially descended into another three years of civil war.

Is it destruction or reconstruction that really drives intrastate violence? If post-disaster reconstruction would be irrelevant, destruction would be the primary cause of war and peace. If reconstruction does matter, however, we risk incorrectly attributing the causes of war to destruction and its antecedent conditions such as physical forces and mitigative infrastructure, and thereby understating the role of human actions after a natural disaster. Thus, depending on whether destruction, reconstruction, or both are primary drivers of intrastate violence, we will have

[1] This chapter is based on a published article: Kikuta, Kyosuke. 2019. "Postdisaster Reconstruction as a Cause of Intrastate Violence: An Instrumental Variable Analysis with Application to the 2004 Tsunami in Sri Lanka." *Journal of Conflict Resolution* 63 (3): 760–85.

markedly different interpretations with their own sets of practical implications about the effect of a natural disaster on conflicts. In this paper, I contend that reconstruction is a crucial explanatory variable in the relationship between a natural disaster and intrastate violence. I also address the endogeneity problem by using the wave heights in the 2004 Tsunami in Sri Lanka as an instrumental variable.

With increasing attention on the political effects of environmental change, a growing number of scholars have analyzed the relationship between natural disasters and intrastate violence (Gleditsch 2012). Surprisingly, however, no studies of which I am aware have incorporated the role of post-disaster reconstruction in the causal relationship. This void in the literature requires special attention, as humans are not only affected by natural disasters but can also respond to them. Indeed, in the wake of natural disasters, the international community pours relief assistance into disaster zones. Thus, without considering post-disaster reconstruction, we can only partially understand the disaster-violence nexus. This omission may provide an explanation for the unstable and even contradictory findings in previous studies.

To address this problem, I extend bargaining theories of war to explain how destruction and reconstruction change the strategic landscape and consequently affect patterns of violent conflicts. Theory implies that destruction does not preclude the possibility of peaceful settlement and thus may not be the primary driver of violence. Instead, post-disaster reconstruction causes a strategic predicament between parties because it provides parties with opportunities to divert resources to military use and to expand sources of taxes and manpower. These opportunities can enhance the bargaining position of a warring party, while missing these opportunities relegates it to an inferior bargaining position. The strategic environment incentivizes parties to use violence with the aim of controlling the locations of reconstruction programs. Thus, I hypothesize that reconstruction has the effect of increasing the number of violent events, while, in contrast to previous studies, the effect of destruction is only weakly identified as a coarse proxy of reconstruction.

This hypothesis, however, raises a new empirical challenge; unlike physical hazards, reconstruction processes are endogenous to violence. The expectation of future violence can hinder reconstruction efforts in disaster zones. Even worse, the fear of violence makes data collection unfeasible, resulting in underreporting of destruction and reconstruction. Thus, naïve estimators may suffer substantial bias about the effects of reconstruction on violent events.

To overcome these difficulties, I apply an instrumental variable approach to the case of Sri Lanka before and after the 2004 Indian Ocean Tsunami. The fact that Sri Lanka experienced a nationwide disaster between two civil wars, the Eelam III and IV Wars, provides a unique opportunity for a before-and-after comparison. Drawing on the exogeneity of wave heights, and using it as an instrument, I separately estimate the effects of the number of destroyed houses and the difference in the number of constructed houses on the difference of violent events before and after the tsunami. Consistent with expectations, an increase in housing construction raises the number of violent events, while the number of destroyed houses has no discernible impact on violence. The finding survives extensive robustness checks, as summarized at the end of this paper.

These findings remind us of a crucial aspect of human responses; reconstruction is manipulable and therefore highly contingent on the decisions of policy makers. The analysis in this paper suggests that a natural disaster does not automatically determine the future of intrastate violence and that we can potentially alter the trajectories through post-disaster responses. This point is of particular importance because it is not uncommon for natural disasters to occur before or during civil wars, as seen in the 1970 Bhola Cyclone in East Pakistan, 1972 Nicaragua

Earthquake, 1976 Guatemala Earthquake, 1997 Somali Flood, 2002 Hindu Kush Earthquake, 2008 Cyclone Nargis in Myanmar, and 2012 and 2015 avalanches in Afghanistan.

DESTRUCTION, SCARCITY, AND INTRASTATE VIOLENCE

There is a growing number of studies on the security implications of environmental change (Gleditsch 2012; Hsiang, Burke and Miguel 2013; Salehyan; 2014). Although several authors have focused on historical trends of climate change and patterns of violence (Tol and Wagner 2009; Zhang et al. 2011), the causal effect of long-term climate change on political violence is subject to numerous conceptual and empirical challenges, such as an absence of micro-level explanations, conflicting results across different analyses, and issues of endogeneity (Scheffran et al. 2012; O'Loughlin, Linke, and Witmer 2014). These difficulties shifted scholarly attention from long-term climate change to short-term weather variability (Hendrix and Glaser 2007; Ide et al. 2014), including natural disasters, which is the focus of this study.[2]

A natural disaster is defined as a situation in which "a natural hazard affects a vulnerable population so forcefully that it causes substantial death and/or damage" (Slettebak 2012, 164). In this paper, a natural "hazard" refers to a geophysical event such as earthquakes (Alexander 2000), which is exogenous to human factors, while a natural "disaster" is a function both of a natural hazard and human vulnerability, of which the latter is endogenous to society (Busby, Smith and Krishnan 2014). Previous studies, for instance, show that a natural disaster causes different levels

[2] Although natural disasters are often associated with climate changes (IPCC 2014), the former are comprised of sudden and large physical shocks, while the latter takes the form of a gradual and long-term shift in the environments.

of damage, depending on political institutions (Cohen and Werker 2008), economic development, ethnicity (Kahn 2005), and gender roles (Neumayer and Plumper 2007).

Some scholars have posited adverse impacts of natural disasters on intrastate violence, taking positions analogous to the broader literature of Neo-Malthusianism (Kaplan 1994; Percival and Homer-Dixon 1996). They hypothesized that a natural disaster causes marginalization of minorities (Drury and Olson 1998; Raleigh 2010), shortages of basic needs (Brancati 2007; Burke et al. 2009; von Uexkull 2014), and overwhelming demands on a government and a resultant resentment of the government's failure (Berrebi and Ostwald 2011; Carlin, Love, and Zechmeister 2014). According to this approach, a natural disaster leads to acute scarcity, which in turn provides motives, strategic incentives, and opportunities for initiating violent conflicts (Nel and Righarts 2008).

Other scholars have argued that a natural disaster has pacifying effects (Adano et al. 2012; Slettebak 2012). For instance, relying on sociological studies about post-disaster behaviors (Fritz 1996), Slettebak hypothesizes that "disasters should reduce the likelihood of violent conflict through generating a sense of unity among the victims and reducing the importance of divides that might otherwise be conducive to conflict" (Slettebak 2012, 165). Furthermore, large-scale destruction removes buildings and thins vegetation, and thereby creates unfavorable environments for insurgents (Theisen 2012).

These two unconditional explanations not only offer opposing stories, but also lack robust empirical support. The results of cross-country analyses, in particular, are highly sensitive to small specification changes (Gleditsch 2012; Meierding 2013). The absence of consistent findings has led to the recent emphasis on conditional effects of disasters. There is an emerging consensus that numerous antecedent and intervening factors condition the consequences of a natural disaster, and that without accounting for them, the complexity masks the actual causal relationship (Meierding 2013; Scheffran et al. 2012). Previous studies analyzing conditional factors have considered agricultural production and state repression (Nardulli, Peyton, and Bajjalieh 2015), economic growth (Bergholt and Lujala 2012), size of a ruling coalition (Bueno de Mesquita and Smith 2010), and rebel-civilian relationships (Walch 2014).

Surprisingly, however, no studies of which I am aware have systematically analyzed the role of post-disaster reconstruction in the relationship between a natural disaster and political violence. Post-disaster reconstruction refers to "the process of repairing damage, restoring services and (re)constructing facilities after disaster has struck" (Alexander 2002, 5).[3] Because post-disaster reconstruction is the opposite of destruction, it holds special importance for properly specifying the effect of a natural disaster. If a natural disaster induces greater scarcity of basic materials, for instance, reconstruction could compensate for this shortage, which may either rescue the victims from despair or drive them to self-interested competition for disaster relief. Thus, the effect of a natural disaster on intrastate violence is highly contingent on post-disaster reconstruction, and without isolating the heterogeneous effects, we can only weakly identify the causal relationship.

The absence of scholarly consideration of post-disaster responses relates to a broader theoretical problem in the literature. Previous studies of disasters and conflicts have not answered the core puzzle of war: why do actors initiate a violent conflict despite its *ex post* inefficiency (Fearon 1995)? In other words, are there any reasons that destruction and post-disaster

[3] For analytical purposes, I use "reconstruction" as a generic process and "construction" as specific efforts of building facilities.

reconstruction impede a peaceful settlement and thus lead to costly violence? Both the Neo-Malthusian and disaster-peace approaches consider the onset of violence as a function of resource availability, but they do not provide the reasons why a shift in the amount of resources triggers inefficient violence. This corresponds to the generic limitation in the expected utility models (Bueno de Mesquita 1985; Grossman 1992); the existence of something new to bargain over does not preclude the peaceful division of the stakes, and hence we need an explanation about the bargaining failure. The emerging conditional arguments are more nuanced, but still do not explicitly answer this question. As discussed in the following section, bargaining theories of war provide insight into this problem.[4]

RECONSTRUCTION, STRATEGIC OPPORTUNITIES, AND A COMMITMENT PROBLEM

A central puzzle of bargaining theories is that of inefficiency (Fearon 2004); why government and rebel parties fight each other even though war entails massive human and material costs. Ideally, whether a natural disaster produces scarcity or abundance of relief, parties would peacefully divide the resources without paying extra costs for violence. A peaceful solution, however, is not always achieved, most notably due to commitment problems (Powell 2006). When the power balance between parties begins shifting, a rising party has an incentive to renege on the peace agreement and demand a better settlement once it gains power. Being aware of this incentive, the opponent

[4] The explanation provided in the following section is similar to those of Lischer (2006) and Narang (2014). My focus, however, is to show that destruction itself does not generate the expectation of a future power shift, while reconstruction does create such an expectation and thus triggers a war.

cannot accept a settlement and initiates war to stop the power shift. Note that the commitment problem assumes that parties foresee a power shift in the future, and do not look back at a power shift in the past. Indeed, if the power shift were completed in the past, parties could reach a negotiated settlement based on the ex post power balance.

Based on the commitment problem framework, I explain how the two primary components of a natural disaster–destruction and reconstruction–affect the strategic dynamics of civil war. Throughout this section, I consider two parties that contest for a preexisting bargaining issue, such as territorial autonomy and government authority.[5] Following the foreign aid literature (Nielson et. al 2011; Findley et. al 2017), I also assume that negotiators do not have control over the allocation of disaster relief. Given the urgent humanitarian needs, donors' strategic interests (Girod 2012), vested local politics (Strandow et al. 2014), and bureaucratic processes (Carey 2007), it is extremely difficult for negotiators to manipulate the allocation of disaster relief for the purposes of war bargaining.[6] The possible cases that potentially meet these assumptions include the 1970 Bhola Cyclone in East Pakistan, 1972 Nicaragua Earthquake, 1976 Guatemala Earthquake, 2002 Hindu Kush Earthquake, 2008 Cyclone Nargis in Myanmar, 2012 and 2015 avalanches in Afghanistan, and the 2004 Tsunami in Sri Lanka.

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[5] Although this is beyond the scope of this paper, a recent study extends bargaining theory to multiple competing parties (Wolford 2015).

[6] One could argue that negotiators might not have full controls *over a bargaining issue as well*. This, however, requires further extensions to intra-party politics (for instance, Wolford 2012). In this paper, I follow the standard assumption in bargaining theory.

Note also that although the primary outcome variable in this paper is the locations of intrastate violence *given* the onset of civil war, the explanation about locational patterns of ongoing civil war requires proper understandings about the onset of the war. Furthermore, there are no reasons to believe that bargaining theory has implications only about the onset of civil war. Thus, I focus on strategic dynamics after a natural disaster in order to derive implications about the location of intrastate violence.

Destruction

A natural disaster entails substantial fatalities and property destruction. The distribution of destruction, including damages to military facilities, deaths of solders and supporters, and loss of tax sources, is rarely proportional among parties, and thus destruction may disturb the power balance between a government and potential rebel organizations. However, because a physical hazard and the extent of destruction cannot be known in advance, destruction generates only the *ex post* fact of, not *ex ante* expectation for, a power shift. In other words, because parties cannot predict the onset of a natural hazard, the parties have no expectation for a future power shift before the disaster and thus engage in bargaining *as if there would be no power shift in the future*.[7] Immediately after a hazard occurs, the parties have already experienced the power shift, and thus

[7] According to the formal model of Powell (2006), if parties foresee a sufficiently large power shift before the onset of a natural hazard, they should initiate war even before the hazard. Thus, the fact that parties do not engage in war before the onset of a hazard means that there is no expectation for a major power shift at the time.

they can reach a new settlement based on the post-disaster power balance.[8] Since the power shift already took place, neither party has reason to fear a weakened bargaining position in the future. Therefore, from the perspective of bargaining theory, destruction itself does not cause a commitment problem.

Moreover, destruction does not *automatically* create the expectation of reconstruction and the resultant power shift. The post-disaster reconstruction is a highly political process and almost never a mere function of destruction. As the literature on foreign aid suggests, the allocation of international aid depends on numerous factors, including the strategic interests of the donors (Girod 2012), the political stakes in a recipient country (Strandow et al. 2014), and the bureaucratic process in planning and implementation (Carey 2007). This means that destruction is only a crude proxy for reconstruction, and thus that we need to consider post-disaster reconstruction as an independent variable.

Reconstruction

If there is a commitment problem after a natural disaster, it comes from the expectation of post-disaster reconstruction. Unlike destruction, reconstruction can be anticipated, at least after the initial implementation. It thus generates an *ex ante* expectation of a power shift. Although the

[8] Destruction and resultant insanitation may sow epidemics and gradually erode the human resources of a region, resulting in a power shift. I assume however that post-disaster reconstruction generally outweighs the long-term power shift caused by destruction. An epidemic, for instance, will eventually spread in both government- and rebel-controlled regions, and hence the distribution of medicines generally determines which side suffers a relatively large loss of human resources.

complexity of the post-disaster reconstruction process may obfuscate the pattern of relief allocation, the initial implementation of reconstruction and a resultant slight shift in the power balance indicate which parties will obtain a more substantial amount of benefits and hence a bargaining advantage in the long run.

Post-disaster reconstruction provides strategic opportunities to accumulate power. As the political studies on foreign aid imply (Strandow et al. 2014), disaster relief can directly benefit parties. As an intermediary agency, a party that controls the afflicted locations is able to receive foreign relief (Blouin and Pallage 2008). This allows a party to divert the aid to military purposes, especially in the absence of proper monitoring mechanisms during the humanitarian emergency. In addition, parties can levy taxes and fees on the import of emergency supplies and visits by humanitarian agencies, and even plunder the resources (Webersik 2006).

More importantly, in the absence of lucrative resources such as gemstones and oil, parties need to rely on taxes and conscription from citizens. As the economic literature of foreign aid suggests (Collier and Dollar 2002), international relief can stimulate the growth of local economies and populations. Indeed, post-disaster reconstruction involves resettlement of victims, which in turn increases the consumption level and thus bolsters local business even to above the pre-disaster level (Skidmore, Mark, and Hideki Toya 2002). In the long term, the resurgence of the population and economy provides parties with important sources of taxes and manpower, and hence influences the power balance between parties (Strandow et al. 2014). These points indicate that post-disaster reconstruction can cause a potentially large shift in the power balance.

Except on rare occasion, it is extremely difficult to allocate post-disaster reconstruction proportionally so as to mitigate a future power shift. The strategic interests of donors can permeate the allocation of disaster relief, favoring one side over another (Girod 2012). Furthermore, when a party has a say in the allocation of disaster relief, the party has strong incentives to speak for local demands even though it creates a strategic deadlock in the war bargaining (Lischer 2006). Finally, because the parties engage in bargaining *given the power balance after a natural disaster*, if one side suffers significantly heavier damage, the return to the pre-disaster status means a power shift from the post-disaster status quo. This means that in most cases the power shift is prevented only at the expense of meeting humanitarian needs. Thus, although it is theoretically possible that the parties could allocate resources without perturbing the power balance (Chadefaux 2011), they are rarely able or willing to do so in practice.[9]

As formal models about preventive war show (Fearon 1995; Powell 2006; Walter 2013), the expectation for a future power shift triggers a war. A party that is expected to gain less from post-disaster reconstruction resorts to violence in order to prevent the power shift. If it were not for the preventive war, the opportunities of post-disaster reconstruction would allow the opponent to misappropriate disaster relief, expand their tax and manpower bases, and therefore accumulate power over time. Because the power shift would erode its bargaining position (Powell 2006), the declining party would be forced to accede further concessions. To prevent the unfavorable shift in

[9] The argument can be extended to the cases where parties have imperfect but partial controls over reconstruction. As Powell (2006) and Chadefaux (2011) show, a peaceful settlement requires parties to influence every bit of power transfers (so-called a continuous bargaining space). For instance, when parties can influence the allocation of disaster relief only at program levels, the power adjustment remains coarse and hence it still results in a power shift. The power shift may be small in the short term but can accumulate to very large in the long term.

the power balance, the party is incentivized to initiate war and thus seize the locations of reconstruction programs or at least disrupt its opponent's opportunities.[10]

Thus, I hypothesize that post-disaster reconstruction has a causal effect of increasing the number of violent events; *ceteris paribus* violent events are more likely to increase in the locations of active reconstruction. Note that while bargaining theories have been applied to the onset of war at a national level, they also have implications at a subnational level, as this hypothesis indicates. If a power shift triggers a civil war, violent events should be concentrated in the locations associated with the power shift. This paper focuses on the subnational implication.

In contrast to the disaster-peace approach, which identifies destruction as a main explanatory variable, I posit that destruction is only a rough proxy of reconstruction and thus its effect is only weakly identified, as destruction is not a primary cause of a commitment problem. To examine this possibility, I estimate the effect of destruction on violent events. More importantly, in contrast to the Neo-Malthusian approach, which claims that violence arises from scarcity, bargaining theory predicts that violence is more likely to happen in the locations of active reconstruction. Because the predictions are opposite and neither of the theories is nested in each other, I empirically test the hypotheses by estimating the effect of reconstruction on violent events.[11]

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[10] Given the strategic incentive of the declining side, the rising side also has an incentive to preempt the attacks. Thus, the theory does not exactly specify which side starts violence at a subnational level.

[11] Another possibility is that parties compete for reconstruction materials even incurring the costs of violence; put simply, violence happens if there is something to fight for (a something-to-fight-

An empirical challenge is that unlike geo-physical hazards, reconstruction is endogenous to violent events. Because people tend to avoid living and working in violence-prone areas, there may be reverse causation; future violence may curtail current reconstruction efforts. Moreover, outbreaks of violence will hinder data collection on post-disaster reconstruction programs. This results in underreporting of violent events in locations of higher violence risks and thus further underestimates the causal effect of reconstruction. These problems require careful case selection and a suitable identification strategy, to which I now turn.

THE 2004 TSUNAMI IN SRI LANKA

In the following empirical analysis, I chose Sri Lanka as it provides at least five unique analytical opportunities. As an island beside the southern tip of the Indian subcontinent, Sri Lanka has a long history of civil war (Bandarage 2009), which can be traced back to 1983 when the Liberation Tigers of Tamil Eelam (LTTE), the northern-based Tamil separatists, killed thirteen government soldiers. Although the first war, called the Eelam I War, was halted in 1987 by an Indian military intervention, the peace talks broke down and were followed by another war (the Eelam II War), which continued until both sides agreed on a ceasefire in January 1995. The ceasefire lasted less than a year and the country descended into the Eelam III War in July 1995.

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for explanation; Grossman 1992; Collier and Hoeffler 2002). Note that the something-to-fight-for and bargaining explanations are complementary; while the former explains how post-disaster reconstruction increases the stakes of bargaining, the latter explains why the bargaining actually fails. Because previous studies, both Neo-Malthusian and disaster-peace approaches, rest on utility-based explanations, this paper extends the bargaining theory and hence fills the missing link in the something-to-fight-for argument.

The first reason for the case selection lies in the fact that the 2004 Indian Ocean Tsunami occurred between the two civil wars, the Eelam III and IV wars, which provides a unique opportunity for a before-after comparison. The Eelam III War began in July 1995 when the government launched an offensive operation against the LTTE and ended with a ceasefire agreement on 21 February 2002. After nearly four years under the ceasefire and the tsunami in 2004, the country descended back into violence with the outbreak of the Eelam IV War in early 2006. Taking advantage of this temporal structure, I investigate the *changes* in the counts of violent events between the two wars, which controls for static factors, such as geography, and reduces potential confounders.

Second, the existence of the ceasefire agreement at the time of the tsunami meant that subnational data were collected over the entire country, which are rarely available in countries facing civil war. The government of Sri Lanka and the LTTE signed a ceasefire agreement on 21 February 2002. However, the ceasefire agreement encountered increasing uncertainties, such as the LTTE's unilaterally pulling out of the negotiations in April 2003, the demonstration of over 50,000 Sinhalese nationalists opposing the ceasefire in February 2004, the LTTE's split into two factions in March, and the rise of the Sinhalese nationalist party in the parliamentary election in April. The tsunami and nationwide suffering in December, at least tentatively, rescued the ceasefire from possible breakdown (Bandarage 2009). This political environment enabled the Department of Census and Statistics to gather the data regarding the tsunami and its effects even in the LTTE-held regions.

Third, the unexpected nature of the magnitude and distribution of tsunami damage ensures relative exogeneity of the physical hazard to confounders, providing a unique source for an identification strategy. Although the first wave hit the island 120 to 150 minutes after the earthquake (Wijetunge 2009), the absence of a warning system and prior experience hindered a rapid response and evacuation across the nation, making the disaster almost unexpected for the residents (Kurita et al. 2006). Indeed, according to the best available records, the last tsunami had occurred in 1883 and produced a wave of only 0.5 meters in Colombo (Goff et al. 2006). That tsunami also lacked a significant second wave. In addition, the fact that the extent of suffering due to the 2004 tsunami varied across the country also ensures the variability of the destruction variable. According to the census, while more than one third of the houses in the eastern coastal districts collapsed, less than one sixth were completely destroyed in the west (Department of Census and Statistics 2005).

Fourth, in addition to the tsunami damage, the post-disaster reconstruction process also varied across the country, which provides variability on the key explanatory variable. Despite the urgent needs, the number of constructed houses increased only by one and a half times in the northern and northwestern provinces, while the number tripled in the eastern and southern provinces. The variation of housing construction may reflect reconstruction policy that intentionally or unintentionally favored the government's strongholds. Indeed, by May 2006 the Reconstruction and Development Authority (RADA) disbursed over 46 million dollars in order to build houses in the southern and eastern provinces of Sri Lanka, which were under the control of the government, while the rebellious northern and northeastern provinces received only about 13 million dollars (RADA 2006).

Fifth, after the natural disaster, Sri Lanka experienced another civil war, which is a prerequisite for this analysis because the outcome variable is a subnational pattern of violent events. Although the government and LTTE agreed on the Post-Tsunami Operational Management Structure after the disaster, the Muslim and Sinhalese constituencies protested the agreement, and a former coalition partner, JVP, appealed to the Supreme Court, claiming the agreement would infringe on its sovereignty. The LTTE also boycotted the presidential election in November 2005. In that election, Rajapakse, seen as a hardliner against the LTTE, won a majority of votes. Around early 2006, there were increasing skirmishes, suicide bombings, assassinations, factional battles within the Tamil rebels, and reports of human rights violations committed by the LTTE. In February 2006, the new president ordered a full-scale military offensive against the Tamil Tigers, which is generally regarded as the de facto beginning of the Eelam IV War. That war continued until May 2009 when the president declared a complete victory over the LTTE.

Despite these analytical opportunities, the Sri Lankan Civil War may be a somewhat unique case. Although Sri Lanka has several common characteristics of civil war countries—such as underdevelopment, fragile democracy, and ethnic diversity—this case differs in that the rebels were highly organized and the duration of the conflict was one of the longest of all civil wars. Nonetheless, because the theory of commitment problems is particularly appropriate for prolonged wars (Fearon 2004; Powell 2006), the case of Sri Lanka provides an opportunity to test its applicability to the analysis of natural disasters and violent conflicts. To be sure, the purpose of this paper is not to establish a general law but to show that without considering reconstruction, destruction could at best weakly explain the pattern of civil war. The analysis of a single case serves this purpose.

RESEARCH DESIGN

The unit of analysis in this study is the Grama Niladhari (GN) division, the lowest of Sri Lanka's four administrative levels (their average geographical size is about two thirds of Manhattan).

Because the scope of the theory implies areas that are susceptible to a tsunami, I limit the sample to the 1,567 GN divisions within one kilometer of the coast line.[12]

The outcome variable of interest is a count of violent events, obtained from Yuichi Kubota (Kubota and Kikuta 2014).[13] The violent events are coded based on the procedures of the Armed Conflict Location and Event Data Project (ACLED).[14] Political violence is defined as "the use of force by a group with a political purpose or motivation" (Raleigh, Linke, and Dowd 2014, 5). The variable, *violence,* is a count of all types of battle events between the government and rebel organizations, and violence against civilians that resulted in more than one deaths.[15] For the period before the tsunami, I include the past events during the Eelam III War up to the day of the tsunami (19 April 1995 - 26 December 2004). For the post-disaster period, the sample contains the events during the Eelam IV War, excluding the events in 2004 and 2005 to avoid endogeneity concerns

[12] Descriptive statistics are provided in Table A1-1-1 and Figure A1-1-1 in SI 1-1. I also conduct the analyses with divisions up to five or ten kilometers from the coast. The larger sample sizes even increase the statistical significance. See the later subsection about robustness checks.

[13] Upon publication, the data used in the analysis will be publicly available.

[14] The data sources are the newspaper articles stored in LexisNexis, primarily the wires of the Associated Press and United Press. See Table A1-2-1 in SI 1-2.

[15] I check robustness of the measurement. See the subsection for the robustness checks.

(1 January 2006 - 19 May 2009).[16] In total there are 167 events in 50 divisions before the tsunami and 117 events in 35 divisions afterwards.[17]

The empirical analysis is comprised of two regression models: one of violence on destruction, which addresses the logic of previous studies, and a main regression model of violence on reconstruction. In the main model, the key explanatory variable is *construction*, which is available at the GN division level.[18] The data are obtained from the Census of Population and Housing 2012 (Department of Census and Statistics forthcoming). For the period before the tsunami, the data are the annual average of newly constructed houses from 2000 to 2004.[19] For the post-disaster data, the variable captures the number of constructed houses in 2005.[20]

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[16] Including the events during 2004 and 2005 does not change the results. See the next subsection for the robustness checks.

[17] Note that the Eelam IV War was shorter and more intensive than the Eelam III War. In annual averages, there were 17.3 and 35.1 events each year during the Eelam III and IV Wars respectively.

[18] The number of houses that were reconstructed by the recovery programs is available only at the DS division. As I will discuss in the subsection of robustness checks, I find that *construction* correlate with the DS-level reconstruction variables in statistically significant and substantively sensible ways.

[19] Table A1-2-7 in SI 1-2 describes the data source. The data for each individual year are not available in the census.

[20] Because the sample distribution of this variable is highly skewed, and also because this variable is an outcome variable of the first-stage regression of an instrumental variable analysis, I take its natural logarithm. In addition, although the housing construction data in 2006 is also available, I

Note that the measurement of housing construction is retrospective; the number of constructed houses was recorded during the 2010 census. This fact raises a substantial risk of reporting bias. The bias, however, is expected to be opposite the working hypothesis. Housing construction would tend to be undervalued in divisions that are more susceptible to violent events, and thus a larger number of constructed houses should be associated with a *lower* risk of violence, which is contrary to my prediction. Therefore, if the results show a positive relationship, the finding can be considered as stronger evidence. In the robustness check, I also assess the validity of the measurement by comparing the housing construction data to alternative data. In addition, the instrumental variable approach I later describe further addresses this endogeneity problem.

The variable, *destruction*, is an explanatory variable in the destruction regression and a control variable in the main model. The variable is the proportion of collapsed, destroyed, and damaged housing units to total housing units.[21] The data are derived from the first-round reports of the Tsunami Census (Department of Census and Statistics 2005). I divide the sum of collapsed,

only use the data in 2005 to capture the initial implementation of reconstruction. I also check the robustness of this decision. See the next subsection for the robustness checks.

[21] Table A1-2-4 in SI 1-2 describes the data source. I suspect the death and casualty measures are more susceptible to endogeneity, because these measures highly depend on the people's instant behaviors after the occurrence of the earthquake. The indicators of non-housing units have less variation and many missing values. The proportion of collapsed, destroyed, and damaged non-housing units is very highly correlated with *destruction* (r = 0.898).

destroyed, and damaged housing units by the total number of housing units.[22] Since the tsunami may affect violent events not only by destroying buildings but also by directly affecting the population and causing demographic shifts, I include a control variable, *affected*, in all models. This variable is derived from the 2004 Tsunami Census to measure the proportion of people who moved out of a division after the tsunami.[23]

To these data, I apply an instrumental variable approach. First, and most importantly, I use an instrumental variable analysis for addressing the problems of reverse causality and reporting bias, as well as the omitted-variable problem. Intuitively, the instrumental variable estimator limits the variation of an explanatory variable to that caused by an exogenous variable, called an instrumental variable. Because such exogenous variation cannot be explained by omitted variables, reverse causality, or reporting bias, the instrumental variable design can effectively address these inferential problems. The instrumenting variable is offshore wave height (*wave*), a measurement of the level of the physical hazards. The average wave height of each GN division is computed from the GIS data of Garcin et al. (2008), which is available at the 540 meter grid resolution.[24] I

[22] I check the robustness of the measurements of housing destruction. See the next subsection for the robustness checks.

[23] Despite the high correlation of *affected* and *destruction* ($r = 0.811$), exclusion of *affected* does not alter the results.

[24] Table A1-2-11 in SI 1-2 describes the data source. The method of simulation is a modified version of the GEOWAVE model. The GEOWAVE model requires the information about the locations and magnitudes of the seismic movements as well as bathymetry. Later, as a robustness check, I also repeat the analysis using a different data source of tsunami.

use the offshore, instead of nearshore, measure for isolating it from the coastal geographies and infrastructure, which could introduce endogeneity.[25] In each of the destruction and reconstruction regressions, the explanatory variable is instrumented by the tsunami wave height.

Although the instrument of offshore tsunami wave height addresses the omitted-variable, reverse-causality, and missing-data problems, it is not perfectly random and does correlate with pre-tsunami demographic and geographic variables as I mention in the robustness check. To control for these pretreatment conditions, I use the first-differences of the variables. In particular, I estimate how the *differences* in the numbers of constructed houses affected the *differences* in the counts of violent events before and after the tsunami. Because the pre-tsunami conditions are fixed and thus cannot explain the changes in the outcome variable, the first differencing accounts for the

[25] There are a number of difficulties to measure nearshore wave height, runup elevation, runup distance, inundation areas, and inundation levels, partly because these metrics are affected by coastal infrastructure and geographies. However, because offshore waves are less affected by coastal infrastructure and geographies, they are more exogenous and thus more appropriate as an instrument than the alternatives.

pre-tsunami confounders.[26] Thus, I assume that after differencing out the variables, the instrument is orthogonal to potential confounders.[27]

Another assumption required for the instrumental variable analysis is the exclusion restriction; the instrument should have conditionally no effect on the difference in violent events except for the effect via the instrument (Sovey and Green 2011). Controlling for housing destruction and demographic shifts “blocks” the backdoor paths that circumvent the explanatory variable. In other words, by holding the levels of destruction and population changes constant, I can exclude the possibility that wave heights affect the number of violent events bypassing its

[26] The first-differencing accounts for pre-tsunami, not post-tsunami, confounders. Although demographic compositions might change after the tsunami, these post-tsunami changes were most likely to be the effects of the tsunami-related destruction, and hence the instrumental variable and the control for destruction effectively address the problem.

[27] Although I believe that first-differencing mitigates this risk, in a robustness check, I also add further controls, including regional fixed effects and observed covariates respectively.

effect on housing construction.[28] I also estimate an overidentified model to show the Sargan test does not reject the validity of the instruments.[29]

The instrumental variable analysis also assumes that the instrumental variable has a strong conditional effect on the explanatory variable. Holding the levels of housing and human damage constant, the offshore wave height positively correlates with an increase of constructed houses. When a division suffers a higher wave but bears the same degree of damage as a division experiencing a lower wave height, the division is proven to be more resilient to tsunamis than the others; the level of destruction is the same despite a higher wave. Thus, if people prefer tsunami-resilient locations to vulnerable places, they should build more houses in the locations of higher waves *even after controlling for the level of destruction*. As I later discuss, the instrument has very strong predictive power. In addition, I also assume that the instrument has a monotonic relationship with the explanatory variable, and that the wave height in a given division has no conditional effect on the outcome variables in the other divisions.

Formally, therefore, the main regression of violence on reconstruction is expressed as;

$$\Delta violence_i = \beta_0 + \beta_1 \Delta \ln(construction)_i + \beta_2 destruction_i + \beta_3 affected_i + \varepsilon_i \; ;$$

$$\Delta \ln(construction)_i = \delta_0 + \delta_1 wave_i + \delta_2 destruction_i + \delta_3 affected_i + u_i \; ,$$

[28] The blocking strategy requires an assumption that the level of destruction that is not explained by the wave heights, namely wave-unrelated destruction, is orthogonal to the difference in violent events. Although pre-tsunami socio-economic factors, such as housing resilience, may affect the level of destruction, I assume that the first-difference strategy mitigates the confounding effects of these pre-tsunami fixed variables.

[29] Because the over-identification test requires multiple instruments, I use a standard deviation of the wave heights as an additional instrument. See the next subsection for the robustness checks.

where i is a subscript for a GN division, and Δ denotes a first-difference. In the estimation, I use robust standard errors, and therefore apply the two-step feasible generalized method of moments (GMM) instead of two-stage least square (2SLS) estimator.[30]

ANALYSIS

While previous studies have considered destruction as a primary explanatory variable of intrastate violence, bargaining theory predicts that destruction itself has no observable effect on violent events because it does not eliminate the possibility for a negotiated settlement. I instead propose post-disaster reconstruction as a cause of a commitment problem, hypothesizing that parties fight for the strategic opportunities created by post-disaster reconstruction and thus that violent events are more likely to increase in the locations of housing construction. Table 1 presents the empirical results of these expectations.

[30] As discussed in the sensitivity checks, the results of the 2SLS, LIML and GMM estimates are the almost same and statistically significant.

Table 2-1. Estimates on the Effect of Housing Construction on Violence

Outcome: Δ*violence*	(1)	(2)	(3)	(4)
Δln(*construction*)			0.006	**0.577****
			(0.03)	**(0.28)**
destruction	-0.109	**1.329***	-0.113	-0.487**
	(0.15)	**(0.75)**	(0.15)	(0.21)
affected	0.079	-1.640	0.077	-0.104
	(0.21)	(0.89)	(0.21)	(0.26)
Constant	-0.017	-0.142	-0.020	-0.269*
	(0.04)	(0.09)	(0.04)	(0.15)
Instrument	No	*wave*	No	*wave*
Estimator	OLS	GMM	OLS	GMM
Observations	1569		1567	

* $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Robust standard errors in parentheses.
Estimates of instrumented variables are shown in bold letters.

As seen in the first two columns in Table 1, destruction and violent events have no discernible relationship. The first column in Table 1 shows the baseline OLS estimate without instruments. The coefficient of the destruction variable alone is not significantly different from zero (p = 0.468). In the second column, I use wave heights as an instrument of the number of destroyed houses. The coefficient has a p-value close to the conventional threshold (p = 0.075), suggesting a potential but inconclusive increase in violent events. Substantively, a division suffering complete destruction is predicted to experience one more violent event than a division with only a very minor level of destruction (destruction of 2 percent of houses). Despite the substantial effect size, the standard error is relatively large, implying weak identification of the causal effect. The estimate can be unbiased, but it is substantively under-identified as we cannot reject the possibility that the positive relationship reflects the effect of post-disaster reconstruction of houses instead of destruction itself.

The last two columns in Table 1 present the estimates of the main model that includes the variable of housing construction. The theory predicts that violent events became more frequent in more actively reconstructed divisions. Although the OLS estimates yield null results due to the endogeneity problem discussed above, the instrumental variable model provides firm support for the hypothesis. The OLS estimate of the coefficient in the first column of Table 1 exhibits a weak association due to two factors: housing construction's effect to increase violent events and the endogenous effect of violence to decrease reports of housing construction. Since people tend to avoid and underreport housing construction in violence-prone areas, the endogeneity may mute the causal effect of housing construction on violent events.

Once I account for the endogeneity, as seen in the last column in Table 1, the regression coefficients behave as expected. Consistent with the hypothesis, higher growth in housing construction *increases* violent events (p = 0.041). The coefficient represents a change in the number of violent events when the number of constructed houses from the pre-tsunami level is roughly doubled. The effect size suggests that doubling the number of constructed houses from the pre-disaster level, which is not unusual in the sample, is sufficient to offset the reduction of violent events due to the destruction of all houses.[31] In an absolute number, if three houses are built in a division without any prior houses, it increases the number of violent events by one on average.

In the main model, the coefficient on the destruction variable becomes negative and is statistically significant. This might imply that the positive relationship observed in Table 1 indeed

[31] In fact, the values of *construction* were more than double the pre-disaster levels in one third of the divisions.

proxies the effect of reconstruction, while destruction itself has an effect to reduce the number of violent events. However, because the destruction variable is a control in the main model, the coefficient estimate is subject to posttreatment-control bias, and hence it should not be interpreted as a causal effect (Angrist and Pischke 2009; Achen 2005).[32] Thus, without denying the possibility that destruction has a pacifying effect independent of reconstruction, I leave further examination to future studies. The bottom line is that the causal effect of destruction is discernible only when we account for reconstruction. This underlines my argument that post-disaster reconstruction is crucial in the relationship between a natural disaster and conflicts.

In the main instrumental variable model, the predictive power of the first-stage regression is very strong, suggesting there is no problem of a weak instrument; the coefficient of *wave* is highly significant ($p < 0.001$) and the robust F statistic is 43.39, far beyond the Stock-Yogo critical value. The exogeneity of housing construction to the outcome variable is rejected at a 5 percent significance level, suggesting the existence of endogeneity. This result is consistent with the above interpretation that the reporting bias obscures the causal effect of housing construction.

Robustness checks

I check the sensitivity of the results to additional controls, spatial dependency, measurement, and various specifications and estimations. Table 2 provides a brief summary of the results.

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[32] Under the framework of mediation analysis, the coefficient for destruction in the last column of Table 1 would be interpreted as the effect of destruction independent of housing construction. But Bullock and Ha (2011) show that this interpretation requires a strong set of assumptions, which I do not think the present analysis satisfies. For this reason, I do not give a definite interpretation about the coefficient.

Table 2-2. Summary of Robustness Checks

	Tests	Pass the test?	SI Table
Measurement	(1) Exclusion of violence against civilians	Yes	A1-2-2
	(2) Inclusion of riots and protests	Yes	A1-2-2
	(3) Exclusion of low-intensity violence	Yes*	A1-2-2
	(4) Inclusion of violent events in 2004 and 2005	Yes	A1-2-3
	(5) Exclusion of events during the peace talk	Yes	A1-2-3
	(6) Standardized violence	Yes*	A1-2-3
	(7) Alternative calculations of housing destruction	Yes	A1-2-6
	(8) Alternative measurement of housing construction	Yes	A1-2-8
	(9) Alternative data of wave height simulation	Yes	A1-2-12
	(10) Divisions up to 5 or 10 km away from the coast	Yes	A1-2-13
Control Variables	(11) District fixed effects	No	A1-3-2
	(12) Province fixed effects	No	A1-3-2
	(13) Twenty-one covariates	No	A1-3-3
	(14) District fixed effects with larger samples	Yes*	A1-3-4
	(15) Province fixed effects with larger samples	Yes	A1-3-4
	(16) Twenty-one covariates with larger samples	Yes**	A1-3-4
Spatial Dependency	(17) Control for neighbors' average of *destruction*	Yes*	A1-4-1
	(18) Control for neighbors' average of *construction*	Yes*	A1-4-1
	(19) Control for neighbors' average of *violence*	Yes	A1-4-1
	(20) Standard errors clustered at a DS division level	Yes*	A1-4-2
	(21) Standard errors clustered at a district level	Yes*	A1-4-2
	(22) Standard errors clustered at a province level	Yes*	A1-4-2
Specification and Estimation	(23) 2SLS estimates	Yes	A1-5-1
	(24) LIML estimates of the overidentified model	Yes*	A1-5-1
	(25) OLS estimates of the reduced form	Yes	A1-5-1
	(26) Ordered probit model	Yes	A1-5-2
	(27) Multinomial probit model	Yes	A1-5-2
	(28) Separate IV probit models	Yes	A1-5-2
	(29) Separate rare-event logit models	Yes	A1-5-2

* Significant at a 10 percent level.
** Significant only with the sample of GN division up to 10 km away from the coast.

First, the results are quite robust to changes in measurement, such as the exclusion and inclusion of different types of violent events, different weightings of the housing destruction measure, different ways to calculate the number of constructed houses, alternative wave height data, and inclusion of GN divisions up to 5 or 10 kilometers away from the coast (Tests 1 to 10 in Table 2).[33] I also assess the validity of the housing construction data by comparing it to an

[33] For the details of the weights for the housing destruction data, see Table A1-2-5 in SI 1-2.

alternative dataset.[34] The Reconstruction and Development Agency (RADA) made *DAD Data Report 2nd edition* on 15 May 2006, which contains the numbers of housing units at four stages: (1) an original Memorandum of Understanding (MoU) was made (*planned*), (2) the MoU was actually signed (*signed*), (3) the housing units were under construction (*started*), and (4) the construction was completed (*completed*). One shortcoming however is that the RADA dataset is available only at the level of DS divisions (third administrative level) and thus the number of observations is very limited (86 DS divisions).[35] To check the validity of $\Delta construction$, which is available at the level of GN divisions, I regress the RADA indicators on $\Delta construction$. The results show that not only all of the four indicators are positively associated with $\Delta construction$ but also the coefficients for *started* and *completed* are more than three times larger than those for *planned* and *signed* and they are statistically significant. The latter result is consistent with the fact that the signature of MoU does not necessarily entail housing construction, while starting construction almost certainly means the completion of the project.

Second, the tsunami wave heights may not be perfectly random and therefore may correlate with some confounders. In particular, as shown in Figure A1-3-1 in SI 1-3, the tsunami height correlates with demographic, ethnic, religious, and geographic variables. One way to address this concern is to limit the comparison within a specific group, presuming that the GN divisions within

[34] See Table A1-2-19 in SI 1-2.

[35] Because the instrumental variable analysis requires a sufficient number of observations (the estimators are consistent but biased), and also because the outcome variable does not have a large variation, the instrumental variable analysis is biased and inefficient if I use the RADA dataset. For this reason, I use the 2012 housing census as a primary data source.

a group are homogeneous. Then I add fixed effects for the districts (the second administrative units) and provinces (the first administrative units), and demographic and geographic covariates respectively (Tests 11 to 13 in Table 2).[36] These methods substantially reduce the variability of the variables, and if the results hold, it should provide strong support for the hypotheses. Once I include either of the fixed effects or the DS-level covariates, however, both of the coefficients become statistically indistinguishable from zero.

This result may be due to the reduced efficiency in the estimators or to the presence of unknown confounders. To diagnose this, I increase the sample size by including the GN divisions up to five or ten kilometers away from the coast. If the regional fixed effects and demographic covariates reduce the efficiency of the estimates without causing biases, the increased sample size should address the problem. In contrast, if the fixed effects and covariates would represent confounding variables, they should alter the estimates and p-values regardless of the sample sizes. After I increase the sample size, nearly all of the coefficients regain statistical significance (Test 14 to 16 in Table 2). Moreover, in all cases, the coefficient values for $\Delta construction$ are within

[36] See Table A1-312 and A1-3-3 in SI 1-3. The demographic covariates are derived from the 1981 Census and hence available only at a Division Secretary (DS) division level (the third administrative units). I also add geographic covariates as well as night light densities, which are available at the GN division level. Table A1-3-1 in SI 1-3 provides descriptive statistics of the covariates.

the confidence interval of the main estimate.[37] Therefore, the best possible empirical data provide no evidence for substantial bias due to the covariate imbalance.

Third, in order to account for spatial dependency, I retest the hypothesis by adding neighbors' averages of the explanatory and outcome variables (Test 17 to 19 in Table 2). The results are similar to the main analyses, though the inclusion of neighbors' average of housing construction modestly increases the standard errors of the coefficients. In addition, the standard errors become only modestly larger even when I cluster it by the DS divisions, districts and provinces respectively, though still statistically significant at a 10 percent level (Test 20 to 22 in Table 2).

Fourth, the results are robust to alternative model specifications and estimations. Following the advice of Angrist and Pischke (2009), I retest the hypotheses with the 2SLS estimation, the limited information maximum likelihood (LIML) estimation of the overidentified model (the standard deviation of the wave heights as an additional instrument), and the OLS estimation of the reduced form (Test 23 to 25 in Table 2). In the overidentified model, I use the standard deviation of wave heights in a GN division as an additional instrument. The 2SLS and LIML estimates are quite similar to those of the GMM estimates. In addition, in the overidentified model, the Sargan test of exclusion restriction does not reject the validity of the instruments at a 5 percent significance level. The reduced-form regression also produces similar results. The results are also robust to non-linear specifications (Test 26 to 29 in Table 2), such as ordered and multinomial probit models

[37] See Table A1-3-4 in SI 1-3. Only when I use the sample of the GN divisions up to 5 km away from the coast, the coefficient in the regression with the covariates has a p-value $p = 0.133$. This is due to the missing values in the covariates and the small sample size.

with the two stage residual inclusion technique (2SRI; Terza, Basu, and Rathouz 2008), separate instrumental-variable probit models, and separate rare-event logit models with 2SRI (King and Zeng 2001).

Finally, because the primary purpose of this paper is the construction of a theoretical model and the identification of the causal effect, and due to space constraints, I leave the brief qualitative assessment to SI 1-6, in which I investigate the disproportionate distribution of disaster relief, possible effects of post-tsunami reconstruction on tax revenues and military expansion, and the failure of bargaining over reconstruction. Although the analysis only probes the plausibility of the quantitative results, the qualitative evidence provides further support for the argument of this paper.

CONCLUSION

I presented a theoretical logic and empirical analysis of the disaster-conflict nexus, introducing post-disaster reconstruction as a crucial explanatory variable. Bargaining theory implies that post-disaster reconstruction causes a commitment problem and thus incentivizes parties to fight for the location of active reconstruction, while destruction does not exclude the possibility for a peaceful settlement. The empirical analysis of Sri Lanka provides robust support for the hypotheses; the statistical results hold in the 26 out of 29 robustness tests.

We need to be cautious about generalizing the case of Sri Lanka. The existence of the ceasefire prior to the tsunami distinguishes the case from some countries ongoing civil wars and those without previous wars, though the relatively long duration of the ceasefire means the Sri Lanka case can resemble the onset of civil war in other cases. Furthermore, although I argue that it is extremely difficult for the government and international donors to allocate reconstruction materials without causing a commitment problem, very careful post-disaster policies might successfully open a way to a negotiated conflict settlement. Such a dynamic might explain, for instance, the peaceful outcome of the 2004 tsunami in the Aceh conflict in Indonesia.[38]

Despite these limitations, this research implies that considering the role of reconstruction and humanitarian responses more broadly is the first step to address the "disparate" (Salehyan 2014, 1) findings in previous studies. Several previous studies, including a widely-cited article by Brancati (2007), found that frequent natural disasters were associated with a larger number of conflict events. Although the authors argue that a natural disaster causes scarcity and violent competition for resources, natural disasters can entail ample emergency aid, which in turn may disturb the extant power balance and lead to violent conflicts. Therefore, without considering post-disaster reconstruction, we cannot decide whether it is destruction or reconstruction that really underlies in the empirical estimate.

Finally, this study has both bad and good news for post-disaster peace policies. The bad news is that reconstruction can *exacerbate* violence. Post-disaster reconstruction can destabilize the extant power balance and thus induce violence. This finding, however, does not necessarily mean that we should abandon all efforts for reconstructing societies devastated by natural disasters. Instead, this analysis suggests that even with urgent humanitarian needs immediately after a disaster, we need to carefully consider the bargaining environments and how reconstruction can potentially affect the strategic calculation of the local actors.

[38] Beardsley and Brian (2009) qualitatively compare the cases of Sri Lanka and Indonesia after the 2004 tsunami, focusing on the incentives of the rebel organizations. The within-group analysis supplements the inter-group bargaining framework of this paper.

Some good news is that reconstruction is manipulable and thus ultimately depends on the decisions of policy makers. In the theory section, I maintained that the rival parties cannot credibly commit to a peaceful settlement due to fear of a weaker future bargaining position. The international community, however, can alter this strategic conundrum by stabilizing the shift in the power balance after a natural disaster and assuring the enforcement of reconstruction for both parties. Although in the case of Sri Lanka the government possessed larger control over the allocation of aid, international organizations could have played a larger role in post-disaster reconstruction so as to keep the power balance between the government and rebel group stable at the post-tsunami level. In this sense, a natural disaster never automatically abolishes or bolsters peace; we can change the trajectories of conflicts. Future research could fruitfully examine the interactions of environments, human responses and civil war, and develop more effective post-disaster peace policies.

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Chapter 3. The Drowning-out Effect: Voter Turnout, Uncertainty, and Protests

On 31 March 2006, when the government of West Bengal, led by the Left Front, announced a deal with an Indonesian conglomerate, Salim Group, to set up a chemical hub spread over 10,000 acres in the town of Nandigram, it raised tensions between the government and the local residents who would be displaced from their land. Two weeks after the agreement, the West Bengal State Assembly Election appeared to confirm broad-based support for the government. Over 81 percent of eligible voters turned out in the election, in which the Left Front obtained 78 percent of the seats. With the landslide victory, which was "beyond all our expectations" (Outlook India 2006) even for the party leader, the government announced the expansion of the project on 18 May, now promising further 10,000 acres of the land in the nearby town of Singur to Tata Motors. The announcement, however, triggered a protest by more than 3,000 people on 1 June. The protest soon escalated into a series of demonstrations and riots that continued for years. Retrospectively, a witness states that "the poll outcome was *wrongly interpreted* as a popular support in favour of the path followed by the LF [Left Front] for industrialisation" (Dinda 2013, 28). Indeed, it is widely believed that the government's failure was one of the major reasons that put an end to the 34-year rule of the leftist government in the following election (Roy 2009).

What are the effects of electoral participation on protests? Our conventional wisdom suggests that high turnout in a free and fair election would be laudable; it might mean better representation of people's opinions, which could allow a government to identify and address social discontents and hence mitigate the risks of protests. The case of West Bengal, however, casts doubt on this intuition, implying an alternative, or even opposite, possibility; high turnout may make a government overconfident in its popularity, and hence make it even more difficult to resolve the conflict efficiently. Does high turnout really help to resolve social conflict without invoking protests? If not, what is the underlying logic?

In this paper, by combining a bargaining model of conflict with a behavioral model of voting, I argue that high turnout indeed *increases* the risk of post-election protest. Even though high turnout may make an election more indicative of the *average* citizen's preferences, it does not necessarily reflect the intensity of a minority's dissatisfaction with a government's policies. Indeed, in the contemporary world, a majority of people are not strongly interested in politics or do not participate in protests, often prioritizing their private lives (World Values Survey 2016). When those people happen to turn out, the vote shares are less representative of those who are motivated enough to protest. The resulting uncertainty can result in the bargaining failure and inefficient outcomes, such as protest. Thus, rather counterintuitively, high turnout is predicted to increase the risk of protests.

Testing this hypothesis, however, raises empirical challenges. As an overwhelming number of electoral studies indicate, voter turnout is endogenous to various electoral strategies, including policy stances, clientelism, and pre-election protest and violence. Drawing on American voter scholarship (Hansford and Gomez 2010), I address the problems by using election-day rainfall deviation as an instrumental variable (IV or instrument) for turnout and applying it to a new constituency-level dataset of Indian local elections. I also extend a new near-far matching approach to the IV analysis (Baiocchi et al. 2010; Keele and Morgan 2016), which can address weak-instrument bias and, perhaps more importantly, make the causal comparison more explicit and less dependent on parametric assumptions. Consistent with the theoretical expectation, the analysis shows that electoral participation raises the risk of protests after the elections.

The finding provides one rational for the idea that electoral democracy is imperfect as a conflict resolution mechanism even in its ideal form. Although conflict studies tend to focus on the problems of electoral fraud and rigging, an election itself may have inherent limitations. As long as all citizens, whether they are interested in politics or not, have rights to vote, elections may not reflect the opinions of real dissenters. This gap can create room for strategic miscalculations and inefficient outcomes. Instead, this study implies that free and fair elections must be complimented with the freedom of assembly so that citizens can efficiently signal the intensity of their preferences and discontents to policymakers.

LITERATURE REVIEW: ELECTION AND CONFLICT

A common explanation about the effect of elections on conflicts can be found in so-called sore-loser effects, which posit that competitive elections create "sore" losers and drive them to pursue options outside the political system, including violent protests and armed conflicts (Collier 2011). This explanation however does not account for why a winner of the election does not accommodate or make concessions to the sore losers. In fact, from the perspective of bargaining theories (Fearon 1995), since having a protest is potentially costly (Pierskalla 2010; Little, Tucker, and LaGatta 2015),[1] the incumbent is better off by offering peaceful conflict resolution and hence avoiding the unnecessary risks of facing protestors. Thus, even if elections create sore losers, it

[1] The costs include a lower likelihood of victory in the next election (Madestam et al. 2013), potential escalation to armed conflict (Little, Tucker, and LaGatta 2015), and lower stock market evaluation on the firms associated with the incumbent (Acemoglu, Hassan, and Tahoun 2018).

does not eliminate the possibility of an efficient conflict settlement. For a complete explanation, we therefore need to explain the failure of peaceful conflict resolution.

One possible explanation for the bargaining failure is informational uncertainties (Fearon 1995).[2] In the presence of asymmetric information, a government and opposition can have conflicting views about the strength of their support bases. The government can underestimate the popularity of the opposition and thus propose a conflict resolution that is unacceptable to its opponents. The opposition rejects such an offer and initiates protests to compel their preferred policies (Londregan and Vindigni 2006; Cheibub and Hays 2017) or, more realistically, to signal the strength of their support base (Little, Tucker, and LaGatta 2015).

From this perspective, an election is considered as an institutional medium though which people express their discontent. It is possible that free and fair elections would reveal the public support for each party, prevent strategic miscalculations, and thus lessen the need for violent actions (Przeworski 1991; Londregan and Vindigni 2006; Little, Tucker, and LaGatta 2015; Cheibub and Hays 2017). Contrariwise, as a number of recent studies show (Magaloni 2010; Daxecker 2014; Hafner-Burton, Hyde, and Jablonski 2016; Wig and Rød 2016; Knutsen, Nygård, and Wig 2017), fraud and rigging can make the election a biased signal of popular opinion, which can create a risk of misunderstandings and hence incentivizes dissidents to take extra-institutional means, such as violent and non-violent protests. The unconsolidated democratic culture, including parochial or ethnicized politics (Varshney 2003; Wilkinson 2006), the lack of democratic

[2] The other possible avenue is the logic of commitment problems (Fearon 1995; Powell 2006), which has been applied to the cases of post-conflict elections (Walter 1999; Chacón, Robinson, and Torvik 2011; Brancati and Snyder 2011, 2013).

experience (Salehyan and Linebarger 2015), and post-conflict instabilities (Brancati and Snyder 2013), can also undermine respect for electoral outcomes and result in inefficient outcomes.

What is missing in these studies, however, is a possibility that elections can be inherently imperfect as a signaling mechanism; since elections by themselves can only provide information about aggregated vote counts, they may not reflect the distribution of preference intensity. For example, Little et.al (2015) and Cheibub and Hays (2017) incorporate this possibility into their formal models, in which elections provide varying qualities of information about popular opinion and thus result in different likelihoods of protests or violent conflicts. These studies, however, treat the quality of the revealed information as exogenous parameters and thus do not explain why some elections provide precise information while others do not.

One potential answer to the question lies in electoral participation. Intuitively, one might argue that elections could accurately represent public opinion only when a large number of, and if possible all, citizens cast votes. For instance, Fearon (2011), who examines the roles of elections in collective actions against a ruler, assumes that all people cast either Yes or Nay votes for an incumbent. With this assumption, the elections are expected to reveal people's opinions, allow the citizens to coordinate their collective actions, and hence incentivize the ruler to appease the citizens. Similarly, Londregan and Vindigni (2006) also assume that voting is costless, and thus that every individual (at least weakly) prefers voting to abstention. This central feature of the model ensures that the elections provide precise information about parties' support bases, reduce the risks of strategic miscalculation, and hence allow peaceful conflict resolution.

Although the unanimous-turnout assumption in these studies is useful for their own purposes, it does not deny the importance of analyzing the strategic consequences of electoral participation. In fact, a large fraction of people in contemporary democracies are not strongly interested in politics, elections, or protest. According to the World Value Survey (2016),[3] 52.5% of the 69,553 respondents says they are not interested in politics, 47.6% of them have never and will never join any political activities, and 14.7% answered that they have never voted either national or local elections. Even among those who have casted votes, 45.3% said they would never participate in political actions. That is, for ordinary citizens, political issues have only secondary importance, and even those who turn out in elections may not be interested in protests or other political activities. If these disinterested citizens turn out in elections, does it help or hinder the information revelation mechanism? In the next section, I answer the question by incorporating a simple behavioral model of voting to a bargaining model.

THEORY: THE DROWNING-OUT EFFECT

Suppose two groups, winner (W) and loser (L) parties of an election, who have a dispute over a political issue. The issue can be local (such as the land appropriation in the case of the Nandigram-Singur conflict) or broader. Consistent with the literature (Little, Tucker, and LaGatta 2015; Fearon 2011), I consider a two-candidate majoritarian system. The winner has an opportunity to make a settlement offer to the loser, but she does not know the exact power of the loser: that is, she does not know how many people would take to the streets if the loser calls for a protest. If a large number of people would join the protest, the winner would be forced to accept

[3] Non-responses are dropped.

their demand, while the winner could easily ignore minor protest.[4] The winner therefore faces a dilemma; if she makes a conciliatory offer, it risks making too large a concession to a weak loser, while an unsatisfactory offer could potentially trigger massive protest.

Importantly, this model also involves n citizens ($i \in \{1, \ldots, n\}$) who are assumed to be shortsighted and thus non-strategic; they make decisions only based on their immediate payoffs. In elections, voters are supposed to compare the immediate benefits and costs of voting while ignoring its possible strategic consequences. This modeling approach is not unconventional as seen in the median-voter theorem (Downs 1957) and audience-costs model (Fearon 1994), but I believe this extension is still a substantial departure from previous studies, which consider elections as exogenous parameters (Little, Tucker, and LaGatta 2015; Cheibub and Hays 2017) or assume unanimous turnout (Londregan and Vindigni 2006; Fearon 2011). Empirical studies indeed show that individual voting behaviors are often determined by their desire to express the opinions and even retrospective utility considerations (Achen and Bartels 2004; Healy and Malhotra 2009; Healy, Malhotra, and Mo 2010). Although this certainly does not mean that people solely rely on expressive utilities in reality or that we can ignore the continuing debate about the instrumental and expressive voting behaviors (Ashworth and Mesquita 2014), I believe the shortsighted-citizen assumption is at least not unreasonable and makes the model tractable.

The shortsighted-citizen assumption is also applied to their participation in a protest. This model choice is somewhere between Londregan and Vindigni (2006) and Cheibub and Hays

[4] This win-or-lose specification is instrumental; in fact, without altering the main results, one can easily extend the following model such that a protest signals the size of protestors and thus allows a negotiated settlement with extra costs for protest. See Fearon (1995, 1997) and Little et al. (2015).

(2017), who do not incorporate individual participation, and Fearon (2011) and Little et. al (2015), who analyze n-player coordination games. Although participation in organized conflicts, such as civil war and revolution, are often motivated by public goals, people can join protests just for expressive purposes even without considering possible strategic consequences. This view is also consistent with previous studies that emphasize the psychological factors and expressive benefits of individual participation (Kuran 1991; Chen, Zachary, and Fariss 2017). Thus, while it is a fruitful avenue for future studies to relax these assumptions,[5] I believe it provides analytical leverage and parsimony without losing generalizability.

The game starts with the generation of citizens' relative attitudes toward the candidates, which are independently drawn from a cumulative distribution function F_θ with the support of both negative and positive values. The negative θ_i means a negative attitude towards L, and vice versa. Neither W nor L knows the exact value of θ_i. In an election, citizens cast votes for W or L if their attitudes towards the loser are sufficiently negative or positive, and otherwise they abstain;

$$u_{i,election} = \begin{cases} -\alpha\theta_i - c & \text{if} \quad i \text{ votes for W} \\ 0 & \text{if} \quad i \text{ abstains} \\ \alpha\theta_i - c & \text{if} \quad i \text{ votes for L} \end{cases},$$

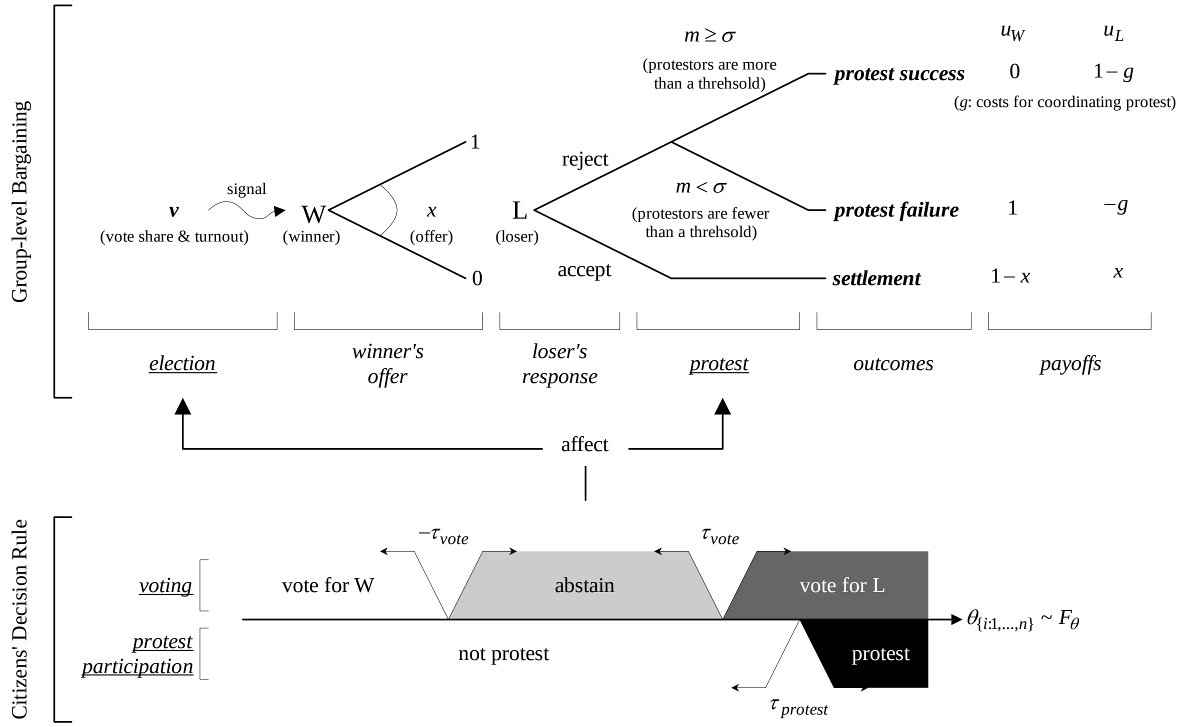
where $\alpha > 0$ represents the expressive value of voting and $c > 0$ is to the costs of voting. By the shortsighted-voter assumption, citizens decide their voting behaviors only based on $u_{i,election}$. Each citizen votes for W if $\theta_i < -\tau_{vote}$, abstains if $-\tau_{vote} \leq \theta_i \leq \tau_{vote}$, and votes for L if

[5] Note that relaxing the assumption usually requires other assumptions. For instance, while Little et.al (2015) model citizens as fully strategic actors, the model is tractable only when citizens' preferences are normally distributed.

$\tau_{vote} < \theta_i$, where $\tau_{vote} = \frac{c}{\alpha}$ is the threshold value for voting. After the election, the aggregated turnout rates and L's vote share $\boldsymbol{v} = \left(v_T, v_{L|T}\right)$ are publicly announced, where v_T is the turnout rate and $v_{L|T}$ is the fraction of loser votes in total votes. Thus, even though W and L cannot observe individual voting behaviors, the election provides new information of aggregated vote counts.

The election follows a standard bargaining protocol (the top of Figure 1). The winner first proposes an offer $x \in [0,1]$, which the loser either accepts or rejects. If L accepts the offer, W and L receive $1 - x$ and x respectively, and the game ends. The loser's rejection, by contrast, entails his call for protest. Each citizen joins the protest if $u_{i,protest} = \beta\theta_i - d > 0$, where β represents the expressive value of joining the protest and $d > 0$ is the cost of participation. Let i's threshold value for joining protest and the number of protestors be $\tau_{protest} = \frac{d}{\beta}$ and m respectively. If the number of protestors is greater than or equal to a "tipping point" σn, where $\sigma \in [0, 1]$, the protest succeeds, forcing W to accept the demand. This gives the payoffs $(u_W, u_L) = (0, 1 - g)$, where $g \in [0, 1]$ is L's costs for coordinating protest. When the participants are fewer than the threshold, the protest fails, giving payoffs $(u_W, u_L) = (1, -g)$. The bottom of Figure 3-1 summarizes the citizens' decision rules.

Figure 3-1. Election, Bargaining, and Protest



NOTE: The extensive form of the game (top) and citizens' decision rule (bottom). Individual citizens' attitudes to the loser $\theta_{\{i;1,\ldots,n\}}$ are drawn from a cumulative density function F_θ. In an election, citizens cast votes for W or L, or abstains, based on $\theta_{\{i;1,\ldots,n\}}$ and a threshold value $\tau_{vote} = \frac{c}{\alpha}$. After voting, the electoral outcomes $\boldsymbol{v}$ (vote shares and turnout) are publicly announced. The winner, who does not know the exact values of $\theta_{\{i;1,\ldots,n\}}$ but can observe the electoral outcomes, then decides an offer x to the loser. If the loser accepts the offer, the game ends with a negotiated settlement. If the loser rejects the offer, he calls for a protest by paying coordination costs g. Individual citizens join the protest if their $\theta_{\{i;1,\ldots,n\}}$ values are greater than a threshold $\tau_{protest} = \frac{d}{\beta}$. If the number of protestors m is larger than or equal to a threshold value σ, the protest succeeds, and otherwise, the protest fails.

Finally, I make several assumptions on substantive grounds. First, there should be a non-zero probability that there is at least one potential protestor: $1 - F_\theta(\tau_{protest}) > 0$, which is equivalent to saying that the citizens' costs for protest participation are sufficiently small $d < \beta F_\theta^{-1}(1)$. Otherwise, there would be no protest simply because joining a protest is too costly. This assumption is consistent with previous studies (Londregan and Vindigni 2006; Fearon 2011; Little, Tucker, and LaGatta 2015) and useful to limit the scope to substantively interesting cases. Second, I assume that L knows the number of potential protestors m while W does not. That is, parties usually have better information about its own core constituencies. This setup is also consistent with previous studies (Cheibub and Hays 2017; Londregan and Vindigni 2006). Third, I assume that the citizens' threshold for joining protest is higher than the threshold for voting for L: $\tau_{vote} \leq \tau_{protest}$. This assumption reflects the idea that joining a protest is physically more costly than just casting a vote.

Analysis

The game has a Bayesian perfect equilibrium that yields a positive probability of protest;

Proposition (Protest Equilibrium). When $g \leq F_{m|\boldsymbol{v}}(\sigma n)$, the following set of strategies are a part of a unique Subgame Perfect Equilibrium. W offers $x^* = 0$, which L accepts if $m < \sigma n$ and otherwise rejects. The probability of protest given electoral outcomes $\boldsymbol{v}$ is;

$$1 - F_{m|v}(\sigma n) \approx 1 - \Phi\left(\frac{(\sigma - v_T v_{L|T}\delta)n}{\sqrt{n v_T v_{L|T}\delta(1-\delta)}}\right)$$

where $\delta = \frac{1-F_\theta(\tau_{protest})}{1-F_\theta(\tau_{vote})}$. See SI 2-1 for proof.

That is, when the costs for initiating a protest (g) are relatively small, the winner needs to make a very large concession $x = 1 - g$ in order to satisfy the loser. Instead, the winner chooses a hardline offer $x = 0$, which the loser accepts only if he cannot gather a sufficient number of protestors. The winner's gamble, however, fails if $m \geq \sigma n$ so that a sufficient number of people are actually ready to protest against the winner.

Under this equilibrium, protest occurs when the winner is optimistic but the loser is actually able to gather σ or more protestors. The probability of protest given the electoral outcomes $\boldsymbol{v}$ is therefore $\text{Prob}(m \geq \sigma n | \boldsymbol{v}) = 1 - F_{m|v}(\sigma n)$. Importantly and rather counterintuitively, the protest probability increases with the turnout rate;

> **Proposition 2** (The Relationship between Turnout and Protest). Under the protest equilibrium, the probability of protest given $\boldsymbol{v}$ increases with the turnout rate v_t.
>
> See SI 2-1 for proof.

When the turnout rate is high, it increases the *variance* of the distribution of m; the loser vote share $v_{L|T}$ is now composed of a mixture of citizens of $\theta_i > \tau_{protest}$ (protestors) and those of $\tau_{vote} < \theta_i \leq \tau_{protest}$ (non-protestors who vote for L), which creates uncertainty about the number of protestors. Since the right tail of the probability distribution is increasing with its variance, the probability of protest $\text{Prob}(m > \sigma n) = 1 - F_{m|\boldsymbol{v}}(\sigma n)$ is also increasing with the turnout rate. Put simply, high turnout means noisy voices from disinterested people, which in turn "*drowns out*" the voices of the potential protestors, making it difficult for the winner to precisely estimate the number of protestors (that is, W has a less informative posterior belief).[6] Thus, an observable implication is;

> **Hypothesis** (Drowning-out Effect of Turnout). When the costs for protests are sufficiently small, elections with higher turnout result in higher probabilities of protest relative to those with lower turnout.

To facilitate our understanding, consider a stylized example of a village of 100 people. If more than 20 people protest, the chief must accept their demand. When voting costs are as large as those for protest, only those 20 villagers vote for the opposition, while the rest will vote for the chief or abstains. Thus, the chief can simply count the number of opposition votes in order to obtain the precise number of the protestors. With the precise estimate, the chief can make a satisfactory offer to the 20 dissidents so that they can avoid the protest. By contrast, when voting costs are much smaller than those for joining the protest, other villagers turn out and can potentially cast votes for the opposition. As a result, the chief can no longer simply count the opposition votes; she needs to make a guess about the number of protestors. When the chief understates the number of protestors, she will make an unsatisfactory offer, which in turn triggers a protest. The high turnout therefore drowns out the voices of the 20 dissidents and creates a probability of protest.

[6] Precisely speaking, turnout has another effect, which I call a *mean-shift* effect; high turnout means a larger number of government and opposition supporters, which in turn can increase the risk of protests. See SI 2-1 for detail.

Note that the drowning-out hypothesis pertains to *the variation within the protest equilibrium*. That is, the scope is limited to the cases in which the costs for protests (g) are sufficiently small such that $g \leq \Phi\left(\frac{(\sigma-\delta)n}{\sqrt{n\delta(1-\delta)}}\right)$,[7] including democracies and perhaps anocracies but almost certainly excluding autocracies. When g is larger, the equilibrium can be different. With the large costs for protest, the winner can easily accommodate the loser by offering a relatively small concession of $1 - g$. Thus, when a losing candidate receives a large number of votes and hence post-election protest is imminent, the winner rather opts to make a small concession and hence to eliminate any risk of protest. Since this prediction is not particularly new and indeed analyzed by previous studies (Pierskalla 2010; Bell and Murdie 2016), this paper examines a case of democracy so as to fix the value of g and hence to analyze the variation within the protest equilibrium. In the later section about causal mechanisms, I also investigate possible variation within the democracy.

It is also worthwhile to mention that the drowning-out hypothesis can hold even if one accounts for alternative signaling strategies, such as pre-election protests, letter writing, and phone calls. Pre-election protests are usually less than an optimal; as far as protests are relatively costlier than elections (even though costs for protests are small in absolute terms), opposition parties have incentives to wait and see the outcome of elections, and then, only if necessary, take to the streets (Little, Tucker, and LaGatta 2015). Moreover, although letter writing and phone calls might be effective, these signals are noisy as well; opposition parties can buy people to write letters or make phone calls, and given this incentive, these signals can be biased and thus cannot tell the exact size

[7] This inequality ensures that equilibrium condition $g \leq F_{m|\boldsymbol{v}}(\sigma n)$ is satisfied regardless of v_T.

of opposition supporters. Although this certainly does not mean that these signals do not provide any useful information or have no effect on protests, unless they could eliminate any uncertainty over the size of protestors, elections and turnout can still affect the remaining uncertainties.

In the model, I deliberately omit any strategic dynamics behind voting decisions, such as candidates' policy positions (Mayer 2007), clientelism and vote buying (Bratton 2008; Nichter 2008; Gans-Morse, Mazzuca, and Nichter 2014), voter intimidation (Klopp and Zuern 2007; Robinson and Torvik 2009; Kibris 2011), and pre-election violence (Blattman 2009; Dunning 2011; Koch and Nicholson 2016; Harish and Little 2017). Although this is a useful simplification (Clarke and Primo 2007) unless voting decisions would be *completely* determined by those endogenous factors, it still poses an empirical challenge; without identifying exogenous variations in voter turnout, we can hardly isolate the effects of turnout from the endogenous relationships.

RESEARCH DESIGN: AN INSTRUMENTAL VARIABLE ANALYSIS WITH THE NEAR-FAR MATCHING

Given the concerns with endogeneity, a critical question is how we can make the test closer to an ideal experiment. One way is to find an as-if randomly assigned variable, called an instrumental variable, that affects turnout. By restricting the variation of turnout to such exogenous variation, I can isolate the causal effect from any endogenous relationship. As an instrumental variable for turnout, American voting scholars use election-day rainfall deviation, measured as the amount of rainfall on an election day minus the average rainfall on the same day but in different years (Hansford and Gomez 2010). As widely recognized both in conflict and electoral studies, rainfall deviation can be considered as-if random and hence provides an opportunity for a natural experiment (Miguel, Satyanath, and Sergenti 2004; Afzal 2007; Ritter and Conrad 2016; Vanden

Eynde forthcoming). Importantly, because the instrument is election-day rainfall *deviation*, its variation cannot be explained by regional or seasonal conditions (Hansford and Gomez 2010).[8]

There is also a theoretical reason for using the instrumental variable approach. Consistent with the drowning-out effect, which is concerned of the turnout of *less motivated citizens* (those of $\theta_i \in [-\tau_{protest}, \tau_{protest}]$), the IV estimators are known to be local to the units whose turnout rates are sensitive to election-day rainfall deviation ("compliers"). As long as those weather-sensitive citizens are less interested in politics, the IV approach provides a theoretically relevant quantity than global estimators.

The instrumental variable analysis, however, does not yield the estimates of the causal effect without additional assumptions. One possibility is that rainfall deviation would affect the onset of protest except for its effect via turnout, and thus that we could not easily isolate the effect of turnout from the circumventing effects. In fact, rainfall deviations are shown to affect a variety of phenomena, including economic production (Miguel, Satyanath, and Sergenti 2004; Afzal 2007; Vanden Eynde forthcoming) and conflict itself (Ritter and Conrad 2016). Given these findings, it might be hard to assume that rainfall deviation would have no circumventing effects (the assumption called exclusion restriction).

[8] Because the rainfall deviation is a function of normal rainfall, one might think that the instrument is confounded by normal rainfall. Although this might be true for a particular observation, the expected value of rainfall deviation is independent of normal rainfall, as $\mathrm{E}\left[rain_{i,t} - \overline{rain}_{i,t}\right] = 0$ for any i. Indeed, in the following analysis, the average rainfall deviation is near zero (0.006 mm/h), and the correlation between the rainfall deviation and normal rainfall is 0.009 ($p = 0.32$).

At this point, Hansford and Gomez's approach is distinguished from other applications of rainfall instruments and perhaps offers something new to conflict studies.[9] Instead of using annual or monthly rainfall deviation, they propose *election-day* rainfall deviation as an instrumental variable. While rainfall deviation in general has broad effects, the effects of rainfall deviation on a very particular day should be fairly limited and thus less likely to violate the exclusion restriction. For instance, while excessive annual rainfall can substantially affect agricultural production, which can, in turn, affect protest risks, rainy weather on an election day cannot have such a large impact. Although election-day weather might directly affect protest on the polling day (Ritter and Conrad 2016), election-day protests are usually prohibited in democracies, and indeed they are extremely rare in the case of India.[10]

The other possibility is that election-day rainfall has no tangible effect on turnout. If this were the case, the instrumental variable would be irrelevant and could tell us nothing about the effect of turnout. Furthermore, even when an instrument has a statistically significant effect but the predictive power is weak, the weak instrument still produces large bias and makes the conventional estimator (two-stage least square: TSLS) extremely sensitive to small errors.[11] A

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[9] A potential exception would be Ritter and Conrad (2016), who use daily, but not election-day, rainfall as an instrument *for violence* and estimate the effect of violence on repression in African provinces. Moreover, their rainfall variables are absolute amounts of daily rainfall and its percentage share in annual rainfall.

[10] I also conduct placebo tests. See the later section about robustness checks.

[11] The other required assumptions include the stable unit treatment value assumption (SUTVA) and the monotonicity (Angrist, Imbens, and Rubin 1996). The monotonicity assumption can

powerful instrument, by contrast, is robust to minor violations of non-random assignments (Angrist, Imbens, and Rubin 1996; Stock, Wright, and Yogo 2002).

The weak instrument however can be a real problem for the election-day rainfall instrument. From one perspective, election-day rainfall appears to increase the physical costs of voting and hence depress turnout rates (Hansford and Gomez 2010). The shift in voting costs however may be negligible (Persson, Sundell, and Öhrvall 2014). On the other hand, election-day rainfall can also decrease the opportunity costs of voting; people may have fewer things to do on a rainy day, or they may leave work early. People can spend the extra hours voting (Lind 2014, 2015; Kang 2015). In fact, recent findings differ across countries (Artés 2014; Arnold and Freier 2016; Meier, Schmid, and Stutzer 2016). Although this paper is not intended to settle this dispute in electoral studies, these studies do provide theoretical reasons to suspect that the rainfall instrument is actually weak.

Methodological Challenge: Strength – Clarity Tradeoff

Given the potential for weak-instrument problems, one might resort to using flexible regression functions at the first stage so as to strengthen the predictive power of the instrument (Newey and Powell 2003; Chesher and Rosen 2017). This approach, however, usually makes the estimate sensitive to the specification of functional forms and requires additional, often highly

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potentially be violated in this study, but the assumption can be relaxed (de Chaisemartin Clément 2017). Regarding the SUTVA and spatial correlation, see the later subsection *Instrumental Variable*.

demanding, assumptions (Marshall 2016). Moreover, the regression approaches can blur the subjects of comparison; we are not sure which units we are really comparing to which units.[12]

A common solution to the functional-form dependency is matching (Ho et al. 2007). Combining matching and an instrumental variable analysis is, however, not as straightforward as one might expect. In fact, matching can actually worsen the very problem of weak-instrument bias. Conventional matching methods restrict the comparison to similar units, but these units tend to have similar values in an instrumental variable as well. Furthermore, while many applications of instrumental variables involve continuous instruments, most matching algorithms can only be applied to binary treatment.

Method: Near-far Matching for Instrumental Variable Analysis

A recent refinement of an instrumental variable design, proposed by Baiocchi et al. (2010, 2012) in statistics and more recently Keele and Morgan (2016) in political science, provides a design-based solution to the weak instrument problems. Their insight is that we can explicitly incorporate the strength of an instrumental variable into the framework of matching and hence to optimize both the instrument strength and covariate balance. Because the method is based on matching (Ho et al. 2007), it not only enhances the predictive power of an instrument but also makes the causal comparison more explicit and less reliant on functional form assumptions (Baiocchi et al. 2010, 2012; Keele and Morgan 2016). Furthermore, since the matching method is

[12] I also use TSLS with flexible first-stage regressions in a robustness check. See the later section about robustness checks. The main analysis does not use this approach.

built upon a non-bipartite matching algorithm (Lu et al. 2011), it can fully accommodate a continuous instrumental variable and thus can be used in a greater variety of applications.

An intuition behind the method is that instead of comparing all units at once, it is better to compare units that are similar in covariate values but *different* in the values of an instrumental variable.[13] Because they are different in the values of the instrument, their turnout rates are also expected to be different as well, indicating a more powerful comparison. The near-far matching creates those pairs by "penalizing" units that have similar values in the instrument. In particular, the matching is done with a penalized distance metric;

$$w_{ij}^* = \begin{cases} w_{ij} + ce^{-|d_{ij}|} & \text{if} \quad |d_{ij}| \leq \tau \\ w_{ij} & \text{otherwise} \end{cases},$$

where i, j denotes two units in a sample, w_{ij} is the rank-based Mahalanobis distance, which is robust to outliers (Rosenbaum 2009; Keele and Morgan 2016), and d_{ij} is the difference in the values of an instrumental variable. The size of penalty c is usually set to a large integer (Rosenbaum 2009; $c = 1000$ in this study), and the fraction of the penalized units τ is selected by a grid-search (in this study, $\tau = 0.3$).[14] Using the penalized distance metric w_{ij}^*, a non-bipartite

[13] Keele and Morgan (2016) applies the method to replicate Hansford and Gomez (2010). To my best knowledge, I am not aware of other studies in political science that apply the near-far matching.

[14] For the detail of the grid-search, see SI 2-6. Note that as far as the penalty function is sufficiently flexible, nearfar matching is robust to the specifications of the function (Rosenbaum 2009).

matching algorithm (Lu et al. 2011) creates pairs that minimize the distances.[15] Once the matching is done, we assign a "treatment" status $T_i = 1$ if its rainfall deviation is larger than its counterpart in a pair, and otherwise give a "control" status $T_i = 0$.[16]

Because the matching creates a dichotomous instrumental variable (T_i), we can use a variety of estimators that are more powerful and robust, and require fewer assumptions than the TSLS and its cousins.[17] Following the literature (Andrews and Marmer 2008; Baiocchi et al. 2010; Keele and Morgan 2016), I use a Hodge-Lehmann (HL) non-parametric estimator, which is more powerful than the Anderson-Rubin semi-parametric estimator (1949) commonly used in economics (Andrews and Marmer 2008). Intuitively, the HL estimator is derived from a series of hypothesis tests regarding a causal quantity λ:

$$\left(Y_{i:T_i=1} - Y_{i:T_i=0}\right) = \lambda\left(D_{i:T_i=1} - D_{i:T_i=0}\right),$$

where $Y_{i:T_i=1}$ and $Y_{i:T_i=0}$ are the outcome variables (the onset of protest) when unit i is treated and not treated respectively, and $D_{i:T_i=1}$ and $D_{i:T_i=0}$ are the corresponding explanatory variables (turnout rates). The parameter λ denotes the causal effect. Under the set of the assumptions which I described above, the causal quantity is estimated by $\hat{\lambda}$;

[15] The algorithm drops several outlier observations that can potentially worsen the quality of covariate balance. The fraction of the dropped observations (ρ) is another tuning parameter that requires a grid-search (in this study, $\rho = 0.1$). See SI 2-6.

[16] Although dichotomizing an *instrumented* variable (turnout) can introduce biases (Marshall 2016), such biases do not arise when we coarsen an *instrumenting* variable (rainfall deviation).

[17] In the later robustness check, I also report the TSLS estimates.

$$\left(Y_{i:T_i=1} - Y_{j:T_j=0}\right) = \hat{\lambda}\left(D_{i:T_i=1} - D_{j:T_j=0}\right),$$

where i, j are treated and control units that are paired by the matching. The point estimate $\hat{\lambda}$ and its confidence interval are obtained by conducting a series of the non-parametric tests for $\hat{\lambda} = \lambda_0$ (Wilcoxon signed-rank sum tests) and retaining the value of λ_0 that is not rejected with the highest *p*-value or those not rejected at a 5% significance level. The non-parametric estimator is particularly advantageous for this study as the outcome is binary and hence linear models are "worrisome" if not misleading (Baiocchi et al. 2010, 1293). Note also that the HL estimate is concerned with pairwise differences, which are analogous to having $\frac{n}{2}$ pair-specific fixed effects in a linear regression model.

CASE AND MEASUREMENT

I apply the near-far matching IV method to the case of Indian State Assembly elections. India is one of the quintessential examples in which long-standing democracy and violent and non-violent protests coexist. Given the availability of data about the protest, rainfall, and covariates, the sample includes all assembly elections between 2000 and 2015.[18] Although I could conduct a similar analysis for the National Parliamentary elections, the area size of the parliamentary constituency is relatively large, and thus the sample size is fairly limited (around 500 units). The following analysis therefore consists of over 4,300 constituencies[19] in the 94 State Assembly elections between February 2000 and November 2015. A majority of the states have two or three

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[18] The geocoded census data are available only after 2000, and the latest data for the other variables are available until 2015.

[19] 4,375 and 4,369 constituencies before and after the 2008 delimitation respectively.

elections in an interval of five years.[20] The overall sample includes 12,597 election-constituency observations (before matching).[21]

In the period of 2000 – 2015, the Indian economy experienced rapid growth, and the political situation in the central government had been relatively stable. The elections in this period are generally free and fair backed by the long history of democracy. In fact, the Election Committee of India is believed to be one of the most competent public institutions in India (Banerjee 2007). The State Assembly elections are held under the first-past-the-post rule. The Election Committee of India decides polling dates one to three months ahead of an election. Depending on the size of a state, they create one to seven groups of constituencies and assign a different polling date to each group. All polls are usually done within a month. Due to heavy rain in the monsoon season (June – September), a majority of the elections are held in dryer months. Note that although these selection processes may indicate a non-random assignment of rainfall amounts, this study uses election-day rainfall *deviation* as the instrument, which is beyond the control of the election

[20] Because the interval between elections is long and the dataset is a wide but short panel, I consider the dataset is as-if cross-sectional. Clustering the standard errors for each constituency does not change the results. See SI 2-8.

[21] Because the sample size is large and thus it is computationally too expensive to apply the near-far matching algorithm to the entire sample, following Rosenbaum (2009), I subset the sample to groups of elections that were held at the same period, apply the matching within each group, and pool the matched observations. I apply the matching to each election group instead of each election, as some states, including Tripura, Mizoram, Meghalaya, Sikkim, and Union Territories, are too small for the matching. Table A2-5-1 in SI 2-5 is the list of the election groups.

committee.[22] In fact, the IV design is particularly useful for the case of India, in which clientelism, parochialism, and vote buying are persistent and hence turnout is hardly exogenous.

Outcome Variable

The outcome variable is the onset of protests after an election, which is derived from the Integrated Crisis Early Warning System (ICEWS; Boschee et al. 2015). The ICEWS dataset is a machine-coded event data based on over 38 million multilingual (including Hindi) news stories all over the world. The ICEWS dataset is even accredited as "the current gold standard for event data" (Metternich et al. 2013, 901).[23] Moreover, the ICEWS dataset has finer classification of protest types, including demonstration, hunger strikes, strikes, obstruction, and riots, which allows me to test underlying causal mechanisms (see the later section about causal mechanisms). My outcome variable takes a value of 1 if there is at least one event of a protest within one-year after a polling day (excluding the polling day) but not one-year before the polling.[24] Since ongoing protest and its cessation are conceptually different from the onset of new protests,[25] following the standard

[22] The postal votes are limited to service and other special voters (less than 1%) and repolling is also extremely rare.

[23] For details of the ICEWS dataset, refer to O'Brien (2010).

[24] I also use different time windows. See the later section about robustness checks.

[25] Although my model assumes the existence of *conflict of interests* before an election, it considers the situation in which the conflict is not yet materialized to protest before the election.

definition of “onset” (Sambanis 2004), I exclude the observations of pre-election protest. In total, the sample contains 529 onsets of protests (4.20% of the sample; before matching).[26]

It is worthwhile to note that alternative datasets of protests are limited in the case of India. Currently, Varshney-Wilkinson dataset about Hindu-Muslim riots (Varshney 2003; Wilkinson 2006) are available only up to 2000 at the level of districts. For this period, Indian democracy is not always stable, and the rainfall data are unavailable. Furthermore, given the potentially large costs of participation in riots, including the risks of arrest and detention, riots are unlikely to be suitable for testing my hypothesis (as the scope of the hypothesis is limited to protest with relatively small costs; see the later section about causal mechanisms). Similarly, the Uppsala Conflict Data Program Georeferenced Event Dataset (Sundberg and Melander 2013) has data about violent events but not non-violent protests. The Armed Conflict Location and Event Data (Raleigh et al. 2010) has records about protests and riots in India but only after 2016, for which period the rainfall and covariates are not available. They also do not distinguish violent and non-violent protests. Finally, the Social Conflict Analysis Database (Salehyan et al. 2012) or Nonviolent and Violent Campaigns and Outcomes (Chenoweth and Lewis 2013) dataset has no data for India.

Another issue is a potential for reporting biases (Weidmann 2016). Because the outcome variable (ICEWS) rests on media reports and hence is susceptible to reporting biases, the *instrumental* (not explanatory) variable must be free from such errors. Otherwise, if *both outcome and instrumental variables* would be contaminated by reporting biases, the causal estimate could

[26] The data summary and their map are in SI 2-4.

also be biased.[27] The satellite-based data of rainfall, which are obtained regardless of media coverage, provides a way to guard against such a possibility. In a later robustness check, I also conduct a placebo test to assure that the main findings are not explained by reporting biases.

Explanatory Variable

The explanatory variable is the turnout rate, measured as the total number of votes divided by the total number of eligible citizens. The data are scraped from the *Statistical Report on General Election* published by the Election Committee of India. The average turnout rate is high (0.69) in India. The dataset also contains other fine-grained information, including polling dates, valid votes, gender ratios in each vote count, and the number of polling stations.

Instrumental Variable

The instrumental variable is the polling-day rainfall deviation, which comes from the Climate Prediction Center Morphing technique (CMORPH) satellite images. The images are available every thirty minutes from 1 January 2000 to 2015 at a spatial resolution of eight-by-eight kilometers. The CMORPH products are created from sensors of multiple satellites (Xie et al. 2011; Joyce et al. 2004).[28] The normal rainfall is estimated by the average rainfall amounts five days around the date of a polling day but in different years.[29] The polling-day rainfall deviation is the difference between the observed and normal rainfall amounts. The average rainfall deviation is, as

[27] See SI 2-11 for detailed discussion.

[28] For details of the rainfall measurement and possible alternative data sources, see SI 2-2.

[29] See SI 2-2 for details.

expected, near zero (0.006 mm/h). To be sure, there is no observation of extreme rain that would cause floods or other natural disasters.

A possible issue for the rainfall deviation measure is spatial correlation (Hansford and Gomez 2010). This study guards against such a possibility both by design and method. Because the polling dates are different even within a single State Assembly election in India, rainfall deviations should be less correlated even within a state. Furthermore, the near-far matching and the HL estimator exploit the variation *within* matched pairs. Since the matched pairs are expected to have different values in the rainfall deviation, they are unlikely to be neighboring constituencies. Thus, the near-far matching not only strengthens the instrument but also provides a non-parametric way to address spatial dependency.

Covariates

The covariates are selected by using Keele and Morgan (2016) as a baseline and considering unique characteristics of India. They include logged population, the proportion of Muslims, scheduled tribes and castes,[30] urban population, and farmers[31] recorded in the 2001 Census of India.[32] Although these covariates are not always necessary for making a valid inference,

[30] The scheduled castes and tributes are groups that are historically disadvantaged and hence protected by the Constitution of India.

[31] I include the indicator of farmers to account for rural non-farmers (e.g. livestock raiser).

[32] For detail of the covariates, see SI 2-3.

they provide additional power to the analysis and guard against possible violations of the IV assumptions.[33]

RESULTS

The following table (Table 3-1) shows the estimates of the effect of turnout rates on the onset of protest and its 95% confidence intervals. Consistent with the drowning-out hypothesis, the result shows that turnout indeed increases the risk of protest in the subsequent period. The point estimate 1.54 indicates that if a turnout rate increases by 1%, it increases the probability of subsequent protest by 1.54%. Given the rarity of protest (only 4.20% of the sample), the effect size is not small; compared to the sample average, a unit has 1.37 times higher probability of protests if its turnout rate is 1% higher.

Table 3-1. The Effect of Turnout on the Onset of Protest

Point Estimate	95% Confidence Interval
1.54	[0.45, 3.15]

NOTE: The Hodge-Lehmann estimate. In the near-far matching, 10% of observations are discarded for the purpose of better balance. $n =$ 10,330.

As reported in SI 2-7 (Table A2-7-1), the near-far matching improves both the covariate balance and the instrument's strength, giving further credence to the above finding. While the covariates are somewhat imbalanced without the near-far matching, probably due to finite-sample errors, the near-far matching properly adjusts the remaining imbalances. Furthermore, even though the power of the instrument measured by the first-stage F statistic is 2.5 before matching, the near-far matching raises it to 28.4, which is far above the conventional criterion of 10 (Stock, Wright,

[33] The summary of the data is provided in Figure A2-4-1 in SI 2-4.

and Yogo 2002). The covariate balance and stronger instrument mean that the above finding is robust to subtle violations of the random-assignment assumption.

On average, an upward deviation in election-day rainfall has an effect to increase turnout rates.[34] The treated observations have a 0.8% higher average turnout rate than the control observations (with a corresponding confidence interval $[0.3\%, 1.3\%]$). While this finding is contradictory to the explanations based on physical costs (Hansford and Gomez 2010), it is consistent with those based on opportunity costs (Kang 2015; Lind 2015, 2014). That says, this paper is not intended to evaluate these explanations in electoral studies. Since the existing findings are different across countries, future studies may need to look at multiple countries and explore institutional and cultural conditions.

CAUSAL MECHANISMS AND ROBUSTNESS CHECKS

Although rigorously testing every step of the bargaining model is difficult and, generally speaking, testing a theoretical model is impossible (Clarke and Primo 2007), it is still important to examine possible causal mechanisms. To this end, I theoretically consider possible alternative explanations and test their observable implications. In supporting information, I also conduct a focused case study of the Singur protest in West Bengal to illustrate, if not test, the causal mechanism.[35]

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[34] On average, the treated observations have 0.077 mm/h more precipitation than their normal amounts, while the control units have 0.055 mm/h less precipitation than usual.

[35] See SI 2-14.

Causal Mechanisms I: Coordination or Enthusiasm?

While my theory is primarily concerned with inter-group strategic dynamics, there may be alternative mechanisms that are based on within-group and individual-level dynamics. First, it might be possible that active electoral participation would reduce the opposition's costs for coordinating protest. If more people turn out in elections, it might allow the opposition groups to communicate with those voters without costs for travelling. Moreover, the opposition groups may also learn their dire electoral prospect from high-turnout elections, making the mobilization for extra-electoral means relatively cheap. Formally, high turnout might reduce the coordination costs (g in my model), which in turn makes the protest equilibrium more likely. From this viewpoint, high turnout is expected to have particularly strong effects on *protests that require greater efforts of coordination in normal circumstances, such as strikes and obstruction*. Only when the opposition groups have difficulties to coordinate protests, electoral participation could possibly alleviate the problems and hence alter the likelihood of protests. Strikes require collective actions for preventing strikebreakers, and obstruction also requires detailed planning for effective deployment of members and barricades. Only when opposition groups is capable of coordinating individual actions, these types of protest can serve to their purposes.

Second, at an individual level, active electoral participation might wake up the public to politics and even create political enthusiasm, which in turn might result in an intense atmosphere and uncontrollable momentum of escalation (Letsa 2016). More formally, the enthusiasm can be conceptualized as an emotional force that makes individual people dismissive about the physical costs of joining protests. From this perspective, high turnout might reduce people's subjective costs for joining protests (d in my model), which in turn might increase the probability of protest. If this could explain the result, we should therefore see similar or even stronger effects on *protests* *that are physically costlier for participants, such as riots and hunger strikes*. Only when joining a protest is physically costly, the enthusiasm can alter people's decisions; otherwise, people would always or never join the protest. In India, rioters can be arrested for the offence against public tranquility or for injury (Chapter VIII in Indian Penal Code). Fasting for several days or even months is also not something physically tolerable for ordinary citizens.

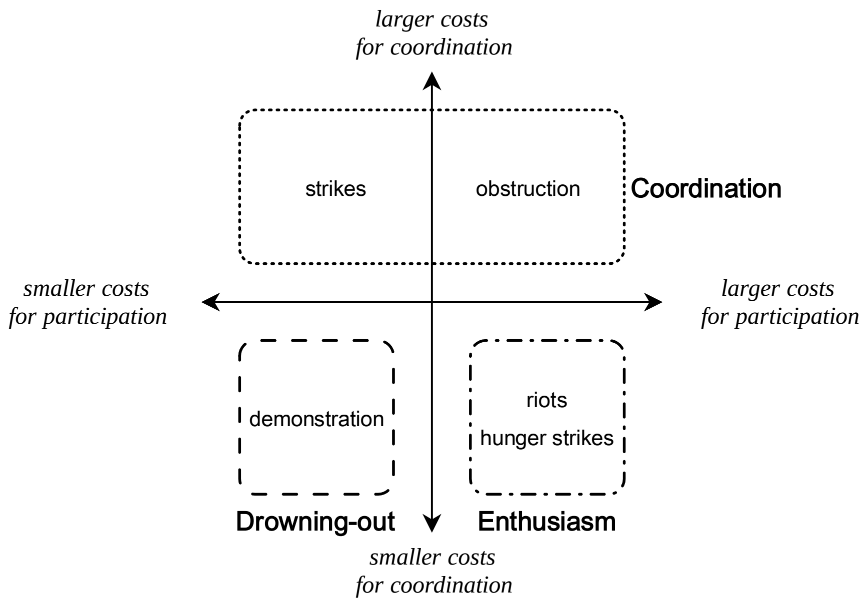
By contrast, the scope of my hypothesis is limited to the cases in which the costs for coordination and participation are relatively small. As I mentioned in the theory section, only when the opposition group can relatively easily coordinate protest, buying off the opposition becomes too expensive for the government such that she is rather willing to take a risk of protests (equilibrium condition).[36] Moreover, my model also assumes that the individual costs for joining protests are fixed and affordable for at least one citizen; otherwise, protests do not occur simply because none will join the protests. Thus, I expect that the drowning-out effect is particularly relevant to *protests that do not require large costs for coordination or participation, such as demonstration*. In democracy, the freedom of assembly warrants the political right for participating in non-violent protest without fear of arrest or repression. Moreover, demonstration requires relatively small costs for coordination; in many circumstances, people attend demonstration for their own purposes (such as expressing their opinions) even without much organizational coordination. [37] Thus, although organizational support might be helpful for sustaining

[36] See the theory section for details.

[37] For instance, in the Singur conflict, the first protest (1 June 2006) occurred before the Krishji Jami Raksha Committee, an organization of the opposition groups, was formed (3 June 2006).

demonstration, it is not a necessary condition for its onset. The following figure (Figure 3-2) summarizes the causal mechanisms and their predictions.

Figure 3-2. Causal Mechanisms and Protest Types



NOTE: The figure shows the causal mechanisms and corresponding types of protest. The vertical and horizontal axes indicate the opposition group's costs for coordinating a protest (g in my model) and the individual costs for participating in a protest (d in my model) respectively. The intersection of the axes is not an origin. The **bold** letters are the labels of causal mechanisms. The *italic* letters are the polar values in the vertical and horizontal axes. The non-bold letters are the types of protests. The position of each protest type in the figure should be considered as its mean value. I put strikes in the category of small participation costs as the labor right of collective action is guaranteed in democracy. I put obstruction in the category of large participation costs as participants can potentially be arrested. These classification however does not alter the argument of this paper.

As seen in Table 3-2, the empirical results are supportive for the drowning-out hypothesis but not for the coordination or enthusiasm mechanism. Not only are the effects of turnout on strikes, hunger strikes, obstruction, and riots weaker than that of demonstration, their effect sizes are very small; the point estimates are less than one-tenth of the main estimate, and even their upper bounds are less than a half of the main estimate. In my theory, these results are not surprising as participants of strikes, hunger strikes, obstruction, and riots (such as unions, politicians, tribes, and ethnic minorities) are usually well predictable even before elections, and hence there is less uncertainty that turnout could possibly change. By contrast, the effect on demonstration is very similar to that on protest in general (1.54). Thus, even though the within-group and individual-level explanations can be useful in other contexts such as civil war (Letsa 2016), they do not explain the main finding of this paper.

Table 3-2. The Effects of Turnout on the Onsets of the Different Types of Protest

	demonstration	strike	obstruction	riot	hunger strike
Point estimate	1.52	0.10	0.07	0.00	-0.13
95% CI	[0.53, 2.97]	[-0.45, 0.70]	[-0.37, 0.56]	[-0.55, 0.56]	[-0.51, 0.21]

NOTE: The second row presents the Hodge-Lehmann estimates of the effects of turnout rates on the outcomes denoted at the first row. The third row shows the corresponding 95% confidence intervals. $n = 10{,}330$.

Causal Mechanisms II: Party Victories

Yet another explanation can be electoral victories. That is, since different parties have different policies, if turnout would alter the winning probabilities of parties, it might also influence the risks of post-election protests. As seen in Table 3-3, however, I do not find any evidence that high turnout affects the winning probabilities of the two major parties' or other minor parties' victories in India. Although this does not indicate that turnout has "no" effect on winning probabilities, the party victories do not explain the main results of this paper.

Table 3-3. The Effects of Turnout on Party Victories

	INC victory	BJP victory	Other's victory
Point estimate	-1.13	0.98	0.15
95% CI	[-3.57, 0.88]	[-0.73, 3.13]	[-2.00, 2.30]

NOTE: The second row presents the Hodge-Lehmann estimates of the effects of turnout rates on the outcomes denoted at the first row. The third row shows the corresponding 95% confidence intervals. INC: Indian National Congress. BJP: Bharatiya Janata Party. $n = 10{,}330$.

Causal Mechanisms III: Loser's Electorate Share

Finally, although this paper is substantively interested in the effects of electoral participation, my formal model also predicts that $v_{L|T}$ (loser's vote share) and $v_T v_{L|T}$ (loser's electorate share) affect the risk of protests as well. Not only high turnout per se but also a larger number of votes for a loser make it difficult for the winner to sort out uncommitted citizens from committed dissidents who can potentially protest. Although the effect of $v_{L|T}$ alone is not easily identifiable as neither rainfall deviation nor other instrumental variables of which I am aware can explain this term, rainfall deviation can affect $v_T v_{L|T}$ (rainfall deviation affects v_T and hence $v_T v_{L|T}$ as well).[38] Thus, as an additional analysis, I replace the explanatory variable by $v_T v_{L|T}$ and re-estimate the instrumental variable model. The following table (Table 3-4) indeed shows that the higher the loser's electorate share is, the more likely the post-election protests are. On average, if $v_T v_{L|T}$ increases by 1%, it increases the probability of a subsequent protest by 2.68%, which is even larger than the effect of turnout on protests.

[38] To be clear, $v_T v_{L|T}$ is not an interaction term.

Table 3-4. The Effect of Loser's Electorate Share on the Onset of Protest

Point Estimate	95% Confidence Interval
2.68	[0.76, 6.64]

NOTE: The Hodge-Lehmann estimate. In the near-far matching, 10% of observations are discarded for the purpose of better balance. $n =$ 10,330. The first-stage F statistic is 14.59.

Robustness Checks I: Exclusion Restriction

I also conduct a series of robustness checks. First, if an upward deviation of polling-day rainfall were to *increase* the risks of protest on the same day, and if the protest risks are positively autocorrelated over time, it would violate the exclusion restriction and thus invalidate the causal inference.[39] However, polling-day protest is prohibited in India and hence extremely rare (only 5 cases: <0.001% of the sample). In fact, a falsification test provides no discernible relationship between the treatment and the onset of polling-day protest.[40]

Similarly, if rainfall deviations positively correlate over time, and if rainfall would *increase* the risks of protest after elections, it would also create a spurious relationship. To explore this possibility, I analyze the effects of turnout on protests in different post-election periods. Since a rainfall deviation in a single day is unlikely to predict a rainfall deviation several months later, I would be more confident if turnout has impacts on protests several months after polling. I find the similar effects for the periods from the three to nine months after elections, and the effects are statistically indistinguishable from zero in the other periods. Theoretically, this result makes sense

[39] If upward rainfall deviations would decrease protest risks, the estimate would be biased towards negative, and hence the true causal effect would be even stronger.

[40] See Table A2-9-1 in SI 2-9.

because it will take several months until a government devises new policies, and also because the information revealed by the elections will also become obsolete as the time passes.[41]

Robustness Checks II: Reporting Bias

If *both* the protest and rainfall measures would be contaminated by reporting biases, it could bias the causal estimate. Although I do not find compelling reasons for reporting biases in the satellite-based rainfall data, I also conduct a falsification test by regressing the incidence of protests one-year *before* polling on election-day rainfall deviation. If there is no systematic reporting biases in the rainfall data, there should be no relationship between rainfall and past protests. By contrast, if anomalous rainfall would be associated with reporting biases, we should see a correlation between election-day rainfall and pre-election protests. The placebo test however shows no association between the instrument and pre-election protests.[42]

Robustness Checks III: Miscellaneous

First, I use a count of post-election protests as an outcome variable to account for the effect of turnout on the intensity of protests, finding that a high turnout rate also increases the number of post-election protests.[43] Second, because the sample includes several states that are distinct in many respects, such as Jammu and Kashmir, Bihar, and Arunachal Pradesh, I conduct an analysis

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[41] See Figure A2-10-1 in SI 2-10. In SI 2-14, I also qualitatively examine whether three to nine months are reasonable estimates.

[42] See Table A2-11-1 in SI 2-11

[43] Unfortunately, the ICEWS does not have information about the number of protestors.

while dropping each of the states in India. The results are robust to the omission of each state.[44] Third, I also find similar results when I drop several constituencies that have large values in the average rainfall deviation.[45] Fourth, I also estimate the causal effects, using the TSLS and Anderson-Rubin estimators with and without the nearfar matching. Although the results of the regression-based methods are sensitive to functional-form specifications, I find consistent results.[46] Fifth, I also conduct the near-far matching with the second best tuning parameters, which entails the omission of 40% of the observations. Despite the large number of discards, the main results hold.[47]

CONCLUSION

In this paper, I argue that high turnout in free and fair elections has an adverse effect on efficient conflict resolution. A simple extension of bargaining theory indicates that high turnout can drown out the voices of potential protestors, make it difficult for the government to precisely estimate the size of the dissenters, and thus create a positive probability of protest. Because turnout is considered to be endogenous to protest, I use election-day rainfall deviation as a source of exogenous variation and apply the near-far matching IV method to make a more explicit and robust causal comparison. The analysis shows that higher turnout indeed increases the risks of protest after the elections.

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[44] See Figure A2-12-1 in SI 2-12.

[45] See Figure A2-13-1 in SI 2-13. Also refer to SI 2-4.

[46] See Table A2-8-1 to A2-8-4 in SI 2-8.

[47] See the end of SI 2-6.

Does this finding imply that democracy is imperfect as a conflict resolution mechanism? The answer is unfortunately yes if democracy refers only to *electoral* democracy, which is characterized by free and fair elections. As long as universal suffrage warrants every eligible citizen, whether they are interested in politics or not, to cast ballots, the vote counts may not reflect the preferences of potential dissenters. The gap between the preferences of the electorates as a whole and those of motivated minorities can lead to socially inefficient outcomes, such as protest.

This however does not necessarily mean that democracy in general is undesirable as a conflict resolution mechanism. In fact, although this paper does not explicitly incorporate, it is possible that protest allows people to non-violently express their opinions and demonstrate their leverage, and hence that protest has an effect to mitigate violent outcomes. This implies that even though high-turnout elections imperfectly reflect the preferences of motivated minorities and lead to periodic social disturbances, such disturbances may help to compensate for the elections' deficiencies and lead to policies that better address the needs of what would otherwise be under-represented populations. Thus, even though elections are imperfect as a conflict resolution mechanism, *the freedom of assembly* can compensate the electoral imperfection. It is a task of future studies to extend the current framework to armed conflicts and hence to provide further insights about democracy and conflict. It is also a promising avenue for future research to extend the current analysis to different electoral systems, such as proportional representation.

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Chapter 4. It Never Rains But It Storms: Armed Conflict and Maritime Piracy as Strategic Substitutes

On November of 2016 when the monsoon storms finally left the Gulf of Guinea, the Nigerian Delta insurgents—who had engaged in battles, looting, and violence against civilians during the summer—returned to the ocean. While the number of reported pirate attacks per month decreased by two thirds from the spring to the summer,[1] the attacks doubled at the end of the year (Coggins 2012). The resurgence of piracy, however, contrasted with the lull of violence in the Nigerian Delta; although 13 armed conflict events (2.36 per month) were reported in the summer of 2016, the violence ceased in the following months. Seeing the seasonal patterns, a witness mentioned an "inverse correlation" between the violence and piracy; "at the tactical level, the 'attackers', when not employed in militancy, oil theft, illegal bunkering or gang warfare, engage in piracy to cover some of their funding needs" (Center for International Maritime Security 2017).

The case of the Nigerian Delta insurgents suggests that weather conditions can affect conflicts in different spaces. The ocean climate may affect not only maritime piracy, but also the use of political violence on the ground. The potential for the cross-spatial effects casts doubt on conventional approaches that analyze the effects of weather conditions on armed conflict and piracy *separately*. If ocean weather would affect parties' choices over armed conflict and maritime piracy, and if the ocean and ground weather conditions correlate with each other, we need to

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[1] The reported attacks per month are 3.11 for the period of 2016-01-01 to 2016-05-14 (14 attacks in total), 1.09 for the period of 2016-05-15 to 2016-10-31 (6 attacks in total), and 2.00 for the period of 2016-11-01 to 2016-12-31 (4 attacks in total). The spring, summer, and winter periods are based on Center for International Maritime Security (2017).

analyze their effects on armed conflict and piracy with a single framework. Otherwise, as I argue in this paper, the separate analyses can suffer the problems of omitted variable biases.

The problem of correlating weather is not limited to conflict studies, but it can potentially be relevant to any study that exploits weather as a source of exogeneity, such as those in electoral studies (Hansford and Gomez 2010) and economics (Dell, Jones, and Olken 2012). Rather counterintuitively, when correlating weather conditions can affect outcomes of interest, a seemingly valid regression of an outcome on a weather condition can yield a bias without accounting for the inter-weather correlations. This means that we need to theoretically and empirically consider whether correlating weather conditions affect our outcomes. In this paper, I examine the problem in case of the effects of rainfall shocks and ocean wind speed on armed conflict and maritime piracy, which have drawn increasing attention from scholars (Hsiang, Meng, and Cane 2011; Axbard 2016).

In fact, theories of strategic substitution (Most and Starr 1984; Morgan and Palmer 2000; Palmer, Wohlander, and Morgan 2002; Clark and Reed 2005; Clark, Nordstrom, and Reed 2008) imply that the inter-weather correlation does matter. When violent actions and maritime piracy constitute strategic substitutes, rebels compare their *relative* costs and benefits to decide their actions. Thus, it is quite possible that windy ocean inhibits rebels from engaging in piracy, makes violent activities on ground relatively more attractive, and thus changes the likelihoods of both violence and piracy. Similarly, rainy weather can increase the costs for violent activities, make maritime piracy relatively cheaper, and hence affect both violence and piracy. Even though the mere existence of the substitutions may not prevent us from conducting separate analyses about wind-piracy and rain-violence links, as I demonstrate in this paper, the substitution coupled with the inter-weather correlations can induce biases in causal inference.

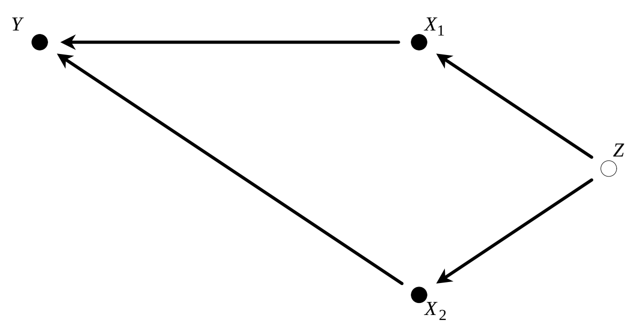
Theories of substitution, however, are not free from limitations. Previous studies rather narrowly focus on the *presence or absence* of substitution, without distinguishing *different types* of substitution. Building on a formal model, I argue that understanding types of substitution is critical and that there exist at least three different types of substitution. When piracy is an *upward substitute* for violence, piracy is more profitable than violence. In this case, it is shown that rebels engage in piracy whenever piracy is low cost (calm ocean), while they resort to violence only when piracy is infeasible (rough ocean) and violence is relatively cheap (sunny weather). By contrast, the situation is reversed when piracy is expected to yield less profit than violence and hence piracy is a *downward substitute* for violence. That is, while the incidence of violence depends only on its own costs (ground weather), the incidence of piracy is conditional on its own costs (ocean weather) and the costs for its alternative strategy (ground weather). Finally, when the returns from the two options are similar, piracy becomes an *equivalent substitute* for violence. In this case, the incidences of piracy and violence depend on the relative costs of these strategies and hence are affected by both ground and ocean weather conditions. Thus, depending on which type of substitution exists, we expect different empirical patterns.

I test these predictions by analyzing a *daily* panel of armed conflict, maritime piracy, ocean wind speed, and ground rainfall for the period of 2001-2016 in 30 coastal conflict countries. The results are the most consistent with the upward substitution hypothesis; while windy weather somewhat lowers the likelihood of piracy events and the effect does not depend on ground weather conditions, rainy weather decreases the likelihood of violent events on the ground only when the ocean weather is windy. Robustness checks lend further credence to these findings.

MOTIVATION: CORRELATING-WEATHER BIASES

Before stepping into a specific substantive topic, it is useful to consider the problem in a more generic sense: under what conditions does correlation among weather conditions matter for our analysis? The following figure (Figure 4-1) is a directed acyclic graph (DAG; Pearl 2009) in which our main interest is in the effect of a weather condition X_1 on a certain outcome Y. The weather variable, however, correlates with the other weather condition X_2 due to an unobserved common cause Z, which can be best considered as a complex climate system.[2] Although in a few cases a weather variable can causally affect another weather variable (for example, rainfall lowers temperature), in most cases, the correlation between weather variables is an outcome of a complex climate system. Rainy weather, for instance, often coincides with gale, but this does not mean that the rain would *cause* the wind. The coincidence reflects a natural phenomenon known as a "storm."

Figure 4-1. Generic Diagram



NOTE: The figure shows a directed acyclic graph (DAG) of two weather variables (X_1 and X_2), an outcome (Y), and a common cause (Z) that generates the two observed weather variables. We are interested in the effect of X_1 on Y.

[2] For simplicity, I consider only two correlating weather variables, but the argument can be easily extended to multiple weather variables.

An obvious but important insight is that in the presence of the unobserved common cause Z, the effect of X_1 on Y cannot be estimated without accounting for X_2. Without controlling for X_2, we cannot identify the causal effect $X_1 \rightarrow Y$ from the backdoor path $X_1 \leftarrow Z \rightarrow X_2 \rightarrow Y$. For instance, we cannot distinguish whether rainfall affects conflict ($X_1 \rightarrow Y$) or rainfall correlates with windy weather, which in turn affects conflict ($X_1 \leftarrow Z \rightarrow X_2 \rightarrow Y$). This means that a seemingly valid regression of Y on X_1 suffers from an omitted variable bias. Note that transformations of the weather variables (such as using rainfall deviation) or conventional regression techniques (such as fixed effect) cannot solve the problem. The problem here is not in generic geographical or climate conditions, but they are in the instantaneous coincidence of multiple weather phenomena.

This does not mean that inter-weather correlation would *always* bias our causal estimates; it depends on our theory and empirical evidence. In fact, when a correlating weather X_2 has no effect on an outcome of interest Y, we can still validly infer the causal effect $X_1 \rightarrow Y$ even without accounting for the inter-weather correlation. Thus, the problem hinges on whether X_2 affects Y. Since there is no one-size-fits-all answer to this question, we need to assess the problem case-by-case based on substantive and empirical knowledge. To this end, I now turn to the problem in a specific context: the effects of ground and ocean weather on violence and piracy.

LITERATURE: VIOLENCE, PIRACY, AND SUBSTITUTION

The potential for the correlating weather bias is mostly dismissed in the literature about the effects of weather conditions on conflicts. Although there are diverse studies on this topic,[3]

[3] I do not provide an extensive survey of the literature. For a more extensive review of the literature, see Daxecker and Prins (2017a) and Axbard (2016) for maritime piracy, and Hsiang, Meng, and

including those about the effects of climate shocks on armed conflict (Hsiang, Meng, and Cane 2011; 2013, 2014; Buhaug et al. 2014; Sarsons 2015), the effects of weather conditions on tactical choices and electoral violence (Condra et al. 2018), the effects of ocean wind speed on maritime piracy (Percy and Shortland 2013), and the effects of plankton abundance on piracy (Flückiger and Ludwig 2015; Axbard 2016), these studies analyze either one of ground and ocean climates without justifying the assumption that ground (ocean) weather conditions would affect only armed conflict (maritime piracy). This raises a question: do we have any reason to believe that armed conflict and maritime piracy depend on both ground and ocean weather conditions?

Theories of strategic substitution (Most and Starr 1984; Morgan and Palmer 2000; Palmer, Wohlander, and Morgan 2002; Clark and Reed 2005; Clark, Nordstrom, and Reed 2008) provide one reason to believe that the answer is positive. Since rebels can allocate only a finite amount of resources (such as money, military personnel, weaponry, logistics, and even time) to piracy or armed conflict, they need to compare the *relative* costs and benefits of those options. This means that if ground weather conditions would affect the payoffs from violent activities, it also changes the *relative* costs and benefits of piracy. Similarly, if rough ocean prevents rebels from going to seas, the rebels may alternatively resort to violence. Thus, from the perspective of substitution, it is a real possibility that ground and ocean weather conditions affect *both* armed conflict and piracy.

In this regard, the most relevant study is Jablonski and Oliver (2013), who examine how labor market opportunities, instrumented by monthly rainfall deviation on the ground, can affect

Cane (2011; 2013) for the effect of climate or weather on conflict. For the purpose of this paper, it is sufficient to state that to my best knowledge no conflict studies account for the possible correlations between ground and ocean weather conditions.

the number of piracy events. Thus, in a reduced form, they effectively analyze the effect of monthly rainfall on maritime piracy. Their analysis, however, does not account for rainfall's effect on armed conflict or possible correlation between ground rainfall and ocean wind speed. As a result, if rainy weather coincides with windy ocean, and if windy ocean decreases the number of piracy events as demonstrated by Percy and Shortland (2013), their instrumental variable can be associated with their outcome variable through the climatic correlation. This implies a possible violation of independence or exclusion restriction, crucial assumptions in instrumental variable analysis.

Another relevant study is Daxecker and Prins (2017a), who analyze the effect of maritime piracy on armed conflict. They argue that maritime piracy can provide armed groups with funding opportunities, and hence that piracy and armed conflict have a *complementary* relationship. Their empirical analysis indicates that conflict events tend to coincide with piracy events. However, the positive association does not necessarily mean that armed conflict and piracy would constitute a complementary relationship. In fact, as Morgan and Palmer (2000) state, the positive association can be attributed to an increase in the overall amount of available resources. With more resources, rebels may undertake more armed conflict *and* piracy, creating a positive association. Furthermore, even though armed conflict and piracy might be in a complementary relationship in the long term (Daxecker and Prins 2017a), they can still be in a substitutive relationship in the short term. In fact, as Most and Starr (1984) argue, unless actors possess infinite resources and time, there almost always exists some substitution in the short term. Thus, even though my argument can be extended to a complementary relationship, I focus on the short-term substitutive relationship.[4]

More importantly, a theoretical problem common to these studies and substitution theories in general is that they focus on the *presence or absence* of a substitutive or complementary relationship, without differentiating *different types* of substitution. As I detail in the next section, this void of knowledge is critical because even if an empirical finding indicates the absence of *a* particular type of substitution, it does not necessarily mean that there does not exist *any* type of substitution. Thus, without understanding the types of substitution and their differences, we may misinterpret our empirical findings.

THEORETICAL MODEL: A TYPOLOGY OF SUBSTITUTION

I argue that there are at least three types of substitution that predict different empirical patterns. When piracy is an *upward substitute* for violence, piracy is assumed to be substantially more profitable than violence. Thus, rebel groups conduct piracy activities whenever it is low cost (calm seas), while they engage in violence only when piracy is costly (rough seas) and violence is low cost (sunny weather). This means that piracy depends only on ocean weather conditions, but violence is affected by both ground and ocean weather conditions. By contrast, when piracy is a *downward substitute* for violence, the situation is reversed; piracy is less profitable, and thus rebels engage in violence whenever its costs are relatively cheap, while they conduct piracy only when it

[4] Since the complementary relationship predicts opposite directions of coefficients, my empirical analysis can test them as well. Due to word limits, I cannot elaborate typologies or predictions of the complementary relationship.

is low cost and violence is costly (rainy weather). This implies that only ground weather conditions affect piracy, while both ground and ocean weather influences violence. Finally, when piracy is an *equivalent substitute* for violence, piracy is as profitable as violence. Thus, piracy or violence occurs when it is lower cost than its alternative. This means that both violence and piracy depend on ground and ocean weather conditions.

To formalize this notion, consider an armed rebel group that can allocate a finite amount of resources $r > 0$ (such as funding, military personnel, weaponry, and even time) to ground military and maritime piracy activities. The amounts of resources devoted to military and piracy activities are denoted by $y_G, y_M > 0$. By spending resources to a certain activity, the rebel group receives returns $b_.(y_.)$, including tactical controls of territories and navigation or monetary opportunities (looting and ransom), while they also need to incur costs for the activity $c_.(y_.)$. Thus, the rebels' generic payoff is $u = b_G(y_G) - c_G(y_G) + b_M(y_M) - c_M(y_M)$ with a resource constraint of $y_G + y_M \leq r$. As Morgan and Palmer (2000) note, the resource constraint is the key element that creates a substitutive relationship. That is, exactly because rebels do not possess an infinite amount of resources, they need to compare and substitute their available options.[5]

I assume a quadratic profit function $b_.(y_.) = (b_. - y_.)y_.$ and a linear cost function $c_.(y_.) = c_.y_.$ with $b_., c_. > 0$, which are commonly used in formal analysis (Osborne 2003). The quadratic profit function, which has an inverted U shape, reflects the notion that allocating a certain amount

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[5] Conceptually, my model is analogous to the "one-goods-two-inputs" model in Morgan and Palmer (2000). The "goods" is the utility u, and the "inputs" are military and piracy activities. This implies that despite the differences in the mathematical expositions, my conceptualization is essentially consistent Morgan and Palmer (2000) and Most and Starr (1984).

of resources to military or piracy activities allows the rebel group to acquire more tactical or monetary gains, but excessive effort for an activity causes *backlash* and associated risks, such as government repression (Young 2012), anti-piracy patrolling (Daxecker and Prins 2017b), and avoidance by commercial vessels (Bensassi and Martínez-Zarzoso 2012). The parameter $b_{\cdot}$ represents the marginal profit when $x_{\cdot} = 0$ (initial marginal returns). In general, the larger $b_{\cdot}$ is, the more profitable the option is. The linear cost function represents the loss of a certain amount of resources that would have been used for other purposes. When the parameter $c_{\cdot}$ takes a larger value, the activity require more resources (larger operational costs), or those resources could be used for other more profitable activities (larger opportunity costs). With these parameterizations, the utility function becomes: $u = (b_G - y_G)y_G - c_G y_G + (b_M - y_M)y_M - c_M y_M$.

I assume that the cost $c_{\cdot}$ is affected by weather conditions (I am not assuming that the costs would depend *only* on weather conditions). As commonly acknowledged by military experts and historians (Ochmanek 2003; Meier 2015), rainy weather poses special challenges in a military operation, including marching on muddy ground, maintenance of supply lines, and lower morale, indicating larger tactical costs.[6] In addition, as Percy and Shortland (2013) argue, windy ocean makes piracy activities potentially deadly endeavors; a majority of contemporary rebel groups or

[6] One might argue that stormy weather would provide favorable conditions for guerrilla tactics. In this case, storms must increase the levels of violent activities on the ground. However, this is not consistent with my empirical findings in the later sections. In addition, although rainy weather might affect the agricultural production and hence b_G (Miguel, Satyanath, and Sergenti 2004), this study is concerned with *day-to-day* weather variation, and the later analysis accounts for it by controlling for long-term rainfall amounts.

pirates do not possess large-scale armored ships, mostly relying on speedboats, fishing vessels, or even handmade wooden boats, which are especially vulnerable to rough waters (Murphy 2009).

Quantities of interest are the effects of the costs c_L and c_S on the incidences of rebels' violence and piracy, $Y_G = \mathrm{I}(y_G > 0)$ and $Y_M = \mathrm{I}(y_M > 0)$, where I is an indicator function. Although the continuous effort levels y_G and y_M may also be of interest, dichotomous outcomes are more useful for deriving a *typology* of substitution. Furthermore, the effort levels are complicated non-linear functions of c_G and c_M, and hence empirical analysis can only weakly identify the theoretical relationship.[7] Finally, as I later mention, the dichotomous variable is less sensitive to the aggregation problems, allowing us to conduct an empirical analysis at an aggregated level.

Notes on the Model

Note that the model does not assume that a rebel group would always consider piracy as an open option. Even though contemporary piracy usually does not require high skills or strong naval capabilities (Murphy 2009), rebel groups might not even think of piracy as their choice. In my model, this is equivalent to setting the costs for piracy c_M to a very large value. With a very large c_M, rebels never engage in piracy activities, and hence the likelihood of rebels' violence only depends on its costs c_G. Thus, if piracy would be out of rebels' choices, ocean weather conditions should have no effect on violence or piracy, and only ground weather conditions would affect violence.

[7] In a later robustness check, I also conduct an analysis with continuous outcome variables.

Relatedly, although the model assumes a unitary rebel-pirate actor, this assumption should be considered as an imprecise but useful construct (Clarke and Primo 2012). In several cases, such as the Nigeria Delta insurgents, rebel groups indeed conduct pirate activities. In other countries, including Somalia, rebel groups do not directly engage in piracy but collect various forms of taxes and fees from pirates (Murphy 2009; Percy and Shortland 2013; Shortland and Varese 2016). Nonetheless, even in those cases, there are substantial overlaps and labor mobility between the members of rebel and piracy groups (ibid). Thus, even in absence of a unitary command system, weather conditions can affect individual soldiers' decisions over fighting on the ground or pirating on the ocean. Importantly, if it were not for any overlap or labor mobility, violence and piracy would be independent processes, and thus violence (piracy) would depend only on the ground (ocean) weather conditions.

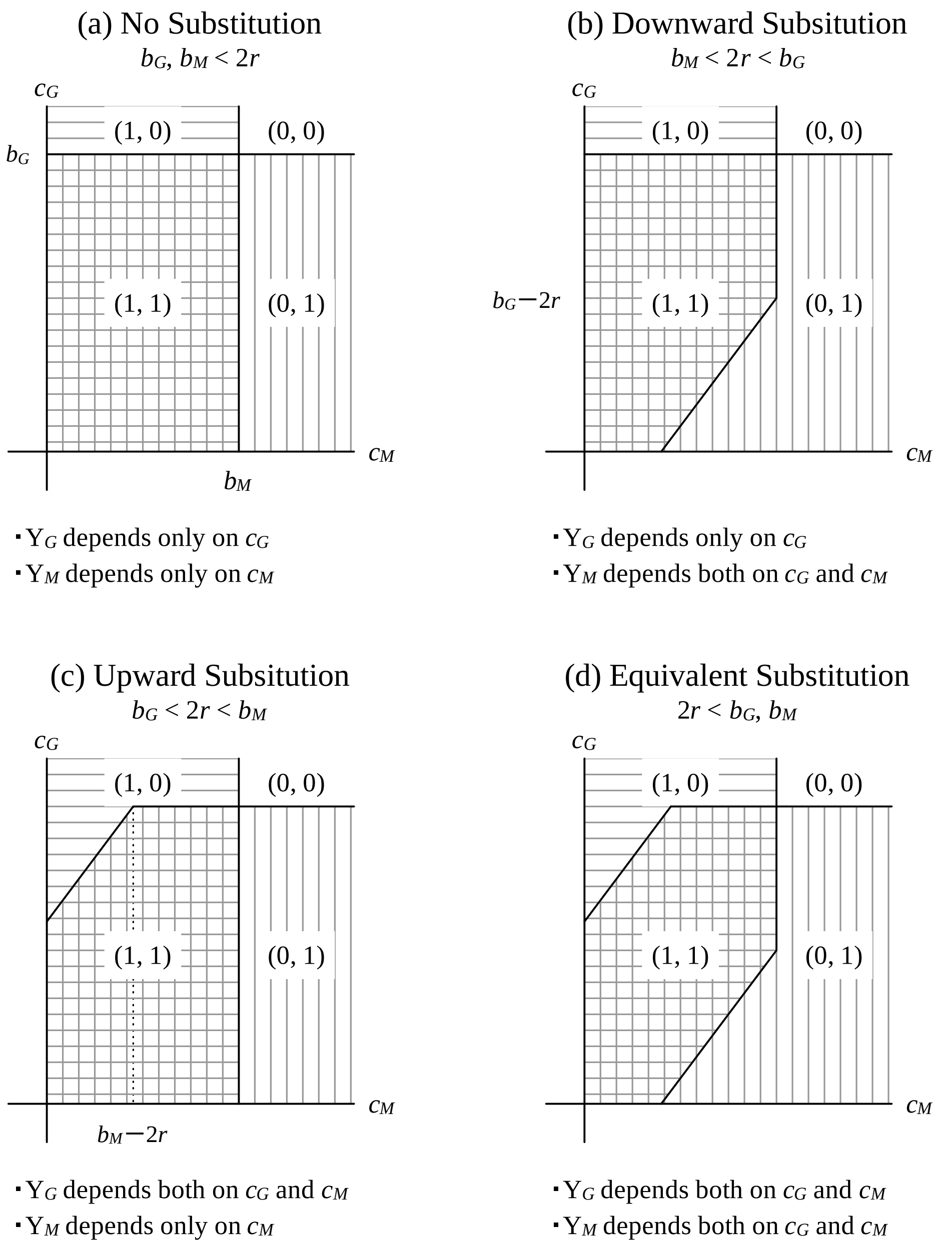
Finally, as I mentioned, if rebels have third choices, such as negotiated settlements, exile to foreign countries (exit strategy; Salehyan and Gleditsch 2006) and nonviolent contestation (Cunningham 2013), it increases the opportunity costs for violence and piracy, indicating larger values of c_G and c_M. If the third choices are far more attractive than piracy and violence, piracy and violence would not occur. Similarly, if the third choices are far less attractive than violence and piracy, they do not alter any results of my model. If the third choices are as profitable as violence or piracy, the groups may choose the third strategy even when my model predicts that they should choose violence or piracy. Although this would not bias the causal estimates (as weather conditions are not affected by the availability of the third choices), the existence of third choices can add noise and hence lower the power of analysis. This makes my empirical analysis a hard test for the substitution hypotheses.

Optimal Outcomes

Since the model has a unique optimum that can take seven different forms, I focus on the effects of the weather conditions on the incidences of rebels' violence and piracy activities, leaving the details of the optima and their conditions to supporting information.[8] As summarized in Figure 4-2, it turns out that the effects of ground and marine weather conditions (c_G and c_M) on the incidences of violence and piracy (Y_G and Y_M) are different, depending on the profits from those options (b_G and b_M).

[8] See SI 3-1.

Figure 4-2. Comparative Statistics



NOTE: The figures show the incidences of piracy and violence with respect to their costs. The parentheses in each pane are the incidences of piracy and violence (Y_M, Y_G). The vertical and horizontal lines indicate the incidences of violence ($Y_G = 1$) and piracy ($Y_M = 1$) respectively. I put gray shade to the areas in which c_M and c_G interactively affect Y_M or Y_G. The panes, (a), (b), (c), and (d) correspond to the cases in which piracy is not a substitute for violence, piracy is a downward substitute for violence, piracy is an upward substitute for violence, and piracy is an equivalent substitute for violence.

No Substitution

When neither option is sufficiently lucrative (pane a. of Figure 4-2), the rebels' decision is affected little by resource constraints. That is, the rebels independently assess the costs and benefits of each option and allocate resources until it causes substantial backlash. This confirms Morgan and Palmer (2000)' claim that if it were not for a resource constraint, there would be no incentive to compare or substitute available options. With no substitution, the incidence of violence depends only on c_G, and the incidence of piracy depends only on c_M (baseline null hypothesis).

Downward Substitution

By contrast, when military activities are sufficiently profitable relative to piracy activities (piracy is *a downward substitute* for military actions; pane b. of Figure 4-2), rebels' choices are constrained by the resource availability. In this case, when costs for these actions are cheap, rebels would like to spend more than r resources on military actions and a few resources on piracy. However, the resource constraint forces them to compare the relative payoffs and optimally allocate the finite resources. While rebels always spend some resources on the military actions unless the costs are enormous ($c_G \geq b_G$), they engage in the less lucrative option—piracy activities—only when piracy activities provide a positive profit ($c_M < b_M$) *and* they are profitable compared to its alternative ($b_M - c_M > b_G - c_G - 2r$). This means that while the incidence of rebels' violence depends only on its costs c_G (and thus ground weather conditions), the incidence of piracy attacks depends on both c_G and c_M (both ground and ocean weather conditions).

Upward Substitution

The situation is opposite when piracy is *an upward substitute* for military actions (pane c. of Figure 4-2). When piracy activities are sufficiently more profitable than military activities, rebels have incentives to spend some amount of resources on piracy activities as far as they are affordable ($c_M < b_M$). However, since military activities are much less lucrative, they are willing to allocate their finite resources to military actions only when military actions provide positive returns ($c_G < b_G$) *and* they provide a better payoff than continuing piracy activities under strong backlash ($b_G - c_G > b_M - c_M - 2r$). Thus, when piracy is an upward substitute for violence, the incidence of piracy depends only on its costs c_M (and thus ocean weather conditions), while the incidence of rebels' violence depends both on c_G and c_M (both ground and ocean weather conditions).

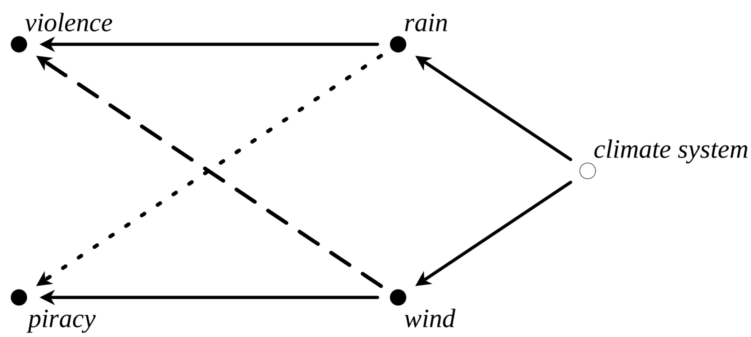
Equivalent Substitution

Finally, when both ground military and maritime piracy activities are sufficiently lucrative (pane d. of Figure 4-2) and hence they constitute *equivalent substitutes*, the results become the mixture of the upward and downward substitutions. That is, because both options are potentially lucrative, rebels can easily substitute one option for the other. As a result, rebels weigh the relative costs and benefits of military and piracy activities, and they decide the allocation of their resources. This implies that rebels employ military activities only when that option provides a positive return *and* the alternative option is not predominantly lucrative, and vice versa. Thus, as seen in pane d of Figure 4-2, neither c_G nor c_M alone could perfectly predict the incidences of violence or piracy. Only when we consider c_G and c_M jointly, we are able to precisely predict the outcomes. This suggests that ground and ocean weather conditions interactively affect the incidences of violence and piracy.

EMPIRICAL MODEL AND OBSERVABLE IMPLICATIONS

With the formal analysis, I am now able to specify the causal diagram presented in Figure 4-1. The following figure (Figure 4-3) presents the specified version of the DAG, where $violence$ and $piracy$ are the incidences of rebels' ground and marine violent activities, $rain$ and $wind$ are the amount of ground rainfall and the speed of ocean wind, and $climate\ system$ refers to an unobserved system of climate. The dotted arrow exists under the downward and equivalent substitution, and the dashed arrow exists under the upward and equivalent substitution.

Figure 4-3. Specified Diagram



NOTE: The figure shows a directed acyclic graph (DAG), in which ground rainfall ($rain$) and ocean wind speed ($wind$) affect the levels of rebels' ground and marine military activities ($violence$ and $piracy$). The dotted arrow exists under downward and equivalent substitution, and the dashed arrow exists under upward and equivalent substitution.

The DAG corresponds to a series of equations;[9]

$$violence = f_{violence}(rain, wind);$$

$$piracy = f_{piracy}(rain, wind),$$

[9] For readability, I omit the functions of rain and wind with respect to climate system.

where $f.$ is a generic function. With a linear approximation proposed by Hsiang, Burke, and Miguel (2013)[10] and adding interaction terms, the equations are converted to empirical counterparts;

$$violence_{it} = \beta_0 + \beta_1 rain_{it} + \beta_2 wind_{it} + \beta_3 rain_{it} wind_{it} + \beta_4 x_{it} + \mu_i + \theta_{id_t} + \phi h(t) + \epsilon_{it};$$

$$piracy_{it} = \gamma_0 + \gamma_1 rain_{it} + \gamma_2 wind_{it} + \gamma_3 rain_{it} wind_{it} + \gamma_4 x_{it} + \nu_i + \vartheta_{id_t} + \varphi h(t) + \varepsilon_{it},$$

where i is a coastal conflict country, t is a date, d_t is a month-day of a given date. The outcome variables $violence_{it}$ and $piracy_{it}$ are the incidences of rebels' violence and piracy events. The explanatory variables $rain_{it}$ and $wind_{it}$ are the amount of ground rainfall and the speed of ocean wind. The column vector x_{it} includes covariates with corresponding row vectors of the coefficients β_4 and γ_4. The model also includes country-specific intercepts μ_i and ν_i, which account for climatic differences across countries, country-month-day-specific fixed effects θ_{id_t} and ϑ_{id_t}, which account for country-specific seasonality, and cubic splines of time trends $\phi h(t)$ and $\varphi h(t)$, which control for long-term trends of the event data and climate changes (Beck, Katz, and Tucker 1998).[11]

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[10] For the purpose of causal inference, linear models are fully meaningful (Angrist and Pischke 2009; Hsiang, Burke, and Miguel 2013). The generalized linear models with fixed effects usually require the control function approach (simply adding dummies causes incidental parameter biases; Wooldridge 2005), which are reliant on parametric assumptions, numerically unstable, and computationally expensive.

[11] Note that as I later mention having the country-month-day-specific intercepts are equivalent to subtracting each variable by its country-month-day averages. Thus, using rainfall or wind speed *deviations* (measured as the observed rainfall or wind speed minus the average rainfall or wind speed on that day) is mathematically the same as the above specification.

I use coastal countries as spatial units because there is no data about the geographical locations of rebel groups and hence we cannot easily assign the values of the climate variables to rebel groups. Since this might cause problems of ecological inference, it is useful to theoretically consider the problems. From the standpoint of my theory, if a rebel group attempts activities, it can cause backlash and thus decrease the marginal returns from the activity, which in turn can disincentivize other groups from engaging in the same activity. This implies that there might be a negative externality and correlation among groups, which can cause *a bias toward zero* in an empirical analysis with aggregated units (King, Tanner, and Rosen 2004). Furthermore, the outcomes are dichotomous variables that take one if even a single group engages in military or piracy activities. As a result, even though the counts of those events might be subject to the abovementioned biases, the dichotomous variables are less affected by the problem. In fact, the theoretical predictions about the dichotomous variables hold even when I extend the model to the aggregated incidences of violence and piracy with two rebel groups.[12]

Predictions

The following table (Table 4-1) summarizes the predicted directions of the interactive effects (β_3 and γ_3 in the above equations). First, in the absence of substitution (first row of Table 4-1), there is no interactive effect; the incidence of violence depends on ground weather conditions, while the incidence of piracy depends on ocean weather conditions. Thus, the coefficients of the interaction terms are predicted to be zero, constituting a baseline null hypothesis. Second, when piracy is an upward substitute for violence (the second row of Table 4-1), violence occurs only

[12] The full description and solution of the game-theoretic model is provided upon request.

when it is more beneficial than piracy, while piracy can happen whenever it is profitable for its own sake. This suggests that ground and ocean weather conditions interactively affect violence and that the interactive effect is negative, while the incidence of piracy depends only on ocean wind.

Table 4-1. Predicted Directions of the Interactive Effects

Outcome Coefficient	$conflict_{it}$ β_3	$piracy_{it}$ γ_3
No Substitution	0	0
Upward Substitution	$-$	0
Downward Substitution	0	$-$
Equivalent Substitution	$-$	$-$

NOTE: The table summarized the predicted directions of the interactive effects of ground rainfall and ocean wind speed on the incidence of violent events (β_3) and the incidence of piracy events (γ_3). "$-$" and "0" indicate a negative and zero coefficient value respectively.

Third, by contrast, when piracy is a downward substitute for violence (the third row of Table 4-1), the situation is reversed. While violence can happen whenever it is profitable for its own sake, piracy occurs when it is more profitable than violence. This indicates a negative interactive effect of ground and ocean weather conditions on the incidence of piracy, while only ground weather conditions will affect the incidence of violence. Finally, when piracy is an equivalent substitute for armed conflict (the bottom row of Table 4-1), rebels weigh the returns from the ground and marine military activities, and hence ground and ocean weather conditions interactively affect both violence and piracy. Since violence occurs when violence is more profitable than piracy and vice versa, the directions of the interactive effects are predicted to be negative. With these predictions, I now turn into the measurement of the variables.

DATA AND METHOD

The sample includes coastal areas of 30 conflict countries for the period of 2001-2016, including 131,831 coastal country-day observations.[13] The time period is selected on the basis of data availability.[14] Since piracy activities are physically infeasible for rebels in inland countries, following Daxecker and Prins (2013), I limit the countries to those along the coast. Furthermore, because my theory assumes the existence of *armed* rebel groups, I drop non-conflict countries and periods (Themner 2012).[15] Finally, among the remaining 44 coastal conflict countries, I drop countries that have zero variance of the outcome variables.[16] The resultant 30 countries are Algeria, Angola, Bangladesh, Colombia, the Republic of the Congo (Congo), the Democratic Republic of

[13] For detailed description of the sample, see SI 3-2.

[14] Satellite images are available only after 2000. Because I take a one-year lag of the predictors (see later subsection *Controls*), the first year is 2001. The latest version of piracy event data is available up to 2016.

[15] The conflict countries are the countries that have at least one episode of armed conflict in the UCDP/PRIO Armed Conflict Dataset (Themner 2012).

[16] The dropped countries are Levant countries (Israel, Jordan, Lebanon, Syria, and Turkey), where piracy is near-impossible due to EU's military commitment, the former-CIS countries (Georgia, Russia, and Ukraine), where the frozen or inland seas inhibit pirates, Cameroon and China, where only the states' violence against civilians are reported in the coastal areas, Pakistan, where the United States had heavy naval presence for the Afghanistan War, and Sudan, where the northern coastal areas are politically stable and far less influenced by the southern turmoil. Finally, I also drop the United States. In a later robustness check, I also conduct an analysis with these countries.

the Congo (DRC), Egypt, Eritrea, Haiti, India, Indonesia, Iran, Iraq, Ivory Coast, Kenya, Liberia, Libya, Malaysia, Mauritania, Mozambique, Myanmar, Nigeria, Peru, Philippines, Senegal, Sierra Leone, Somalia, Sri Lanka, Thailand, and Yemen. This middle-N approach with careful case selection allows us to limit the cases to a comparable set of countries, provides a guard against extrapolation, and hence makes the estimates more valid and meaningful "average" effects.[17]

Outcome Variables

The outcome variables are the incidences of rebels' violence and piracy. The event data of armed conflict come from the UCDP GED (Sundberg, Lindgren, and Padskocimaite 2010), a standard dataset of conflict events.[18] Since rebels can put its efforts towards any types of ground military activities, I include any violent events in which at least one of the participants is a rebel group. Since my theory relates to aggregated outcomes of rebels' behaviors, events that do not involve rebels are omitted. Finally, to select conflict events that are relevant to piracy, I limit the

[17] Strictly speaking, OLS with a continuous treatment does not provides an average treatment effect, but the estimates can be considered as a kind of an weighted average effect (Angrist and Pischke 2009).

[18] The Armed Conflict Location and Event Data (ACLED) is available only for African countries (Raleigh, Linke, and Dowd 2014) and hence is not used in this paper. In a robustness check, I also use the Global Terrorism Database (GTD; LaFree and Dugan 2007).

events that occurred within 100 kilometers from the coast lines.[19] The outcome variable $violence_{it}$ takes 1 if there is any rebels' violent event on a day t within 100 kilometers from the territorial seas of a country i.

The piracy data come from the updated version of the Maritime Piracy Dataset (MPD version 2.5; Coggins 2012). The dataset relies on the reports of piracy attacks submitted to the International Maritime Bureau (IMB). Maritime piracy is defined as "an act of boarding or attempting to board any ship with the apparent intent to commit theft or any other crime and with the apparent intent or capability to use force in the furtherance of that act" (Coggins 2012, 606). Although Daxecker and Prins (2013) also develop an alternative dataset, the Maritime Piracy Event and Location Dataset (MPELD), the following analysis uses the MPD as a primary source. While the MPELD contains information about the countries closest to piracy events, the MPD has records about the country origin of pirate attackers, which is crucial for the analysis of this paper. For instance, even though there are many piracy attacks in the Gulf of Aden, this does not mean that all of the pirates come from Yemen (most of them come from Somalia). Thus, without the information about attackers' origins, we may misclassify the event locations.[20] The variable

[19] Without this criteria, for instance, the conflict events near Lake Tanganyika in the DRC must be considered something relating to piracy activities in South Atlantic Ocean. In later robustness checks, I also conduct analyses with thresholds of 50 and 200 kilometer coastal distances.

[20] In a robustness check, I also use the MPELD. Note that the MPD and MPELD both primarily rely on the IMB's reports (though the MPELD includes a few other reports as well).

$piracy_{it}$ takes 1 if there is any piracy event on a day t in which at least one of the attackers come from a country i.[21]

Although both of the event datasets are based on reports of media or international organizations and hence subject to reporting biases, "as long as the measurement error is uncorrelated with the independent variables, measurement error in the dependent variable is not particularly problematic in a standard regression framework other than increasing the uncertainty around the estimates we obtain" (Weidmann 2016, 208). As I detail in the next subsection, my predictors, $rain_{it}$ and $wind_{it}$ are based on satellite images, which are not subject to systematic reporting biases. Although the precision of satellite images can be different across locations, the errors are unlikely to be systematically different across countries, and the fixed effects can readily account for the country-specific and seasonal biases.

Explanatory Variables

The data of ground rainfall amounts are derived from the Tropical Rainfall Measuring Mission (TRMM; Huffman et al. 2007) dataset, which is available for every three hours in every 0.25-degree by 0.25-degree grid cells (2.8-kilometer by 2.8-kilometer cells at the equator).[22] The

[21] If attackers' origins are not available, the MPD assumes that the pirates come from the closest littoral sea. This approximation is used for about 10% of the events (Coggins 2012).

[22] Other data sources, such as NOAA CPC Morphing Technique (CMORPH), Precipitation Estimation from Remotely Sensed Information using Artificial Neural Networks (PERSIANN), and Moderate Resolution Imaging Spectroradiometer (MODIS) products, have either shorter temporal coverage, lower spatial or temporal resolution, or a large number of missing values.

TRMM is a joint mission by NASA and Japan Aerospace Exploration Agency, and the precipitation data is based on the cross-validation and aggregation of multiple satellite radar and weather sensors. The variable $rain_{it}$ is the average amount of ground rainfall (millimeter per hour) on a day t within 100 kilometers from the coast of a country i.[23]

The data of ocean wind speed come from the Cross-Calibrated Multi-Platform (CCMP; Atlas et al. 2010) gridded surface vector wind dataset, which is available daily at a spatial resolution of 0.25-degree by 0.25-degree grid cells.[24] The CCMP project is based on and funded by NASA's research programs. The CCMP products are cross-calibrated from multiple satellite data so that the wind measures are consistent over time. The variable $wind_{it}$ is the average speed of ocean wind (meters per second) on a day t within 100 kilometers from the lands of a country i.[25] Both of the weather variables are based on satellite images, and hence they are not affected by the problems relating to weather station data (Schultz and Mankin Forthcoming).

Control Variables

The covariates include past-one-year ($t-1, \dots, t-365$) averages of ground rainfall and ocean wind speed and their interactions.[26] Because long-term rainfall may affect daily rainfall (due

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[23] In a robustness check, I also rerun the analysis with 50 and 200 kilometer thresholds.

[24] Other data sources, such as QuikSCAT, Blended Sea Wind, and MODIS datasets, have either shorter temporal coverage, lower temporal resolution, or a large number of missing values.

[25] In a robustness check, I also rerun the analysis with 50 and 200 kilometer thresholds.

[26] I use the past-one-*year* average as it includes growing seasons regardless of seasonality or countries. In a robustness check, I also use past-one-week and month averages.

to autocorrelation) and it may also have long-term consequences, such as the effects on agricultural production (Miguel, Satyanath, and Sergenti 2004), it is necessary to control for the long-term precipitation. Similarly, long-lasting rough seas can reduce income opportunities of fishery-related sectors, which in turn might affect maritime piracy. By controlling for the long-term ocean wind speed, I can mitigate possible biases due to the endogeneity.

I do not include other political, economic, or social covariates, such as democracy indexes and GDP per capita. Because these factors are unlikely to affect the weather variables, it is unnecessary and even harmful to control for those variables. In fact, as Hsiang, Burke, and Miguel (2013) state, these factors can be affected by the weather conditions, and thus controlling for these variables can cause posttreatment control biases. Note also that the geographical and seasonal factors, such as average rainfall and monsoon seasons, are already accounted for by the country and country-month-day fixed effects, and hence that they are not included in the analysis. In addition, even if the covariates change over time, most of them are available only at a yearly level. Thus, country-year fixed effects can account for those time-variant covariates as well.[27]

A remaining concern would be other weather variables that correlate with ground rainfall or ocean wind speed. For instance, temperature might also be an outcome of a climate system and affect conflict events (Hsiang, Meng, and Cane 2011; 2013). If this were the case, there would exist an unaccounted backdoor path, $rain \leftarrow climate\ system \rightarrow temperature \rightarrow violence$. On the other hand, rainfall can also causally affect temperature; precipitation and resultant evaporation can lower surface temperature. As a result, controlling for temperature can cause a posttreatment control bias. Given this tradeoff, my strategy is to report the main results without controlling for

[27] See later robustness checks.

temperature (prioritizing parsimony over complication), while also reporting the results with control for temperature in a robustness check.[28]

Estimator

The linear regression models are estimated with ordinary least squares (OLS). Note that even though the error terms of the two regressions ϵ_{it} and ε_{it} may correlate, the variables in the right hand sides of the equations are exactly the same, and hence seemingly unrelated regression and corresponding feasible generalized least squares (FGLS) are numerically equivalent to OLS (Greene 2011).[29] The standard errors are clustered for each country. Since the cluster standard errors with a relatively small number of units can over-reject the null (Cameron and Miller 2015), I use a conservative degree of freedom, $30 - 8 = 22$. Although this provides a guard against over-rejection, it lowers the power of the analysis, and it thus may favor the "no substitution" hypothesis. Since the calculation of the clustered standard errors becomes numerically unsolvable with $30 +$

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[28] I do not include ocean wave height as it cannot affect ocean wind, but it is affected by ocean wind speed (thus, controlling for wave height risks posttreatment control bias). Other weather variables, such as cloud cover, humidity, and air pressure, might be relevant. However, accounting for all weather conditions is infeasible (the number of interaction terms explodes as I include more variables). In this point, my strategy is similar to traditional observational studies: referring to the literature (Hsiang, Burke, and Miguel 2013), selecting weather variables that are shown to be relevant to conflict, and complying with Achen (2005)'s rule for parsimony.

[29] Note that even when the right-hand-side variables are different, OLS is still consistent, even though it becomes less efficient than FGLS.

$30 * 365 - 2 = 10{,}978$ dummies of fixed effects, I transform the fixed effect models by subtracting each variable by its country average (de-meaning) and then by its country-month-day average (de-seasoning). The resultant variables are the *deviations* from the averages in a country i on a day t.

RESULTS

The following table (Table 4-2) shows the estimated values of the coefficients.

Table 4-2. OLS Estimates

	1	2	3	4
Outcome	$violence_{it}$	$violence_{it}$	$piracy_{it}$	$piracy_{it}$
$rain_{it}$	-0.0064 [-0.0135, 0.0001]	-0.0046 [-0.0116, 0.0025]		0.0026 [-0.0008, 0.0060]
$wind_{it}$		-0.0031 [-0.0124, 0.0062]	-0.0035 [-0.0099, 0.0028]	-0.0046 [-0.0101, 0.0010]
$rain_{it}wind_{it}$		-0.0043 [-0.0068, -0.0018]		0.0060 [-0.0022, 0.0142]

NOTE: The table shows the OLS estimates of the standardized coefficients. The first and third columns show the effects of ground rainfall or ocean wind speed on the incidence of rebels' violent events or piracy events respectively. The second and fourth columns add interactions and their lower terms. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, cubic splines of common time trends. The control variables for model 1 and 3 are the past-one-year average of ground rainfall and ocean wind speed respectively. The control variables for model 2 and 4 are the past-one-year averages of ground rainfall and ocean wind speed, and their interaction. $n = 131{,}831$ with 30 coastal conflict countries for 2001-2016 period.

Without accounting for the cross-space effects of the weather conditions (column 1 and 3 in Table 4-2), there is weak evidence that ground rainfall halts rebels' military activities ($p = 0.077$), while the effect of ocean wind speed on maritime piracy is negative but not distinguishable from zero ($p = 0.258$). These results are not surprising as these models ignore the interactive effects and are thus underspecified.

When the interaction terms are included (column 2 and 4 in Table 4-2), the results indicate that the empirical patterns are the most consistent with the upward substitution hypothesis; the effect of ground rainfall on violent events depends on the ocean wind speed, while such an interactive effect is not statistically discernible for the incidence of piracy events. While the coefficient of $rain_{it}wind_{it}$ in a regression of violent events is negative and statistically significant ($p = 0.0021$), the corresponding coefficient is not significant and even positive for piracy ($p = 0.1450$). Although this does not necessarily mean that there is “no” interactive effect as the upper bound of the confidence interval is large, the lower bound of the confidence interval is close to zero (-0.0022), indicating that the interactive effect is unlikely to be negative. These results contradict the no, downward, and equivalent substitution hypotheses and are best explained by the upward substitution hypothesis.[30]

Substantively, when ocean wind speed takes an average value, an increase of rainfall by one standard deviation (6.49 millimeter per hour) decreases the probability of rebel’s violent events by 0.0046 with a corresponding interval of $[-0.0116, 0.0025]$. By contract, when ocean wind speed is above average by one standard deviation, the effect size is nearly doubled; a one-standard-deviation increase of rainfall lowers the probability of violent events by 0.0089 with a confidence interval of $[-0.0152, -0.0025]$. Regarding piracy, when ground rainfall takes an average value, an increase of ocean wind speed by one standard deviation (1.435 meters per second) decreases the probability of piracy events by 0.0046 with a confidence interval of $[-0.0116, 0.0025]$. In contrast, when ground rainfall is above average by one standard deviation,

[30] Since I use a linear model, I do not present the marginal effect plots. In fact, as Hainmueller, Mummolo, and Xu argue (2018), the marginal effect plots can be misleading (especially in the case of $piracy_{it}$). See SI 3-14 for details.

the effect becomes positive: 0.0014 with a confidence interval of $[-0.0067\ 0.0095]$. Again, these results are the most consistent with the upward substitution hypothesis.

Because the coefficients of the control variables—the past-one-year averages of the rainfall and ocean wind speed, and their interaction—are not directly relevant to my theory or cannot be directly interpreted as causal effects (Blackwell and Glynn 2018), I do not report the estimates in Table 4-2. Nevertheless, even with a proper method,[31] none of the estimated coefficients are statistically significant. Although this does not mean that there is "no" long-run relationship (the confidence intervals are relatively large), the findings do not provide much meaningful inference about the long-run effects. This can be attributed to the relatively small number of countries and the conservative degrees of freedom.

Correlating-weather Biases?

These results provide support for the upward substitution hypothesis, indicating that both ground and ocean weather conditions affect the incidence of violent events. Does this mean that if we omit ocean weather conditions from the regression and hence ignore the backdoor path $rain \leftarrow climate\ system \rightarrow wind \rightarrow violence$, our estimate about $rain \rightarrow violence$ would be heavily biased? The following table (Table 4-3) indicates that the answer is negative; the results are similar whether I account for correlating-weather or not. Thus, it is unlikely that the naïve estimates would suffer large correlating weather biases, which is not surprising given a statistically significant but relatively weak correlation of $rain_{it}$ and $wind_{it}$ ($r = 0.0053$ with $p = 0.0270$).

[31] Structural nested mean models (Blackwell and Glynn 2018).

Table 4-3. Average Effects

Outcome	1 $violence_{it}$	2 $violence_{it}$	3 $piracy_{it}$	4 $piracy_{it}$
$rain_{it}$	-0.0064 [-0.0135, 0.0001]	-0.0046 [-0.0112, 0.0021]		
$wind_{it}$			-0.0035 [-0.0099, 0.0028]	-0.0046 [-0.0098, 0.0006]
Correlating weather	omitted	included	omitted	included

NOTE: The table shows the sample averages of the effects of $rain_{it}$ on $violence_{it}$ (model 1 and 2) and the effects of $wind_{it}$ on $piracy_{it}$ (model 3 and 4). The results about model 1 and 3 show the OLS estimates when correlating weather variables ($wind_{it}$ and $rain_{it}$ respectively) are omitted. The results about model 2 and 4 show the average marginal effects when the correlating weather variables and its interactions with the main weather variables ($rain_{it}$ and $wind_{it}$ respectively) are included. The 95% confidence intervals are in brackets. The models include country fixed effects, country-month-day fixed effects, cubic splines of common time trends. The control variables for model 1 and 3 are the past-one-year average of ground rainfall and ocean wind speed respectively. The control variables for model 2 and 4 are the past-one-year averages of ground rainfall and ocean wind speed, and their interaction. $n = 131{,}831$ with 30 coastal conflict countries for 2001-2016 period.

Although these results imply that the correlating-weather bias is not large in this study, we must be careful about generalizing these results to different samples or weather variables. While $rain_{it}$ and $wind_{it}$ are only weakly correlated in this study and hence the size of the correlating weather bias is small, depending on the magnitude of inter-weather correlations, it is possible that a separate regression would suffer a large bias. It is therefore recommended for future studies to check correlations among relevant weather variables, and if the correlations are large, to include the correlating weather variables and their interactions in a regression. Also note that even when separate regressions suffer minor biases, it does not mean that the estimated causal effect has theoretical interpretations or substantive meanings. Indeed, *for the purpose of testing the substitution types*, the models without interaction terms provides little theory-relevant information.

Robustness Checks

I also conduct a series of robustness checks. The following table (Table 4-4) summarizes the robustness checks, their results, and corresponding sections in the supporting information. As seen in the table, my main findings hold in most of the robustness checks. First, I use 50- and 200-kilometer thresholds to identify the coastal lands and seas (which are used to calculate the incidence of violent events and the weather variables). Although the statistical significance becomes somewhat lower for $violence_{it}$ with a 200 kilometer threshold—perhaps because $violence_{it}$ now includes events that occurred in inland areas and hence were less relevant to pirates—the other results are similar to the main results.

Table 4-4. Robustness Checks

		Coefficient of an interaction term		SI
		$conflict_{it}$ ($\hat{\beta}_3$)	$piracy_{it}$ ($\hat{\gamma}_3$)	
1	50 kilometer coastal distance	$-$	null	SI 3-3
2	200 kilometer coastal distance	$-^{\dagger}$	null	SI 3-3
3	Alternative measure of $violence$ (GTD)	$-$		SI 3-4
4	Alternative measure of $piracy$ (MPELD)		$+$	SI 3-4
5	Controlling for maximum and minimum temperature	$-$	null	SI 3-5
6	Controlling for past-one-week averages of $rain_{i,t}$ and $wind_{i,t}$, and their interaction	$-$	null	SI 3-6
7	Controlling for past-one-month averages of $rain_{i,t}$ and $wind_{i,t}$, and their interaction	$-$	null	SI 3-6
8	Controlling for country-specific trend variables	$-$	null	SI 3-7
9	Controlling for country-year fixed effects	$-$	null	SI 3-8
10	Analysis with all coastal conflict countries	$-^{\dagger}$	null	SI 3-9
11	Count variables	null	$+^{\dagger}$	SI 3-10
12	Leave-one-country-out tests	$-$	null	SI 3-11

NOTE: The table shows the results of the robustness checks. The first column shows the robustness checks. The second and third columns show the directions of $\hat{\beta}_3$ and $\hat{\gamma}_3$ (the coefficients of the interactions terms in regressions of $conflict_{it}$ and $piracy_{it}$). "$-$" indicates a negative value of a coefficient, "$+$" indicates a positive value, and "null" indicates a null result. The daggers † indicate the results with statistical significance of $0.05 < p \leq 0.1$. The last column shows the indexes of the supporting information.

Second, I use the Global Terrorism Database (GTD; LaFree and Dugan 2007) as an alternative measure of rebels' violence. This does not alter the results. Third, I use the MPELD (Daxecker and Prins 2013, 2017a) as alternative data of piracy events. With the alternative measure, the coefficient of the interaction term for $piracy_{it}$ becomes *positive* and statistically significant, indicating that windy ocean tends to *increase* piracy events when it is rainy in the coastal areas. A key to interpret this result is the fact that the MPELD does not identify the origins of pirates; instead, the outcome includes pirate attacks that occurred within 100 kilometers from coastal lines of a country i.[32] One possible explanation therefore would be that while rebels in country i are less or no more likely to engage in neither ground nor marine military activities with stormy weather, it actually invites even a larger number of foreign pirates who try to exploit the void, resulting in an increase in the reported piracy events. Since formally establishing this argument or rigorously testing the hypothesis is beyond the scope of this paper, I leave it to future studies. For the purpose of this paper, it suffices to state that the information about pirates' origins is crucial for the present analysis.

Fourth, as I previously mentioned, I add controls for the maximum and minimum temperature. The results are robust to the inclusion of these control variables. Fifth, I also control for past-one-week or past-one-month, instead of past-one-year, averages of $rain_{it}$ and $wind_{it}$, and their interaction. These do not change the results. Sixth, I also control for cubic splines of country-specific trend variables in order to account for time-varying heterogeneities within countries. The results are robust to the inclusion of the country-specific trend variables. Likewise, seventh, I also include country-year fixed effects and find similar results. This implies that the results are robust to the inclusion of country-year covariates, such as GDP per capita and democracy indexes.

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[32] When I use the MPD and include the events that occurred within 100 kilometer from coastal lines, the results are similar to those with the MPELD-based measure. This means that the difference lies not in the coding rules or data sources of the MPD and MPELD but in the ways to calculate $conflict_{it}$.

Eighth, I conduct analysis with additional 14 coastal conflict countries that have zero variance in $violence_{it}$ or $piracy_{it}$ and thus are omitted in the above analysis. Although the inclusion of the zero-variance cases decreases the within-country variances in the outcome variables and hence lowers the statistical significance, the main results hold even with those countries. Ninth, I use the counts of violent events and piracy attacks as outcome variables. As I mentioned in the theory section, the typology of substitution is based on to the dichotomized outcomes, and since the interactive effects on continuous or count outcome variables can take complicated non-linear functions, it is difficult to empirically identify the interactive effects. Consistent with these theoretical expectations, I find only weak evidence for the linear interactive effect when I use the count variables. Tenth, I also check the influence of outliers by dropping each country and re-estimating the models. The results are mostly robust to the omission of those countries. In addition, while not listed in Table 4-4, I also check multicollinearity and non-linearity, and find no indication of these problems.

Causal Mechanisms

An alternative explanation would be fishing (Hendrix and Glaser 2011; Flückiger and Ludwig 2015; Axbard 2016); rainfall decreases rebels' violence especially when ocean is windy, because rough seas mean limited opportunities for fishery industries. That is, when the ocean is rough, there might be more people who are willing to join rebels' violent activities, and hence ground rainfall could substantially affect the levels and incidence of violence. Although I am skeptical of this view as switching from non-violent (fishing) to violent activities (violence) is usually more difficult than switching between violent activities (piracy and violence), I conduct an additional analysis. I collect data on the phytoplankton absorption coefficient, which is a measure of phytoplankton abundance in the ocean (Flückiger and Ludwig 2015). Since phytoplankton abundance is a predictor of fish abundance and does not affect piracy's operational costs, I can exclude this alternative explanation if the above results hold with the phytoplankton abundance control. The phytoplankton data are derived from the MODIS Aqua products. The analysis shows that the main results hold even after controlling for phytoplankton abundance, and that it does not have a statistically significant effect on piracy or violence.[33]

One may also be interested in whether the substitution is driven by *tactical* or *monetary* incentives. As I mentioned in the theory section, the "profits" that the rebels receive can be the tactical control of territory and navigation or the monetary gains from looting and ransom. I test the causal mechanisms by disaggregating the types of rebels' violence and piracy attacks. The analysis, which is detailed in SI 3-13, provides evidence for the tactical explanation; that is, the interactive effects exist on rebels' attacks relating to tactical objectives but not on those relating to monetary gains. These results imply that there exists an upward substitution in rebels' *tactical* choices over ground and ocean military activities.

CONCLUSION

In this paper, I have developed a series of hypotheses about the effects of ground and ocean weather conditions on armed conflict and maritime piracy. A formal analysis indicates that the interactive effects can be different, depending on relative utilities of alternative strategies, namely whether maritime piracy is a downward, equivalent, or upward substitute for violence. The regression analysis shows that the empirical patterns are the most consistent with the upward

[33] See SI 3-12.

substitution hypothesis. A series of robustness checks provide further credence to the empirical findings.

An important implication of this study, which can go beyond the field of conflict studies, is that a simple regression on a weather variable can suffer the classical problems of omitted variable biases, and that conventional statistical techniques are not perfect remedies of those problems. For instance, although it is well known that election-day rainfall decreases voter turnout (Hansford and Gomez 2010), a potential backdoor path is that the rainy weather may coincide with lower temperature and freezing weather, which might in turn lower the voter turnout. In the presence of the backdoor path, we may overstate the effect of rainy weather on turnout rates. Regression techniques, such as controlling for average rainfall or including fixed effects, do not account for the backdoor path. Although the size of the correlating weather bias can be small as in this study, we need to carefully consider how climate conditions relate with each other, and whether correlating weather conditions would affect outcomes of interest.

For conflict studies, an implication is that the relationship between armed conflict and its alternative strategies can crucially depend on the type of substitution. Although recent studies examine the roles of alternative strategies, such as non-violent contestation (Cunningham 2013) and refugees as an exit strategy (Salehyan and Gleditsch 2006), they tend to presume a certain relationship of substitution. Cunningham (2013), for instance, argues that non-violent contestation does not require large-scale mobilization and hence is less costly than violent conflict. However, an interesting question is how the interpretation of the empirical findings would change if the relationship were revered and non-violent contestation became a downward or equivalent substitute for violent conflict. It is a task of future studies to expand the theory of substitution and to shed further light on the roles of alternative strategies in armed conflict.

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Appendix I. Supporting Information for “Post-disaster Reconstruction as a Cause of Intrastate Violence: An Instrumental Variable Analysis with Application to the 2004 Tsunami in Sri Lanka”

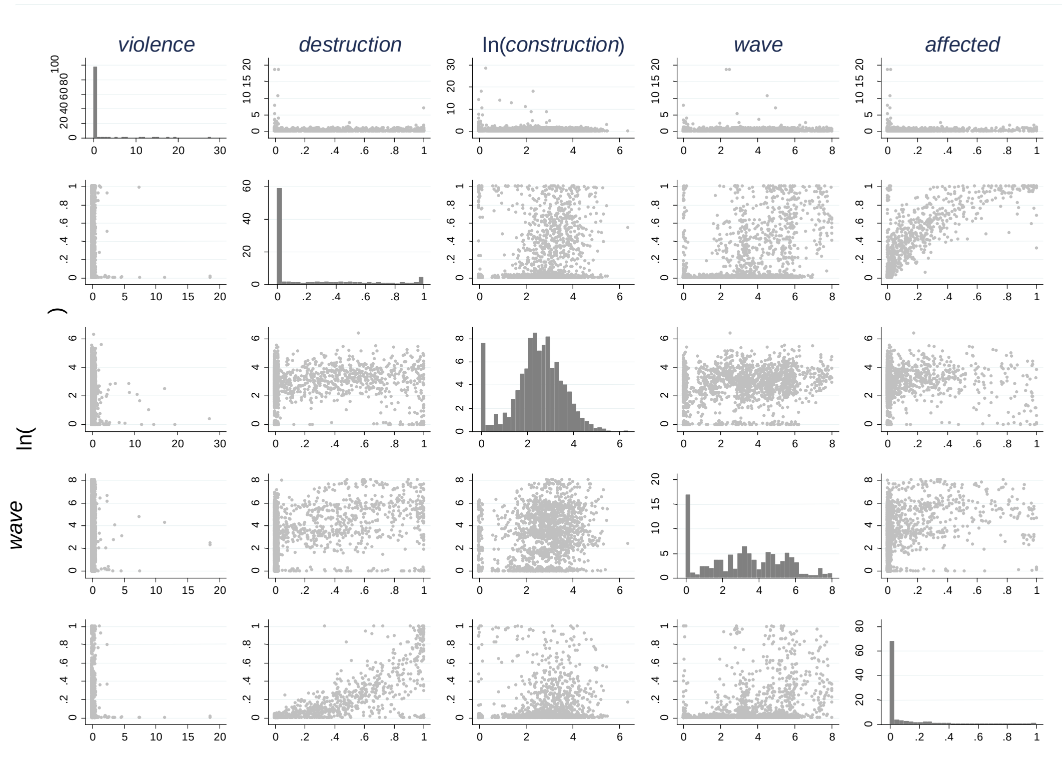
SI 1-1 DATA DESCRIPTION

The table and figure (Table A1-1-1 and Figure A1-1-1) show the descriptive statistics of the main variables. The outcome variable, *violence*, has a highly skewed distribution with an inflated number of zeroes. Reflecting the fact that the sample includes the unaffected coastal divisions, more than 60 percent of the GN (Grama Niladhari) divisions have zeroes in the proportion of destroyed homes. Because the distribution of housing construction is also highly skewed to the right, and because this variable is an outcome variable in the first-stage regression, I take a natural logarithm of the housing construction values. As seen in the figure, the sample distribution of logged *construction* is approximately normal, though there is a relatively large number of zero values. The average wave heights range from 0 to 8 meters with only a few outliers. Because of the relatively small range, I do not transform the variable into a logarithmic form. Finally, the proportion of affected people has a similar distributional form as *destruction*, and these two variables have a very high correlation (r = 0.833).

Table A1-1-1. Summary Statistics

Variables	Observations	Mean	Median	Standard deviation	Minimum	Maximum
violence	3138	0.091	0	0.941	0	28
destruction	1569	0.213	0	0.317	0	0.998
construction	3134	19.28	11.6	26.19	0	590
wave	1569	3.247	3.336	2.164	0	8.048
affected	1569	0.105	0	0.215	0	0.998

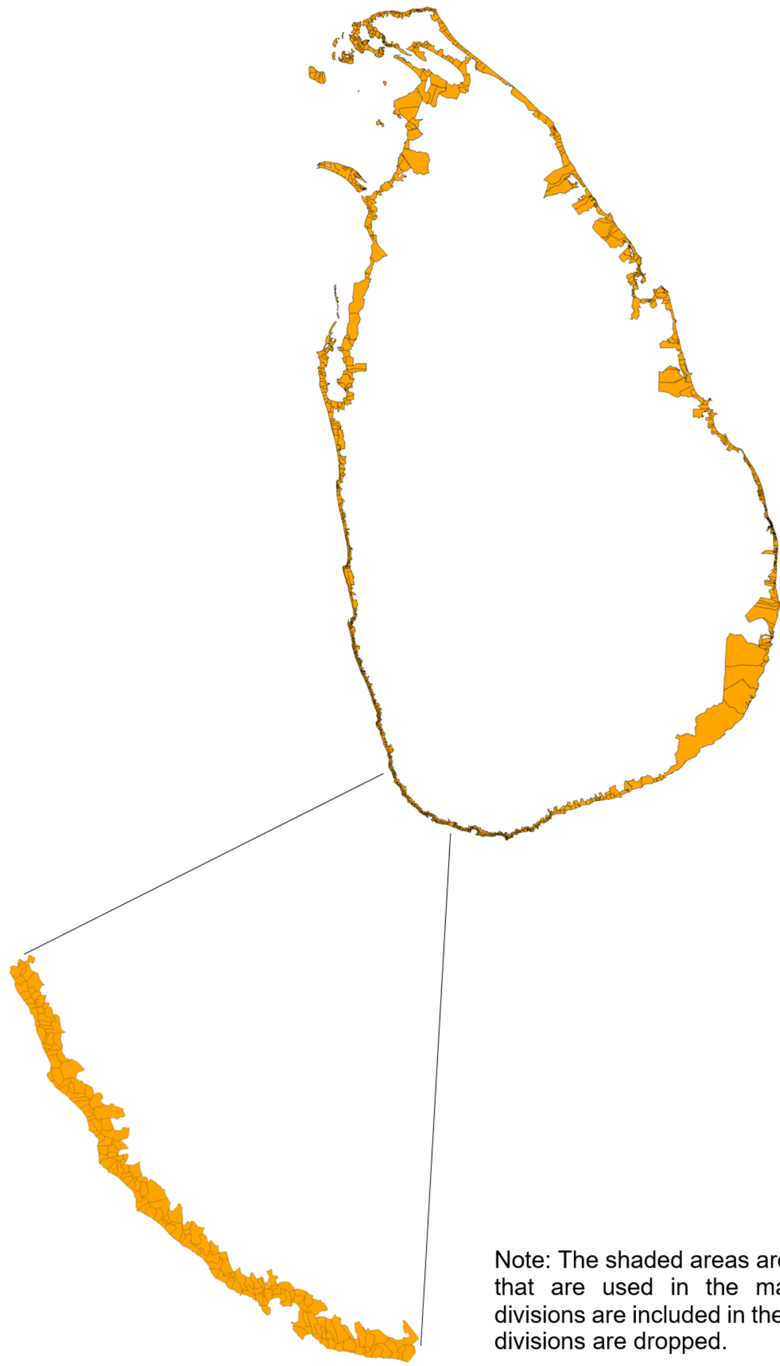
Figure A1-1-1. Scatter Plots and Histograms



Note: The non-diagonal panels are scatter plots of variables. The diagonal panels are histograms of variable.

The following figure (Figure A1-1-2) is the map of Sri Lanka. The colored regions are the coastal GN divisions that are used in the analysis, while the white regions are not used. Although the number of coastal divisions is very small relative to the number of inland divisions, it is important to keep the sample confined to the coastal region, as the inland divisions never suffer tsunamis and thus the inference cannot be extended to the inland divisions. It is substantively meaningless to estimate the effect of the tsunami and reconstruction in regions that have no possibility of suffering a tsunami.

Figure A1-1-2. Map of Sri Lanka



Note: The shaded areas are the coastal GN divisions that are used in the main analysis. All coastal divisions are included in the analysis, while the inland divisions are dropped.

SI 1-2 MEASUREMENT

Because this research uses an original, rather than an off-the-shelf, dataset, there may be some doubts on reliability.

Violent events

First, I check whether the outcome variable, a count of violent events, is sensitive to inclusion and exclusion of certain event categories. I also diagnose whether reporting bias in the conflict dataset may influence the results. To this end, following Weidmann (2016), I conduct a robustness check by dropping low-intensity violence (reported number of deaths is less than or equal to 15), as these events are more sensitive to media coverage and thus susceptible to systematic bias.

In the main analysis, I use the dataset of violent events. Table A1-2-1 provides a quick overview of the data source. I use a total count of events, which are classified as “Battle” or “Violence against Civilians” between 1 April 1995 and 26 December 2004 and between 1 January 2006 and 1 June 2009 respectively.

Table A1-2-1. Review of Violence Data

Variable name	*violence*
Dataset name	Sri Lankan Civil War dataset
Dataset provider	Yuichi Kubota
Primary data sources	Lexis Nexis Academic News (Associate Press and United Press)
Coding details	Compliant on ACLED (Raleigh, Linke, and Dowd 2014)

The following table (Table A1-2-2) shows the results when I exclude the "Violence against Civilians" event category, when I include the "Rioting/Protesting" event category, and when I drop low-intensity violence. The outcome variables are labelled as *battle*, *unrest,* and *massive*, respectively. As seen in Table A1-2-2, the exclusion and inclusion of the event categories only result in negligible changes in the coefficient estimates and standard errors, suggesting that the main findings are robust to the changes in the event categories. Although the coefficients become smaller when I drop low-intensity violence, possibly because post-disaster reconstruction relates to low-intensity violence, the coefficient value is still statistically significant.

Table A1-2-2. Estimates with Exclusion and Inclusion of the Event Categories

Outcome	(1) Δ*battle*	(2) Δ*unrest*	(3) Δ*massive*
Δln(*construction*)	0.556** (0.28)	0.590** (0.28)	0.277* (0.15)
destruction	-0.437** (0.20)	-0.482** (0.21)	-0.236** (0.12)
affected	-0.108 (0.25)	-0.111 (0.26)	-0.003 (0.13)
Constant	-0.272* (0.15)	-0.282* (0.15)	-0.143* (0.08)
Observations		1567	
Instrument		*wave*	

* p<0.10, **p<0.05, ***p<0.01.
Robust standard errors in parentheses.

I also examine the sensitivity of the results to the time periods of the violent events. In the main analysis, I exclude any events in 2004 and 2005 from the post-disaster period in order to avoid endogeneity with regards to post-disaster reconstruction. Violent events during the period of reconstruction might affect the allocation of the disaster relief, and thus compound this endogeneity problem. Nonetheless, the omission of the violent events may also raise the problem of selection bias. I then retest the hypotheses, extending the post-tsunami period to 27 December 2004 through 1 June 2009. In addition, I exclude violent events during the peace talk (22 February 2002 – 26 December 2004) so that the comparison is based on the two civil wars, and not the two periods before and after the tsunami. Because the lengths of the pre- and post-tsunami periods are different, I also standardize the number of violent events within each period and then take the difference. In this way, I can estimate the effect of reconstruction on the changes in the relative distribution of violent events. As seen in Table A1-2-3, the estimates are very similar to those in the main analysis, indicating the inclusion, exclusion, and standardization of the violent events do not alter the main findings in the paper. If an alternative violence dataset were available, I could examine the reliability of the violence data more explicitly. Because no such datasets are currently available, I cannot conduct further analyses.

Table A1-2-3. Estimates with Different Event Periods and a Standardized Outcome

	(3)	(4)	(5)
outcome	Δ*violence*	Δ*violence*	Δ*violence*
Δln(*construction*)	0.568**	0.582**	0.492*
	(0.28)	(0.28)	(0.27)
destruction	-0.487**	-0.494**	-0.435**
	(0.21)	(0.21)	(0.21)
affected	-0.099	-0.096	-0.094
	(0.26)	(0.26)	(0.22)
Constant	-0.261*	-0.268*	-0.197
	(0.15)	(0.15)	(0.14)
Observations		1567	
Instrument		*wave*	

* p<0.10, **p<0.05, ***p<0.01.
Robust standard errors in parentheses.

Housing destruction

Second, I check the measurement of housing destruction. The following table (Table A1-2-4) is a brief overview of the variable and data sources. In the main analysis, I use four indicators in the first report of the census to construct the composite index: numbers of houses classified as "completely damaged" (*level 3*), "partially damaged and cannot be used" (*level 2*), "partially damaged and can be used" (*level 1*), and "not damaged" (*level 0*). I take a sum of the former three indicators and then divide it by the sum of the four indicators. I interpolate zero for the GN divisions that were not enumerated in the Tsunami Census.[1] This calculation, however, is

[1] The Tsunami Census enumerated the GN divisions in which at least one housing unit was affected by the tsunami (Department of Census and Statistics 2005). The census covered most of

implicitly based on the assumption that the extent of housing destruction does not matter for operationalizing the concept of destruction. This assumption may be overly simplistic because heavier destruction may increase the costs of violence and therefore the rival parties may have less incentive to engage in fighting.

Table A1-2-4. Review of Housing Destruction Data

Variable name	*destruction*
Dataset name	The Tsunami Census
Dataset provider	Department of Census and Statistics, Government of Sri Lanka
Primary data sources	Door-to-door survey
Coding details	See Department of Census and Statistics (2005)

I take two approaches regarding this problem. First, I decompose the variable into three variables: proportion of housing units that suffered each of three levels of destruction. Second, I use three different weightings for operationalizing the concept of destruction. Specifically, I use the three combinations of weights described in Table A1-2-5. The first approach requires no assumption about the weights but discards the prior information that the indicators represent the same concept, while the second approach requires parametrization of the weights. The table (Table A1-2-6) shows the results. As seen in Table A1-2-6, the estimate related to the housing construction is fairly robust to the measurement of housing destruction.

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the costal divisions including the North except the northwestern coast, in which few sufferings are reported.

Table A1-2-5. Weightings for Housing Destruction

Weighting 1	*destruction 1* = 1.0**level 3* + 0.75**level 2* + 0.50**level 1*
Weighting 2	*destruction 2* = 1.0**level 3* + 0.50**level 2* + 0.25**level 1*
Weighting 3	*destruction 3* = 1.0**level 3* + 0.75**level 2* + 0.25**level 1*

Table A1-2-6. Estimates with Different Measures of Housing Destruction

	(4)	(5)	(6)	(7)
Δ*ln(construction)*	0.542*	0.545**	0.513**	0.518**
	(0.32)	(0.27)	(0.26)	(0.26)
level 1	-0.421*			
	(0.24)			
level 2	-0.300			
	(0.57)			
level 3	-0.632			
	(0.44)			
destruction 1		-0.533**		
		(0.24)		
destruction 2			-0.551**	
			(0.26)	
destruction 3				-0.545**
				(0.25)
affected	0.005	-0.029	0.012	0.005
	(0.46)	(0.29)	(0.31)	(0.31)
Constant	-0.256	-0.261*	-0.253*	-0.254*
	(0.16)	(0.15)	(0.14)	(0.14)
Observations	1567			
Instrument	*wave*			

* p<0.10, **p<0.05, ***p<0.01.
Robust standard errors in parentheses.

Housing construction I

Third, I examine the sensitivity of the results to the measurement of the housing construction. In the main analysis, I use the average number of constructed houses from 2000 to 2004 for a pre-tsunami period and a number of construction houses in 2005 for a post-tsunami period. The following table (Table A1-2-7) provides a quick description of the variable. Since the data of housing construction in 2006 is also available, I conduct additional analyses using the alternative data as well as the average of 2005 and 2006 years. To avoid endogeneity, I exclude violent events in 2006 as well as 2004 and 2005 from the post-tsunami period. The coefficient of the alternative housing construction variable becomes relatively smaller, though the result of the statistical test is the same.

Table A1-2-7. Review of Housing Construction Data

Variable name	*construction*
Dataset name	The 2012 Census
Dataset provider	Department of Census and Statistics, Government of Sri Lanka
Primary data sources	Door-to-door survey
Coding details	See (Department of Census and Statistics forthcoming)

Table A1-2-8. Estimates with Alternative Housing Construction

	(8)	(9)
Δln(*construction 2006)*	0.460**	
	(0.22)	
Δln(*construction 2005-06)*		0.520**
		(0.25)
destruction	-0.388**	-0.426**
	(0.17)	(0.19)
affected	-0.363	-0.372
	(0.34)	(0.34)
Constant	-0.135	-0.219*
	(0.08)	(0.12)
Observations	1567	
Instrument	*wave*	

* p<0.10, **p<0.05, ***p<0.01.
Robust standard errors in parentheses.

Housing construction II

Another issue regarding the housing construction data is its validity as a measurement of *re*-construction. Furthermore, it is important to demonstrate that the housing construction data, which are available at the GN division level, represent *re*-construction even after controlling for the extent of housing destruction. Although the number of houses that are reconstructed in the post-tsunami recovery programs are not available at the level of GN divisions (fourth administrative level), they are available at the level of DS (Divisional Secretariat) divisions (third administrative level). The post-tsunami housing reconstruction effort was driven by donor-driven and owner-driven housing construction (DHC and OHC respectively) programs (see SI 1-6 for details). The *DAD Data Report 2nd edition* that was made by the Reconstruction and Development Agency (RADA) on 15 May 2006 reports the progress status of the DHC programs at the DS divisions (third administrative units).[2] In particular, the report contains the numbers of units at four stages: (1) an original Memorandum of Understanding (MoU) was made, (2) the MoU was actually signed, (3) the housing units were under construction, and (4) the construction was completed. Based on the report, I create four corresponding variables: *planned*, *signed*, *started*, and *completed*. Note that these variables are not mutually exclusive. For example, *planned* is the sum of *signed*, *started*, and *completed* plus those whose MoU was created but not yet signed. Because these variables are available only at the DS-division level, I also recalculate the differences in the numbers of constructed houses (Δ*construction*), which is recorded in the 2012 census, and the proportions of destroyed houses (*destruction*), which is available in the 2004 Tsunami Census, at the level of DS divisions. I then separately regress Δ*construction* to *planned*, *signed*, *started*, and *completed* with control for *destruction*.

If Δ*construction* measures housing reconstruction even after controlling for housing destruction, the coefficients of *planned*, *signed*, *started*, and *completed* should be positive. In addition, because Δ*construction* captures only the houses that were planned and actually constructed, it should have a stronger relationship with the reconstruction variables as construction progresses from the initial plan to completion.

[2] The information on the OHC programs, which were managed by the Sri Lankan government, is not available even at the DS level. In the next section of this SI, I examine the possible effect of the OHC programs on conflicts.

Table A1-2-9. Regression of Housing Construction on Housing Reconstruction

	Δconstruction			
planned	0.194* (0.12)			
signed		0.166 (0.12)		
started			0.636** (0.29)	
completed				0.772* (0.44)
destruction	193.7 (233.7)	234.2 (227.8)	319.4 (194.1)	395.3** (176.5)
constant	249.5** (49.5)	263.3** (51.3)	224.0** (52.2)	240.2*** (50.6)
Observations	86			

* p<0.10, **p<0.05, ***p<0.01.
Robust standard errors in parentheses.

The results in Table A1-2-9 are congruent with these expectations. Even though the sample size is very small ($n = 86$), most of the coefficients are statistically significant and positive. Furthermore, the coefficient values increase as housing reconstruction proceeds from the MoU to completion. There is a large gap in the coefficient values between *signed* and *started*. This reflects the fact that the signature of MoU does not necessarily entail housing construction, while starting construction almost certainly means the completion of the project. Although the coefficient values are not substantively large because *Δconstruction* includes housing construction unrelated to the tsunami, the results strongly support the validity of the measurement. Note also that because the number of observations is very limited, I cannot apply the instrumental variable analysis—which has a large bias in a small sample—to the DS-division level.

Housing construction III

A remaining issue regarding the housing construction data is that even though my housing construction measure is associated with the number of houses that were reconstructed by the DHC (Donor-driven Housing Construction) programs, we are still unclear about the effect of the OHC (Owner-driven Housing Construction) programs. Because the benefits distributed by both programs tended to favor the government-held regions and can potentially shift the power balance (as I explain in SI 1-6), they are equally relevant to my theory. Furthermore, one may think that the donors are more concerned with the balance of power while the Sri Lankan government, which managed the OHC programs, may favor its constituencies. If this is the case, the OHC and DHC programs may have different or even opposite effects on conflicts.

To account for possible heterogeneity, I conduct two robustness checks. In the first model, I use ln(Δ*construction*) that is unexplained by the DHC programs. In particular, I first run a regression:

$$\ln(\Delta construction_i) = \gamma_0 + \gamma_1 planned_{i,j} + \gamma_2 signed_{i,j} + \gamma_3 started_{i,j} + \gamma_4 completed_{i,j} + e_i,$$

where i and i refer to the GN and DS divisions respectively, and *planned*, *signed*, *started*, and *completed* are the variable related to the DHC programs (see the previous subsection for details). Using the residuals in this regression—which are intuitively interpreted as the variation of $\ln(\Delta construction_i)$ that is not explained by the DHC housing construction—as an explanatory variable, I estimate the instrumental variable regression. In the second model, I add *planned*, *signed*, *started*, and *completed* as additional control variables in order to account for the effects of the DHC programs.

Table A1-2-10. Estimates with Controls for the DHC programs

	(10)	(11)
non-DHC Δln(*construction)*	1.007*	
	(0.54)	
Δln(*construction)*		0.651*
		-0.35
destruction	-0.647**	-0.550**
	(0.31)	-0.24
affected	-0.173	-0.094
	(0.32)	-0.27
Constant	0.124*	-0.294*
	(0.06)	-0.17
Observations	1567	
Instrument	*wave*	
Controls for DHC programs	No	Yes

* p<0.10, **p<0.05, ***p<0.01.
Robust standard errors in parentheses.

As seen in Table A1-2-10, the main results are maintained. The first column shows the effect of the residuals (*non-DHC*) on the conflict risk, while the second column shows the model with the additional controls for the DHC programs. In both regressions, the explanatory variables (non-DHC construction and Δln(*construction)*) are instrumented by the tsunami wave heights. The results suggest that the adverse effect of reconstruction is not limited to the DHC programs, and that the other housing construction, including that done by OHC programs, also has a similar impact on the conflict risk.

Tsunami wave heights

I also rerun the analysis with different data of the wave heights. In the main analyses, I use the simulated data of wave heights provided by Garcin et.al (2008). The potential data sources for wave heights are either numerical simulations or field observations (Wijetunge 2009; Goff et.al 2006; Liu et.al 2005). Although field observations are potentially useful, they only provide information about runup and inundation, and their geographic coverage is limited. I requested data access from six authors who conducted wave height simulations (Poisson, Oliveros, and Pedreros 2009; Garcin et.al 2008; Grilli et.al 2007; Titov et.al 2005; Tomita et.al 2006; Imamura 2004).[3] Out of the six authors, Garcin and Imamura kindly provided the data from their wave high simulations. Because the Garcin at.al (2008) provide data of higher geographical resolution, I use their wave height estimation as a primary data source, and the data from Imamura (2004) in a robustness check.

The grid resolution of Garcin et.al (2008) is 540 meters. In order to capture the elevation of the first wave, I only use the data cells surrounding the Ceylon Island, trimming cells in the outer sea. Then, I calculate the average wave heights of a GN division within the surrounding five kilometers. The polygon data of the administrative boundaries are derived from the Office for the Coordination of Humanitarian Affairs (OCHA; 2015). The following table (Table A1-2-11) is a summary of the data.

[3] The six studies are the all of the studies which I could find that simulated the maximum amplitudes of the 2004 tsunami in Sri Lanka.

Table A1-2-11. Review of Tsunami Wave Height Data

Variable name	*wave*
Dataset name	Integrated Approach of Coastal Hazard and Risk in Sri Lanka
Dataset provider	Manuel Garcin
Primary data sources	Estimated seismic source (Grilli et al. 2007; Vigny et al. 2005)
	Bathymetry (U.S. Department of Commerce 2006)
	Sources provided by Survey Department of Sri Lanka and National Hydrographic Office (references are not provided)
	Wave simulation: Modified GEOWAVE (Watts et al. 2001)
Coding details	See Garcin et al. 2008

For checking the robustness of the main results, I estimate the same regression models but use different data for the wave heights. The alternative data for the wave heights are obtained from Imamura (2004). While the data of Garcin et.al is available at 540 meter grid resolution, the resolution of Imamura's simulation is 2 minute grids, approximately 4 kilometer grids, which may lower the explanatory power of the instrumental variable. As seen in Table A1-2-12, the robustness of the result to the wave height data. In fact, the effect size of the housing construction becomes even larger. Furthermore, contrary to my expectation, the alternative instrument also has high explanatory power (the F statistics of weak instruments is 35.3). Thus, it appears that the findings are robust to the measurement of the wave heights.

Table A1-2-12. Estimates with Different Wave Height Data

	(11)
Δln(*construction)*	0.503**
	(0.23)
destruction	-0.438**
	(0.21)
affected	-0.080
	(0.23)
Constant	-0.237***
	(0.12)
Observations	1567
Instrument	*wave*

* *p*<0.10, ***p*<0.05, ****p*<0.01.
Robust standard errors in parentheses.

GN divisions up to 5 or 10 kilometers away from the coast

Finally, to confirm the findings' robustness to sample selection, I include GN divisions within 5 and 10 kilometers away from the coast (the analysis in the main paper uses GN divisions that are within one kilometer from the coastline). As shown in Table A1-2-13, the large sample size increases the statistical significance, while the coefficient values do not change substantially. These findings indicate that the results are robust to the selection of GN divisions.

Table A1-2-13. Estimates with Larger Sample Sizes

	(12)	(13)
Δln(*construction)*	0.566***	0.432***
	(0.19)	(0.16)
destruction	-0.481**	-0.420**
	(0.20)	(0.19)
affected	-0.099	-0.049
	(0.24)	(0.23)
Constant	-0.263***	-0.189***
	(0.09)	(0.07)
Maximum distance from the coast	5 km	10 km
Instrument	*wave*	
Observations	3067	4241

* p<0.10, **p<0.05, ***p<0.01.
Robust standard errors in parentheses.

SI 1-3 ADDITIONAL CONTROL VARIABLES

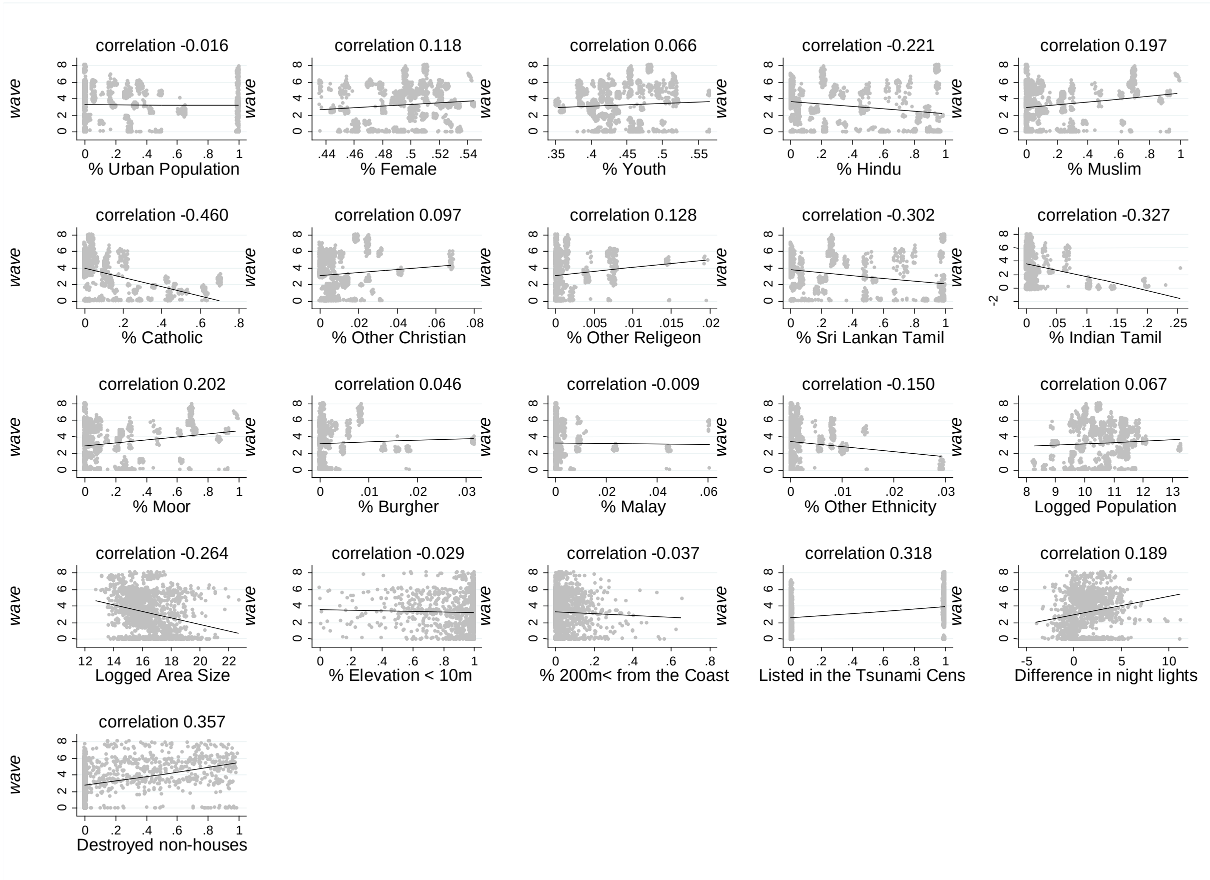
The tsunami wave heights, the instrumenting variable, are not perfectly random and therefore may violate the independence assumption of the instrumental variable analysis. In particular, if the wave heights correlate with some confounders, it could result in substantial bias in the estimates. To examine this possibility, I collect various pre-treatment covariates which are available in the 1981 Census and GIS data sources. The 1981 Census is the last census before the onset of the Eelam III War, and provides a wide range of demographic information across the country. I choose all entries that are available at a DS division level (the third administrative level). I supplement the demographic data with geographic variables, which are derived from GIS sources. I also add the number of destroyed houses in the Tsunami Census. The following table (Table A1-3-1) is the description of the covariates, and Figure A1-3-1 shows the correlation terms and scatter plots of the wave heights with the covariates. The wave heights correlate with several covariates; the waves tended to be particularly high in the locations with lower Hindi and Catholic populations, fewer Tamil and greater Moor ethnicities, and large areas with a higher elevation. If these covariates could also relate to the outcome variable, differences in the counts of violent events before and after the tsunami, then the estimates would be biased.

Table A1-3-1. Summary of the Covariates

Variable	Description	Precision	Data Source	n	Mean	S.D.	Min	Max
urban	% of urban population	DS division	1981 Census	1508	0.36	0.40	0.00	1.00
female	% of female population	DS division	1981 Census	1508	0.50	0.03	0.44	0.54
youth	% of youth population (under 18 years old)	DS division	1981 Census	1508	0.44	0.04	0.35	0.57
hindu	% of Hindu population	DS division	1981 Census	1508	0.26	0.33	0.00	0.98
muslim	% of Muslim population	DS division	1981 Census	1508	0.18	0.25	0.00	0.98
catholic	% of Catholic population	DS division	1981 Census	1508	0.12	0.18	0.00	0.70
christian	% of other Christian population	DS division	1981 Census	1508	0.01	0.01	0.00	0.07
other religion	% of other religions	DS division	1981 Census	1508	0.00	0.00	0.00	0.02
sri lankan tamil	% of Sri Lankan Tamil population	DS division	1981 Census	1508	0.31	0.37	0.00	1.00
indian tamil	% of Indian Tamil population	DS division	1981 Census	1508	0.02	0.03	0.00	0.26
moor	% of Moor population	DS division	1981 Census	1508	0.18	0.25	0.00	0.98
burgher	% of Burgher population	DS division	1981 Census	1508	0.00	0.01	0.00	0.03
malay	% of Malay population	DS division	1981 Census	1508	0.00	0.01	0.00	0.06
other ethnicity	% of other ethnicities	DS division	1981 Census	1508	0.00	0.01	0.00	0.03
ln(*population*)	log of total population	GN division	1981 Census	1508	10.67	0.90	8.29	13.28
ln(*area*)	log of area sizes	GN division	OCHA	1569	16.37	1.46	12.81	22.74
lowland	% of lowland areas (under 10 meters)	GN division	OCHA	1498	0.15	0.22	-0.31	2.33
coast	% of coastal areas (within 200 meters)	GN division	OCHA	1569	0.05	0.08	0.00	0.65
enumerated	Whether listed in the Tsunami Census	GN division	Tsunami Census	1569	0.50	0.50	0.00	1.00
night light	Change in the night light densities, 2003-05	GN division	NOAA	1669	1.41	1.82	-4	11.25
nonhouse	N of destroyed non-housing units	GN divisions	Tsunami census	1569	0.18	0.30	0	0.98

% of Buddhist population and % of Sinhalese population are base categories for religion and ethnicity variables, and thus omitted.

Figure A1-3-1. Scatter Plots of Wave Heights and Covariates



Note: Each panel shows a scatter plot and a fitted line of a covariate and the wave height variable. The correlation coefficient is also shown at the top of each panel.

To explore this possibility, I add fixed effects of the districts (the second administrative level) and provinces (the first administrative level), and many observed covariates respectively. This approach can be effective because potential confounders may be accounted for by the fixed effects or control variables. The cost of this approach is that the explanatory variables may also have less variation and therefore diminish the precision of the estimation. Hence, if the results hold even after including the fixed effects, it should be considered to be equally strong support for the hypotheses, though the lack of statistical significance may be either due to potential confounders or due to lower statistical power.

As seen in Table A1-3-2, once I include either of the fixed effects in the main models (Table 1-1 Model 4 in the paper), the coefficient for housing construction becomes indistinguishable from zero. The standard errors are very large relative to the coefficient values. Even worse, the coefficients of housing construction are opposite to the main result. Thus, the models of the regional fixed effects do not provide any additional evidence for the main findings. The results in Table A1-3-2 also show that the addition of the covariates also makes the p-value above the conventional threshold level ($p = 0.152$). Note that 61 observations are omitted due to missing data.

Table A1-3-2. Estimates with the Regional Fixed Effects

	(1)	(2)
Δln(*construction*)	-0.333	-0.186
	(0.23)	(0.43)
destruction	-0.045	-0.097
	(0.17)	(0.18)
affected	0.244	0.123
	(0.20)	(0.28)
Constant	0.235*	0.230
	(0.14)	(0.30)
Fixed Effects	13 Districts	4 Provinces
Instrument	*wave*	
Observations	1567	

* p<0.10, **p<0.05, ***p<0.01.
Robust standard errors in parentheses.

Table A1-3-3. Estimates with the Observed Covariates

	(3)	
Δln(*construction*)	0.558	(0.39)
destruction	-0.656	(0.32)*
affected	0.026	(0.31)
urban	0.118	(0.09)
female	-2.109	(2.35)
youth	2.017	(1.75)
hindu	0.042	(0.41)
muslim	0.048	(2.56)
catholic	0.141	(0.22)
christian	-2.593	(2.52)
other religion	1.393	(10.29)
sri lankan tamil	-0.206	(0.40)
indian tamil	2.421	(1.24)*
moor	-0.280	(2.64)
burgher	11.110	(11.39)
malay	-0.163	(4.59)
other ethnicity	-6.794	(9.14)
ln(*population*)	0.021	(0.03)
ln(*area*)	-0.072	(0.04)
lowland	0.244	(0.33)
coast	0.573	(0.71)
enumerated	-0.017	(0.14)
Δ*night light*	-0.001	(0.01)
non-house	0.152	(0.26)
Constant	0.624	(1.77)
Instrument	*wave*	
Observations	1506	

* $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Robust standard errors in parentheses.

A question is therefore whether the loss of statistical significance is due to potential biases in the estimates or losses in the power of analysis. At first glance, the observed covariates only cause efficiency loss. In fact, only one of the 21 variables is near the conventional threshold of statistical significance, which is not surprising considering the nature of multiple hypothesis testing. Moreover, the covariates not only individually, but also jointly (p = 0.71), have no statistically significant association, suggesting redundancy of these covariates.

To further examine this problem, I conduct an additional analysis. I increase the sample size by including GN divisions up to 5 or 10 kilometers away from the coast. If the fixed effects and additional covariates cause efficiency losses but do not bias the estimates, a larger sample size should address the problems. In contrast, if the fixed effects would represent confounders, they should bias the estimates and p-values *regardless of the sample sizes*. As Table A1-3-4 shows, in most cases, the coefficients for Δln(*construction*) regain statistical significance. Only when I use the 5 km threshold and include the observed covariates does the statistical significance fall below the conventional criterion ($p = 0.133$). This is because the missing values in the covariates do not allow the sample to increase to a sufficient size. Furthermore, all coefficient values are within the confidence interval of the main estimates. Taken together, the results imply that the increased power of analysis is sufficient to recover the main estimates.

Table A1-3-4. Estimates with Controls and Larger Sample Sizes

	(4)	(5)	(6)	(7)	(8)	(9)
Δln(*construction*)	0.213*	0.291**	0.313	0.224*	0.295**	0.348*
	(0.11)	(0.13)	(0.21)	(0.12)	(0.12)	(0.21)
destruction	-0.356**	-0.293*	-0.507*	-0.436**	-0.308*	-0.568*
	(0.16)	(0.17)	(0.29)	(0.18)	(0.17)	(0.30)
affected	0.178	-0.029	0.117	0.270	-0.016	0.140
	(0.20)	(0.22)	(0.27)	(0.23)	(0.22)	(0.28)
Constant	-0.065	-0.107*	2.571	-0.073	-0.129**	2.522
	(0.04)	(0.06)	(2.03)	(0.05)	(0.06)	(2.00)
Controls	District FE	Province FE	Covariates	District FE	Province FE	Covariates
Distance from the coast		5 km			10 km	
Instrument				*wave*		
Observations	3067		2488	4241		2784

* *p*<0.10, ***p*<0.05, ****p*<0.01.
Robust standard errors in parentheses.

Additional Controls

One possibility is that the tsunami wave heights affect forms of development other than housing reconstruction. Furthermore, the locational changes of violent events and housing construction may correlate with the early movements of the armed forces. Although these variables are unlikely to correlate with tsunami wave heights and hence to bias the estimates in the instrumental variable analysis, it is nevertheless prudent to conduct an additional analysis. I therefore include the changes in the night light densities, the number of destroyed non-housing units, and the number of violent events in 2005 as additional control variables. The differences in the night light densities are supposed to capture the development other than housing reconstruction. Because I am using the difference in the numbers of violent events before 2004 and after 2006, the early violent events can capture the location changes of the armed forces in the tsunami's immediate aftermath.[4] As seen in Table A1-3-5, the results are robust to the inclusion of the additional control variables. Since early violence positively correlates with later violence, it is not surprising that the coefficient for early violence is positive and statistically significant.

Table A1-3-5. Night Light Densities, Non-housing Destruction, and Early Violence

	(10)	(11)	(12)
Δln(*construction)*	0.551**	0.595**	0.562**
	(0.27)	(0.28)	(0.27)
destruction	-0.604**	-0.486**	-0.601**
	(0.27)	(0.21)	(0.27)
affected	-0.099	-0.113	-0.103
	(0.26)	(0.26)	(0.26)
Δ*night light*	0.007		0.009
	(0.01)		(0.01)
non-house	0.164		0.164
	(0.24)		(0.24)
early violence		1.843***	1.836***
		(0.20)	(0.19)
Constant	-0.269*	-0.284*	-0.283*
	(0.15)	(0.15)	(0.15)
Observations		1567	
Instrument		*wave*	

* p<0.10, **p<0.05, ***p<0.01.
Robust standard errors in parentheses.

[4] I suspect that one of the additional controls, *early violence*, is potentially detrimental. Because my theory predicts that early violent events are also endogenous to housing construction and tsunami wave height, the inclusion of early violence will introduce endogeneity biases in the estimates. It is thus theoretically and methodologically untenable to include early violence as an exogenous variable, which is why I did not use this variable in the above long regression.

SI 1-4 SPATIAL DEPENDENCE

Spatial dependence poses another threat to causal inference. To mitigate this issue, I separately include averages of the explanatory and outcome variables in neighboring locations as control variables. I do not include the neighbors' average of the instrumental variable, for it highly correlates with the instrument and the collinearity substantially lowers the instrument's explanatory power. As seen in Table A1-4-1, inclusion of the neighbors' variables results in point estimates that are farther from zero, though the standard errors also tend to be larger. In the first two models, the effect sizes of housing construction become larger, though they fail to reach the conventional threshold of statistical significance (p = 0.091 and 0.084) probably due to the collinearities to the spatial variables. Because neither of the two spatial controls has a statistically significant association with the outcome variable, I consider these variables to be redundant. Admittedly, the specifications of the spatial dependence here are primitive and I leave further exploration of the spatial dependence to future analyses.

Table A1-4-1. Estimates with Neighbors' Averages

	(1)	(2)	(3)
Δln(*construction*)	0.926*	0.849*	0.611**
	(0.55)	(0.49)	(0.30)
destruction	-0.450**	-0.688**	-0.507**
	(0.23)	(0.32)	(0.22)
affected	-0.250	-0.155	-0.107
	(0.35)	(0.31)	(0.26)
neighbors' *destruction*	-0.566		
	(0.46)		
neighbors' Δln(*construction*)		-0.092	
		(0.07)	
neighbor's Δ*violence*			0.120**
			(0.06)
Constant	-0.367	-0.123	-0.294*
	(0.22)	(0.10)	(0.16)
Observations		1567	
Instrument		*wave*	

* *p*<0.10, ***p*<0.05, ****p*<0.01.
Robust standard errors in parentheses.

I also check the estimates of clustered standard errors to account for the effect of spatial dependence on the standard error estimates. As shown in Table A1-4-2, the results hold whether I cluster the standard error by the DS divisions, districts, or provinces.

Table A1-4-2. Estimates with Clustered SE

	(1)	(2)	(3)
Δln(*construction*)	0.577*	0.577*	0.577*
	(0.33)	(0.35)	(0.32)
destruction	-0.487***	-0.487**	-0.487*
	(0.09)	(0.21)	(0.27)
affected	-0.104	-0.104	-0.104
	(0.29)	(0.24)	(0.20)
Constant	-0.269	-0.269	-0.269
	(0.19)	(0.20)	(0.17)
Clustered SE	DS	District	Province
Instrument		*wave*	
Observations		1567	

* $p<0.10$, **$p<0.05$, ***$p<0.01$.
Robust standard errors in parentheses.

SI 1-5 SPECIFICATION AND ESTIMATION

Due to the nature of the data and identification strategy, the model specifications and estimations can take several alternate forms.

Linear models

First, I re-estimate the linear models with different specifications and estimations following the advice of Angrist and Pischke (2009). In particular, I report the two-stage least square (2SLS) estimates, limited information maximum likelihood (LIML) estimates with an additional instrumental variable, and ordinary least square (OLS) estimates of the reduced form. The GMM estimate, which is reported in the main analysis, is asymptotically consistent and efficient in the presence of non-constant error variance, while the 2SLS estimate is unbiased but less efficient. The LIML estimate is less precise than the 2SLS but robust to the finite sample bias. In addition, the LIML estimate of the overidentified model allows for a validity test of the instruments, assuming either one of the instruments is valid. I use the standard deviation of the wave heights as an extra instrument. Finally, the OLS estimate of the reduced form is unbiased and proportional to the LATE. The following table (Table A1-5-1) provides a summary of these estimates.

Table A1-5-1. Estimates with Different Specification and Estimation (Linear)

	(1)	(2)	(3)
Δln(*construction)*	0.577**	0.506*	
	(0.28)	(0.26)	
wave			0.030**
			(0.01)
destruction	-0.487**	-0.441**	-0.182
	(0.21)	(0.20)	(0.14)
affected	-0.104	-0.081	0.063
	(0.26)	(0.25)	(0.22)
Constant	-0.269*	-0.239*	-0.098
	(0.15)	(0.14)	(0.07)
Estimation	2SLS	LIML	OLS
Instrument	*wave*	*wave* *wave sd*	
Observations		1567	

* p<0.10, **p<0.05, ***p<0.01.
Robust standard errors in parentheses.
wave sd is the standard deviation of wave heights.

As shown in Table A1-5-1, both the 2SLS and LIML estimates are similar to those of the GMM reported in the main paper. In the first stage of the LIML estimates of the overidentified model, the mean of the wave heights is still positively associated with the housing construction variables ($p < 0.001$), while the standard deviation of the wave heights negatively correlates with the explanatory variable ($p = 0.051$). Theoretically, this could be due to people judging the locations with lower wave height variance to be safer and moving there. The predictive power of these instruments is lower than that of the just-identified model but still beyond the conventional criteria (the F statistics of weak instruments is 23.6). More importantly, the Sargan over-identification test does not reject the validity of the instrument at a 5 percent significance level.

Finally, in the reduced form regression, the sign of the wave height variable is consistent with the GMM estimate. These results suggest that the estimates of the linear models are robust to changes in specifications and estimations.

Non-linear models

Second, because the outcome variable is not continuous, I examine the robustness of non-linear models. Estimation methods for non-linear instrumental variable regressions are still under development and there is not yet a consensus on which perform best. In addition, violent events are fairly rare, which creates further complications. I first fit the ordered and multinomial probit models, treating the decrease, no change, and increase of the violent events as an ordinal and nominal scale respectively. Next, I employ a control function approach to estimate separate probit models (one for *no change* versus *decrease* and another for *no change* versus *increase* of violent events). Finally, I estimate separate rare-event logit models using the two-stage residual inclusion (2SRI) techniques.

Table A1-5-2. Estimates with Different Specification and Estimation (Non-linear)

Outcome	(4) Trichotomized Δ*violence*	(5) Trichotomized Δ*violence*		(6)	(7)	(8)	(9)
		decrease	*increase*	*decrease*	*increase*	*decrease*	*increase*
Δln(*construction*)	1.060**	-4.133***	-2.538**	-1.420***	-1.283***	-6.797***	-3.767*
	(0.51)	(1.06)	(1.19)	(0.11)	(0.38)	(1.77)	(2.19)
destruction	-1.014**	2.699***	0.569	0.958***	0.155	4.429***	0.857
	(0.43)	(0.68)	(1.23)	(0.22)	(0.69)	(1.02)	(2.36)
affected	0.021	1.449*	1.989**	0.457	1.130*	2.394*	2.848*
	(0.50)	(0.85)	(0.89)	(0.36)	(0.61)	(1.30)	(1.50)
First-stage residual	-1.142**	3.998***	1.962			6.624***	2.797
	(0.51)	(1.07)	(1.20)			(1.81)	(2.20)
Constant		-1.038**	-1.891***	-0.391	-1.189*	-0.861	-2.463***
		(0.44)	(0.50)	(0.29)	(0.61)	(0.74)	(0.91)
Model	Ordered Probit	Multinomial Probit		Instrumental Variable Probit		Rare-event Logit	
Instrumental Variable Approach		2SRI		Control Function		2SRI	
Observations				1567			

* $p<0.10$, **$p<0.05$, ***$p<0.01$.
Robust standard errors in parentheses.

As seen in Table A1-5-2, the estimates of the ordered probit models are consistent with both of the hypotheses, though the proportional odds assumption is rejected ($p < 0.001$). Thus, I also estimate the multinomial probit models and separate probit and rare-event logit models. The results of these models indicate that reconstruction relates to whether the conflicts decrease or not, rather than whether the conflicts increase or not. These results provide further support for the theory, as my interest in this study is the distribution of violent events across the country rather than the absolute number of violent events in each individual location. The fact that housing construction relates to a decrease and non-decrease, instead of increase and non-increase, may indicate that a natural disaster relates to general pacification and therefore a reduction of violent events in absolute terms, as the “pacifying effect” thesis in the literature suggests (Slettebak 2012). This interpretation should nonetheless be balanced with the fact that a civil war, not peace, actually followed the tsunami in Sri Lanka.

SI 1-6 QUALITATIVE ASSESSMENT

In this section, I provide qualitative evidence for the causal mechanisms, especially focusing on whether post-tsunami housing reconstruction provided the government and Liberation Tigers of Tamil Eelam (LTTE) with opportunities to obtain direct benefits, and to expand sources of taxes and manpower. I first describe the scheme and distributions of the tsunami aid. I then discuss how the aid flows contributed to differential power growths of the government and LTTE. Finally, I look at the Post-Tsunami Operational Management Structure (P-TOMS) as an example of a bargaining failure.

Note that the qualitative assessment is not a substitute for in-depth case studies. Because relevant sources are still classified, it is not currently possible to examine the dynamics of power shifts, commitment problems, and the locational choices for battles. Future research will likely conduct in-depth qualitative analyses once the information becomes public.

Tsunami Relief

Soon after the tsunami on 26th of December 2004, the government and the LTTE separately established aid coordination bodies. Within a month, the government set up the Task Force for Rebuilding the Nation (TAFREN), which evolved into the Reconstruction and Development Agency (RADA) in November 2005, while the LTTE coordinated the post-disaster reconstruction through the existing organizations, including the Planning and Development

Secretariat (PDS) and the Tamil Rehabilitation Organization (TRO). There were international and domestic efforts to coordinate the government and rebel bodies, as seen in the agreement of the Post-Tsunami Operational Management Structure (P-TOMS), but the agreement ultimately failed to achieve its goal. Because the information about the PDS and TRO is not available (due in part to the downfall of the LTTE), I focus on the government side.[5]

The government implemented the Owner-driven Housing Construction (OHC) programs and the Donor-driven Housing Construction (DHC) programs. While the OHC programs were funded by foreign states and regional organizations and implemented through the government, the DHC programs were directly managed by the donors with cooperation of national and local agencies. In terms of the committed amounts of aid, the OHC and DHC programs were roughly equal (41 and 50 million dollars in 15 May 2006 respectively; RADA 2006).

In the DHC programs, the donors signed the Memorandum of Understanding (MoU) with housing owners, and then built houses with or without subcontracts. On 15th May 2006, the DHC programs included 469 projects, 35,048 MoUs, 9,319 units under construction, and 5,979 houses. Of the 35,048 MoUs, nongovernmental organizations (NGOs) and intergovernmental organizations (IGOs) accounted for 31,891 (91%), while governmental agencies signed the rest. Although it is unlikely that the government and LTTE misappropriated funding from the NGO-managed programs, that does not mean that the DHC programs distributed resources to the government- and rebel-controlled areas proportionally to needs. Indeed, while only 6,274 MoUs

[5] The TRO did not manage a large amount of disaster relief. For instance, 18 months after the tsunami, the TRO managed to build 836 permanent houses, significantly fewer than that of the RADA even accounting for its narrower regional coverage (TRO 2006).

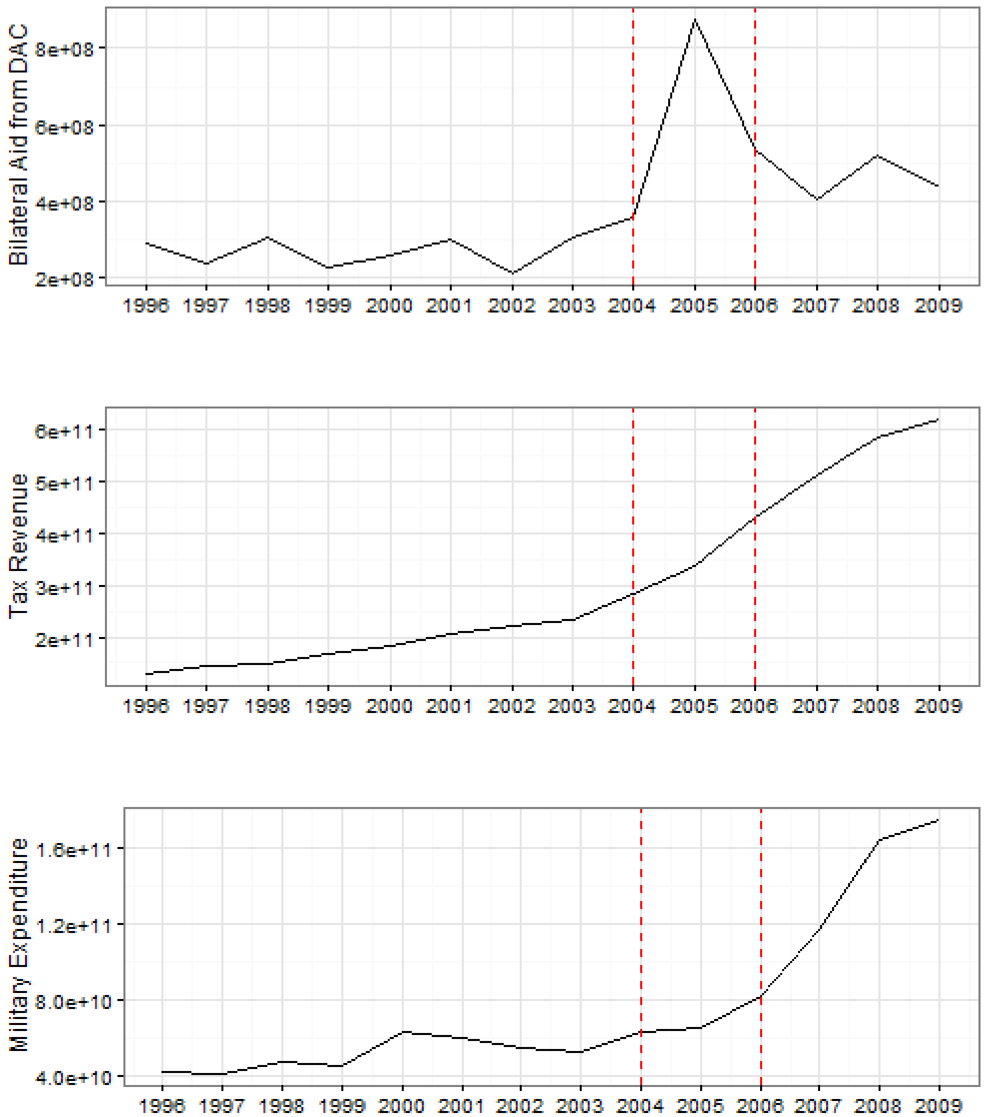
were signed in the LTTE-held districts (Jaffna, Kilinochchi, and Mullaitivu), the government-held districts saw over 28,000 MoUs by 15 May 2006 (RADA 2006).

In contrast to the DHC programs, the OHC programs were implemented through the government. The donors supplied financial resources to the government, and then the government allocated cash to the house owners. Officially, the amount of payment was 250,000 and 100,000 rupees with four and two installments for fully and partly damaged houses respectively. Although the exact numbers are unavailable, the OHC programs are reported to have been mostly funded by foreign governments and IGOs (RADA 2006). In addition, although the government could have allocated the OHC programs to the LTTE areas so as to mitigate the differential growth, the government decisions were substantially constrained by the local politics of disaster relief. Local politicians were reported to attract programs to their constituencies and hence to gain support from their respective publics (Frerks and Klem 2011; Moonesinghe 2007). By October 2005, while only 3,128 owners received the first installments in the LTTE-controlled districts (Jaffna, Kilinochchi, and Mullaitivu), the payment was made for 48,154 owners in the government-held districts (RADA 2006).

Implications for Power Balance

It appears that the disproportionate allocation of disaster relief allowed the government to expand its military capability with a short time lag. Although post-disaster reconstruction could influence the military power directly or indirectly, I focus on the indirect mechanism because the direct effects, including misappropriation of aid and fees on aid activities, are hard to observe.

Figure A1-6-1. Aid, Tax Revenue, and Military Expenditure



Note: The amounts of bilateral aid inflows, tax revenue, and military expenditure in current LCU (World Bank 2015). The red dotted lines indicate the years of the natural disaster and the beginning of the Eelam IV War. The Eelam IV War ended in 2009.

As seen in the first pane of Figure A1-6-1, the 2004 Tsunami caused an upward spike in the aid inflow, which stayed at an elevated level until the end of the Eelam IV War. The influx of disaster aid stimulated the local economies. Athukorala and Resosudarmo (2005, 25–27), for instance, report that one year after the tsunami, Sri Lanka experienced a sharp 18 percent spike in investment growth, as opposed to the pre-tsunami prediction of 8.5 percent, which could even have offset the overall loss from the disaster. The reconstruction boom is also confirmed by the inflation of labor wages. Jayasuriya and McCawley (2008) mention that the wages of skilled laborers, such as carpenters and masons, doubled in several regions. The boom in housing reconstruction led to a labor scarcity, which in turn attracted further migration from the unaffected areas. The labor migration, combined with the resettlement through the OHC and DHC programs, resulted in a large demographic shift in Sri Lanka.

The second panel in Figure A1-6-1 indicates that the aid influx corresponded to the increase in the government's tax revenue. Although the tax revenue had been increasing since 2003, the slope became steeper starting in 2005, and the tax revenue continued to expand during the Eelam IV War. Although we need to be careful about causal attribution, it appears that the government improved its fiscal base in the post-tsunami period.

With a short time lag, the improvement in the government's tax revenue led to an increase in its military expenditure. As seen in the third pane of Figure A1-6-1, although the aid inflow and the increased tax revenue did not immediately entail military expansion, they appeared to allow the government to strengthen its military after the onset of the Eelam IV War. In fact, it was in late 2005, just a half year before the Eelam IV War, that the government reassessed the strategic reasons for the failure of its counterinsurgency policies, leading to the conclusion that "the solution was to increase the force strength" (Hashim 2013, 187). The strategic reorientation made by the new Rajapaksa government led to the expansion of the military forces; "[b]etween 2005 and 2009 the armed forces increased from 125,000 men and women to around 450,000"(Hashim 2013, 188).

Note that the tactical reorientation was made at a very late stage of the post-tsunami period, and that its impact materialized in the military expenditure after the onset of the Eelam IV War. This is consistent with the bargaining argument, which hypothesizes that the initial implementations of reconstruction signals a future shift in the power balance. From a theoretical perspective, the differential growth starting in the early post-tsunami period and the strategic reorientation revealed the future power shift, which in turn created a strategic conundrum in the bargaining between the government and LTTE.

The P-TOMS and Bargaining Failure

The strategic impasse in bargaining can be seen in the failure of the P-TOMS. Soon after the tsunami, the government and LTTE signed the P-TOMS, which was supposed to allow the LTTE to manage the aid inflows for the entire northern region. Although the P-TOMS could have allowed a more rapid recovery in the LTTE-controlled north that might have mitigated the power shift, this did not happen. Part of the reason lies in the fact that the negotiators, especially the government's representative President Kumaratunga, did not have a full control over post-disaster reconstruction. Unlike the ceasefire agreement that she signed in 2002, the P-TOMS sparked large protests by Muslim and Sinhalese constituencies and elicited opposition from all parties except the president's and Tamil-based parties. Even the coalition partner, JVP (Janatha Vimukthi Peramuna), left the government and brought the agreement to the Supreme Count. Finally, on 15 July 2005, the Court annulled the P-TOMS. The loss of the coalition partner also resulted in the fall of Kumaratunga's government and the rise of a new president Rajapaksa in the November 2005 presidential election. Rajapaksa promised more resolute policies towards the LTTE, and indeed pursued the military expansion as I mentioned.

The sequence of events lends credence to the underlying assumption of the bargaining argument; the negotiators could hardly control the allocation of reconstruction materials. The vested local interests, as well as donors' strategic incentives and bureaucratic processes, hampered the government from using post-disaster reconstruction for the purpose of the negotiation with the LTTE. When President Kumaratunga pursued the conciliatory policies toward the LTTE, she provoked a substantial backlash that ultimately brought down her government.

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Appendix II: Supporting Information for: “The Drowning-out Effect: Voter Turnout, Uncertainty, and Protest

SI 2-1. PROOF OF THE PROPOSITIONS

In this section, I first derive the posterior belief about the size of protestors m given the electoral outcomes $\boldsymbol{v} = (v_T, v_{L|T})$. I then prove the Bayesian Perfect Equilibrium.

Posterior Belief

By Bayes’ Rule, the posterior of m given $\boldsymbol{v}$ is;

$$\text{Prob}(m|v) = \frac{\text{Prob}(\boldsymbol{v}|m)\text{Prob}(m)}{\text{Prob}(\boldsymbol{v})}$$

$$= \frac{\text{Prob}\left(v_{L|T}|m, v_T\right)\text{Prob}(v_T|m)\text{Prob}(m)}{\text{Prob}\left(v_{L|T}|v_T\right)\text{Prob}(v_T)} \cdots (1).$$

Consider each term in the last expression

(a) $\text{Prob}\left(v_{L|T}|m, v_T\right)$

Note that all of the m protestors must vote for L by the assumption $\tau_{protest} > \tau_{vote} > 0$. The probability that each of $n - m$ non-protestors vote for L given its turnout is;

$$\text{Prob}(\theta_i > \tau_{vote}|\theta_i < -\tau_{vote} \text{ or } \tau_{vote} < \theta_i < \tau_{protest})$$

$$= \frac{F_\theta\left(\tau_{protest}\right) - F_\theta(\tau_{vote})}{F_\theta\left(\tau_{protest}\right) + F_\theta(-\tau_{vote}) - F_\theta(\tau_{vote})} \equiv \rho_a.$$

Then;

$$\text{Prob}\left(v_{L|T}|m, v_T\right) = \binom{nv_T - m}{nv_T v_{L|T} - m} \rho_a^{nv_T v_{L|T} - m}(1 - \rho_a)^{nv_T(1 - v_{L|T})}.$$

(b) $\text{Prob}(v_T|m)$

Note that all of the m protestors must turnout by the assumption $\tau_{protest} > \tau_{vote} > 0$. The probability that each of $n-m$ non-protestors turns out is;

$$\text{Prob}\left(\theta_i < -\tau_{vote}|\theta_i < \tau_{protest}\right) + \text{Prob}\left(\theta_i > \tau_{vote}|\theta_i < \tau_{protest}\right)$$

$$= \frac{F_\theta\left(\tau_{protest}\right) + F_\theta(-\tau_{vote}) - F_\theta(\tau_{vote})}{F_\theta\left(\tau_{protest}\right)} \equiv \rho_b.$$

Then;

$$\text{Prob}(v_T|m) = \binom{n-m}{nv_T - m} \rho_b^{nv_T - m}(1-\rho_b)^{n(1-v_T)}.$$

(c) $\text{Prob}(m)$

The prior probability that i joins protest is;

$$\text{Prob}\left(\theta_i > \tau_{protest}\right) = 1 - F_\theta\left(\tau_{protest}\right) \equiv \rho_c.$$

Then;

$$\text{Prob(m)} = \binom{n}{m} \rho_c^m (1-\rho_c)^{n-m}.$$

(d) $\text{Prob}\left(v_{L|T}|v_T\right)$

The probability that i votes for L given its turnout is;

$$\text{Prob}(\theta_i > \tau_{vote}|\theta_i < -\tau_{vote} \text{ or } \tau_{vote} < \theta_i) = \frac{1 - F_\theta(\tau_{vote})}{1 + F_\theta(-\tau_{vote}) - F_\theta(\tau_{vote})} \equiv \rho_d.$$

Then;

$$\text{Prob}\left(v_{L|T}|v_T\right) = \binom{nv_T}{nv_{L|T}v_T} \rho_d^{nv_{L|T}v_T}(1-\rho_d)^{nv_T(1-v_{L|T})}.$$

(e) $\text{Prob}(v_T)$

The probability that i turns out is;

$$\text{Prob}(\theta_i < -\tau_{vote} \text{ or } \tau_{vote} < \theta_i) = 1 + F_\theta(-\tau_{vote}) - F_\theta(\tau_{vote}) \equiv \rho_e.$$

Then;

$$\text{Prob}(v_T) = \binom{n}{nv_T} \rho_e^{nv_T} (1 - \rho_e)^{n(1-v_T)}.$$

By inserting (a)-(e) into (1) and simplifying it, we obtain;

$$\text{Prob}(m|\boldsymbol{v}) = \binom{nv_T v_{L|T}}{m} \delta^m (1 - \delta)^{nv_T v_{L|T} - m},$$

where $\delta = \frac{1-F_\theta(\tau_{protest})}{1-F_\theta(\tau_{vote})}$. Let the CDF of the binomial distribution be $F_{m|\boldsymbol{v}}$. By using normal approximation to the binomial distribution, the probability of $m < \sigma n$ is approximated by $F_{m|\boldsymbol{v}}(\sigma n) \approx \Phi\left(\frac{\sigma n - nv_T v_{L|T}\delta}{\sqrt{nv_T v_{L|T}\delta(1-\delta)}}\right)$, where Φ is the standard normal CDF.

Bayesian Perfect Equilibrium

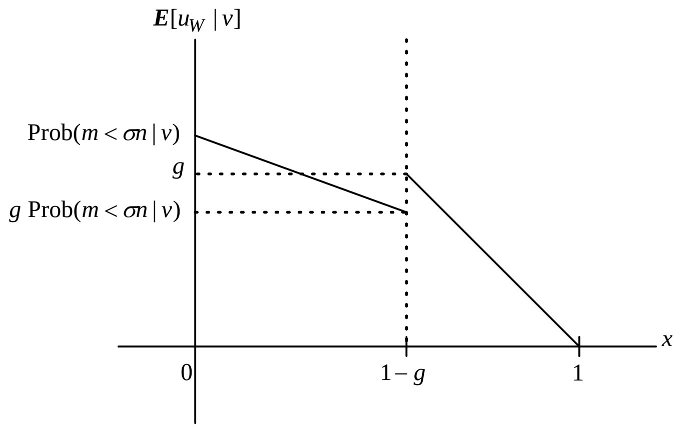
Since L knows the exact value of m, L's response rule is the following. When $m \geq \sigma$, L accepts the offer only if $x \geq 1 - g$. When $m < \sigma n$, L accepts any offer as $x > -g$ is always satisfied. Given L's response rule, W's optimal offer is either $x = 0$, which L accepts only if $m < \sigma n$, or $x = 1 - g$, which L accepts regardless of m. As seen in Figure A2-1-1, any other offer between 0 and $1 - g$ reduces W's payoff when W accepts. Any other offer between $1 - g$ and 1 will be accepted by L, but W receives less than what she receives by offering $1 - g$. W's expected payoff of offering $x = 0$ and $1 - g$ are;

$$\text{E}[u_W(x = 0)|\boldsymbol{v}] = \text{Prob}(m < \sigma n|\boldsymbol{v}) = F_{m|\boldsymbol{v}}(\sigma n);$$

$$\mathrm{E}[u_W(x = 1 - g)|\boldsymbol{v}] = g.$$

Thus, when $F_{m|\boldsymbol{v}}(\sigma n) \geq g$, W offers $x^* = 0$. When $F_{m|\boldsymbol{v}}(\sigma n) < g$, W offers $x^* = 1 - g$.

Figure A2-1-1. W's Expected Utility with Respect to $\boldsymbol{x}$



NOTE: The case of $\mathrm{Prob}(m < \sigma n|\boldsymbol{v}) > g$ .The horizontal and vertical axes show W's offer and her expected utility.

By the above discussion, when $F_{m|\boldsymbol{v}}(\sigma n) \geq g$, the strategy profile in Proposition 1 constitutes a Bayesian Perfect Equilibrium.

Probability of Protest

The probability of protest conditional on $\boldsymbol{v}$ is $1 - F_{m|\boldsymbol{v}}(\sigma n) \approx 1 - \Phi\left(\frac{\sigma n - n v_T v_{W|T}\delta}{\sqrt{n v_T v_{W|T}\delta(1-\delta)}}\right)$, which is increasing with v_T. In particular, the protest probability is increasing with v_T for two reasons. The first is the effect through the variance term $n v_T v_{W|T}\delta(1-\delta)$, which I call a drowning-out effect. High voter turnout increases the variance of the posterior distribution of m, which in turn increases the tail density of $\mathrm{Prob}(m \geq \sigma n|\boldsymbol{v})$. The second is the effect through the mean term $n v_T v_{W|T}\delta$. If the other conditions are constant, high turnout means more active citizens (whether they support W or L). As a result, in expectation, the number of potential protestors is also large as well. This increases the probability of protest after the election. Substantively, the mean-shift effect can be considered as a selection process: high turnout reflects the surge of political interests and hence has an effect to increase the likelihood of protests. The later empirical analysis teases out the drowning-out and mean-shift effects by using election-day rainfall deviation as an instrumental variable. Because the instrumental variable estimator is local to cost-sensitive voters (more precisely, units whose turnout are sensitive to election-day weather), the mean-shift effect, which is primarily concerned with the turnout of motivated voters, cannot explain the local effect.

Finally, note that these arguments hold when g is sufficiently small. By contrast, when g is not negligible (such as in autocracies), it creates a selection process; exactly because a large v_T means a higher probability of protest, it incentivizes W to offer $x = 1 - g$ instead of $x = 0$ and hence to avoid protest. That is, large v_T makes the condition $F_{m|v}(\sigma n) > g$ unlikely to be satisfied, alters the equilibrium to one in which W offers $x = 1 - g$ and L always accepts it, and hence eliminates any probability of protest. The selection process, however, does not exist when the coordination costs are sufficiently small such that $g \leq F_m(\sigma n) \approx \Phi\left(\frac{(\sigma-\delta)n}{\sqrt{n\delta(1-\delta)}}\right)$, where F_m is the CDF of a binomial distribution $\binom{n}{m}\delta^m(1-\delta)^{n-m}$, and hence the equilibrium condition $g \leq F_{m|v}(\sigma n)$ is satisfied regardless of v_T.

SI 2-2. RAINFALL MEASUREMENT

Table A2-2-1 is a list of possible data sources for the rainfall measurement. As seen in the table, the CMORPH provides the finest spatial resolution with the longest temporal coverage. For this reason, this study uses the CMORPH product.

Table A2-2-1. Potential Data Sources

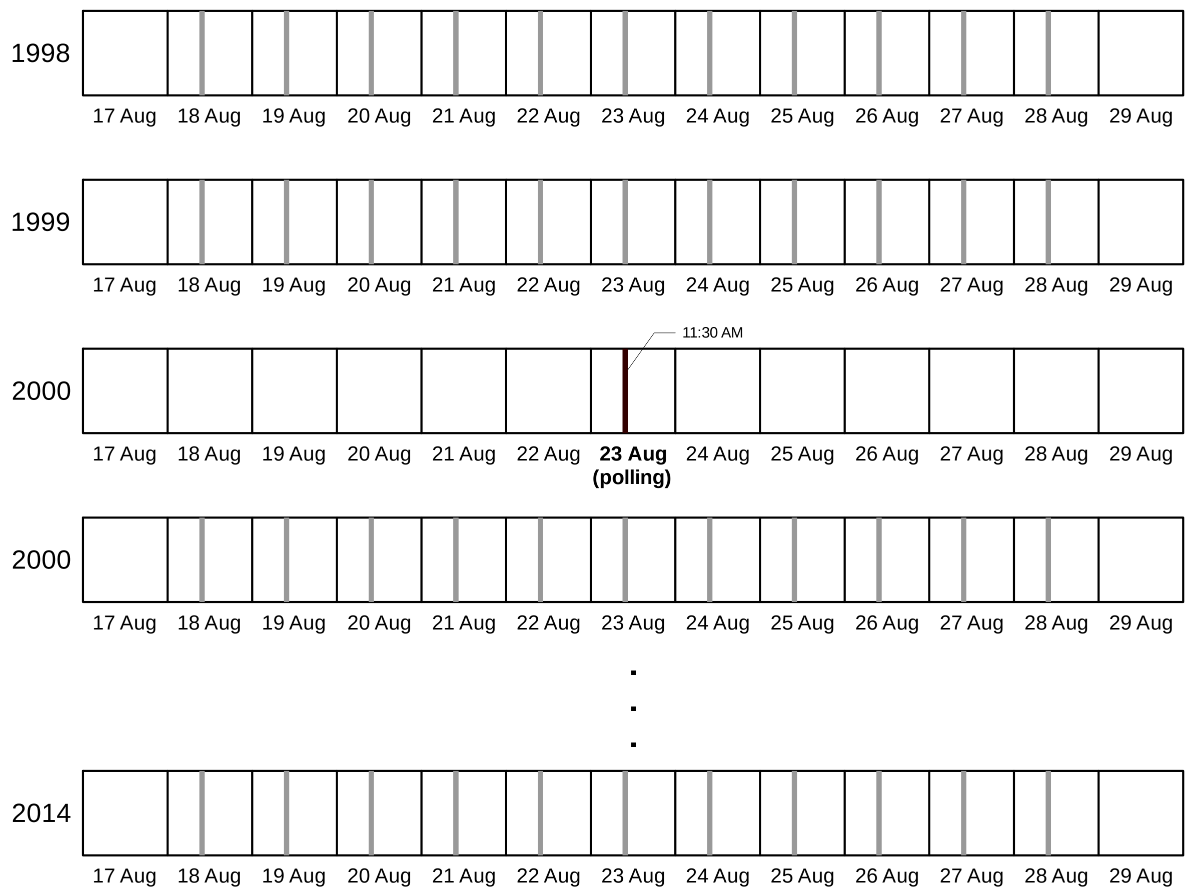
Data Source	Temporal Coverage	Temporal Resolution	Spatial Coverage	Spatial Resolution	Organization
Global Precipitation Measurement (GPM)	2014 –	30 min	Global	0.1	NASA
Tropical Rainfall Measurement Mission (TRMM)	1998 –	3 hour	Tropical regions	0.25	NASA
NOAA CPC Morphing Technique (CMORPH)	1998 –	30 min	Global	0.07	NOAA
Precipitation Estimation from Remotely Sensed Information using Artificial Neural Networks (PERSIANN)	2000 –	1 hour	Global	0.25	UCI
Precipitation Estimation from Remotely Sensed Information using Artificial Neural Networks Climate Data Record (PERSIANN CDR)	1983 –	Daily	Global	0.25	UCI
Quantitative Precipitation Estimates (K1-VHR-QPE)	2008 –	30 min	India	1	MOSDAC

Because the CMORPH satellite images are available every 30 minutes, I calculate the rainfall deviation at every 30 minutes from 8:00 AM to 6:00 PM on polling days. The observed rainfall amount is the average rainfall amount within a boundary of a constituency.[1] The normal

[1] The constituency boundaries are obtained from the website of Sandip Sukhtankar (http://www.dartmouth.edu/~sandip/data.html: accessed on 29 September 2017) and a GitHub host of the DataMeet project (https://github.com/datameet: accessed on 29 September 2017).

rainfall amount is estimated by the average of the observed rainfall amounts in different years. In particular, as seen Figure A2-2-1, for observed rainfall at 11:30 AM on August 23, 2000 (the black bar), the corresponding normal rainfall is the average of the rainfall amounts at the times of the gray bars. The rainfall deviation at 11:30 AM is computed as the observed rainfall minus the average of the rainfall at the times of the gray bars. In this way, each normal rainfall is estimated from a sample of 187 observations, which should be sufficiently large. The daily aggregate is the average of the 30-minutes rainfall deviations. Although I could potentially use the 30-minutes rainfall deviations as separate instruments, the first-stage F statistic becomes small. This is because these variables are highly correlated and thus provide little additional information (the F statistic penalizes the inclusion of weak predictors).

Figure A2-2-1. Rainfall Deviation Measurement



NOTE: Each row is a time window of a given year. The black bar is 11:30 AM on 23 August 2000. The gray bars are the dates whose rainfall amounts are used for calculating the normal rainfall of 11:30 AM on 23 August 2000.

SI 2-3. COVARIATES

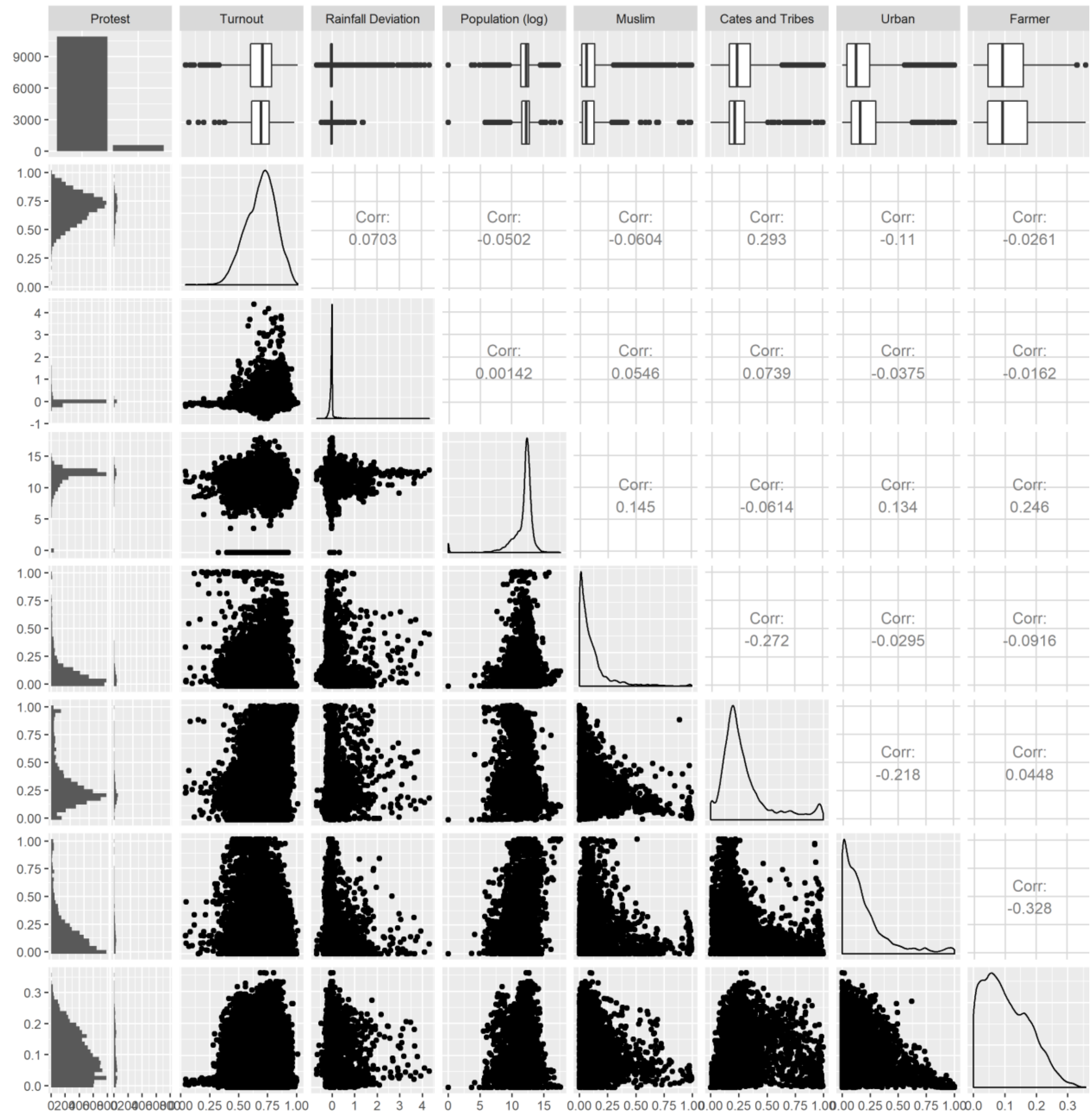
The covariates are derived from the Census of India 2001. The census data are available at the level of third administrative units, which are called “Tehsil”, “Taluk”, “Community Development Block”, “Sub-division”, and “Police Station”, depending on the states. The census data are first projected to the boundary map of the third administrative units.[2] The projected data are then transformed to 1 km-by-1 km grid cells (bilinear interpolation). Finally, for each assembly constituency, I calculate the average values.

SI 2-4. DATA SUMMARY

Figure A2-4-1 is the summary of the outcome, explanatory, instrument, and covariate variables. The first column shows the histograms of the covariates with and without post-election protest. The first row presents the corresponding boxplots with median and quantile values. The diagonal elements show the histograms of the covariates. The lower triangle of the figure (except for the first row and column) includes the scatter plots of the covariates. The upper triangle presents the correlation coefficients.

[2] The boundaries of the third administrative units are obtained from a GitHub host of the DataMeet project (https://github.com/datameet: accessed on 29 September 2017).

Figure A2-4-1. Data Summary

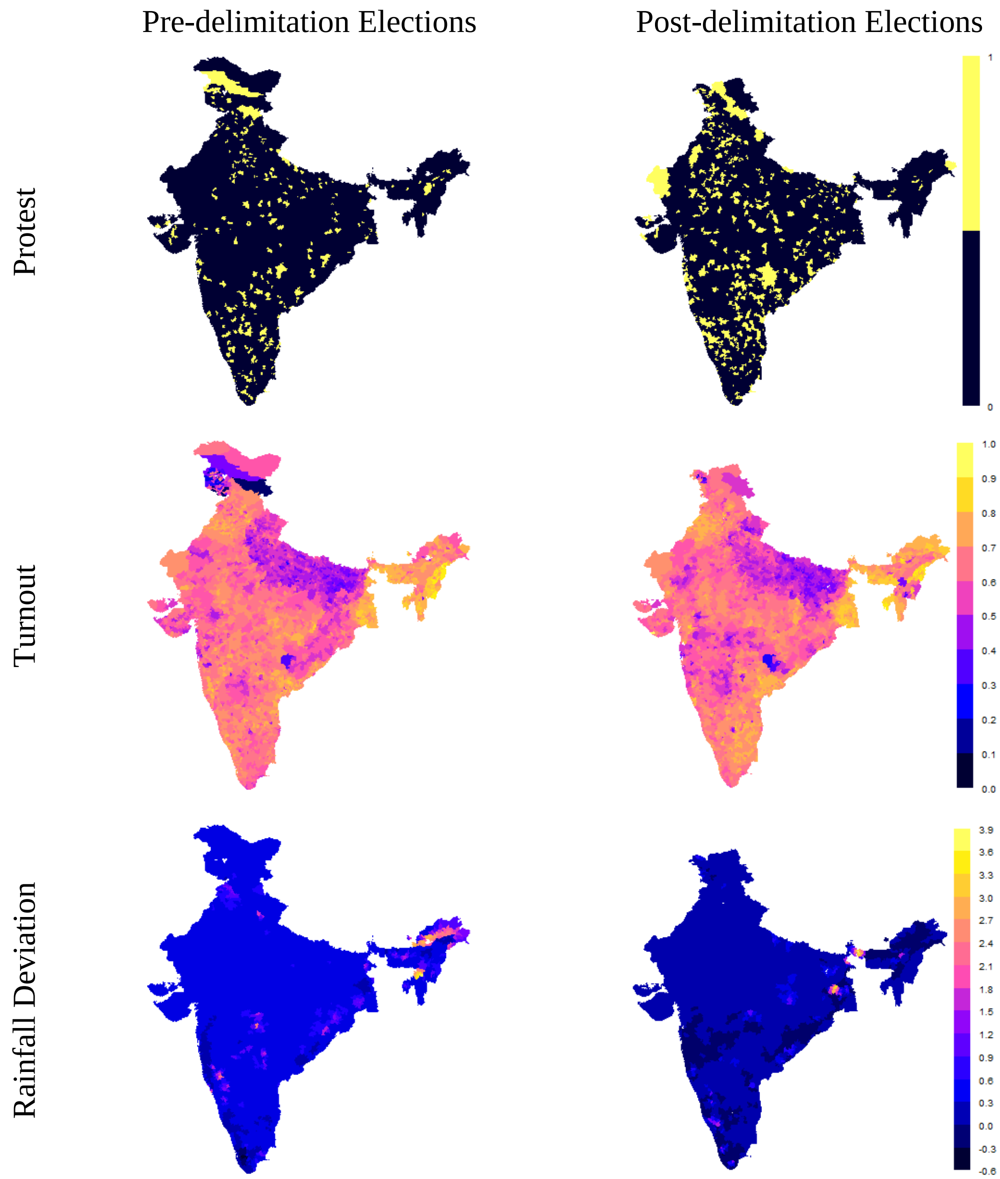


NOTE: The first row and column are the comparisons of the units with and without post-election protest. The other parts contain the histograms, scatter plots, and correlation coefficients of the explanatory, instrumental, and covariate variables.

The following figure (Figure A2-4-2) maps protests, turnout rates, and rainfall deviation for each constituency. The first map shows the constituencies that have at least one protest for the periods before and after the 2008 delimitation of the constituencies, and the second and third maps show the turnout rates and rainfall deviation averaged over the corresponding periods. The locations of protests fairly spread across the country, while the turnout rates are systematically low in the northern states, which is often attributed to the general lack of political interests in those regions. The systematic differences in turnout rates warns against naïve regressions of protests on turnout and justifies the use of the instrumental variable approach. By contrast, the average rainfall deviation has much less variation across the country with only a few exceptions. The average rainfall deviation is between -0.5 and 0.5 in a majority of constituencies, while a few constituencies in Arunachal Pradesh, Assam, and Jharkhand have large positive values.[3] This is not surprising as there are only a few elections for each constituency, and hence if one election is held under heavy rainfall, the average rainfall deviation should take large values as well (however, by the law of large numbers, if there would be a sufficient number of elections, the average rainfall deviation should converge to zero). To be sure, in SI 2-13, I conduct a robustness check by removing the constituencies that take large values of average rainfall deviation.

[3] 131 out of 4375 constituencies (before the 2008 delimitation) and 104 out of 4369 constituencies (after the delimitation) take more than 0.5 in the average rainfall deviation.

Figure A2-4-2. Spatial Distribution of Protests, Turnout, and Rainfall Deviation



NOTE: The figure shows the spatial distribution of protests, turnout, and rainfall deviation in India. The first row shows the constituencies that have at least one protest for the periods before (left) and after (right) the 2008 delimitation of the constituencies. The second and third columns show the turnout rates and rainfall deviation averaged over the elections before (left) and after (right) the 2008 delimitation.

SI 2-5. CONCURRENT STATE ASSEMBLY ELECTIONS

Because the sample size is large, I subset the data to observations that have elections at the same period and apply the near-far matching to each subset. Table A2-5-1 lists the groups of the concurrent elections. Note that I cannot apply the near-far matching to each election, as some states, including Tripura, Mizoram, Meghalaya, Sikkim, and the Union Territories, are too small for the matching.

Table A2-5-1. List of Concurrent Elections

Election Group ID	State	Year	Average Polling Date	First Polling Date	Last Polling Date	n
1	Haryana	2000	2000-02-22	2000-02-22	2000-02-22	90
1	Manipur	2000	2000-02-17	2000-02-12	2000-03-08	60
1	Orissa	2000	2000-02-19	2000-02-17	2000-02-22	147
2	Assam	2001	2001-05-11	2001-05-10	2001-09-22	126
2	Kerala	2001	2001-05-10	2001-05-10	2001-05-10	140
2	Puducherry	2001	2001-05-08	2001-05-01	2001-05-10	24
2	Tamil Nadu	2001	2001-05-10	2001-05-10	2001-05-10	231
2	West Bengal	2001	2001-05-10	2001-05-10	2001-05-10	292
3	Manipur	2002	2002-02-16	2002-02-14	2002-02-21	60
3	Punjab	2002	2002-02-13	2002-02-13	2002-02-21	117
3	Uttar Pradesh	2002	2002-02-18	2002-02-14	2002-05-31	403
3	Uttarakhand	2002	2002-02-14	2002-02-14	2002-02-14	70
4	Goa	2002	2002-05-30	2002-05-30	2002-05-30	38
5	Gujarat	2002	2002-12-12	2002-12-12	2002-12-12	182
6	Jammu and Kashmir	2002	2002-09-26	2002-09-16	2002-10-08	74
7	Himachal Pradesh	2003	2003-03-02	2003-02-25	2003-06-08	68
7	Meghalaya	2003	2003-02-26	2003-02-26	2003-02-26	60
7	Nagaland	2003	2003-02-26	2003-02-26	2003-02-26	60
7	Tripura	2003	2003-02-26	2003-02-26	2003-02-26	60
8	Chhattisgarh	2003	2003-12-01	2003-12-01	2003-12-01	90
8	Delhi	2003	2003-12-01	2003-12-01	2003-12-01	70
8	Madhya Pradesh	2003	2003-12-01	2003-12-01	2003-12-01	230
8	Mizoram	2003	2003-11-20	2003-11-20	2003-11-20	40
8	Rajasthan	2003	2003-12-01	2003-12-01	2003-12-01	200
9	Andhra Pradesh	2004	2004-04-23	2004-04-20	2004-04-26	294
9	Karnataka	2004	2004-04-22	2004-04-20	2004-04-26	224
9	Orissa	2004	2004-04-22	2004-04-20	2004-04-26	147
9	Sikkim	2004	2004-05-10	2004-05-10	2004-05-10	27

10	Arunachal Pradesh	2004	2004-10-07	2004-10-07	2004-10-07	57
10	Maharashtra	2004	2004-10-13	2004-10-13	2004-10-13	287
11	Haryana	2005	2005-02-03	2005-02-03	2005-02-03	90
11	Jharkhand	2005	2005-02-14	2005-02-03	2005-02-23	81
12	Bihar	2005	2005-11-02	2005-10-18	2005-11-19	243
13	Assam	2006	2006-04-06	2006-04-03	2006-04-10	126
13	Kerala	2006	2006-04-26	2006-04-22	2006-05-03	140
13	Puducherry	2006	2006-05-07	2006-05-03	2006-05-08	24
13	Tamil Nadu	2006	2006-05-08	2006-05-08	2006-05-08	231
13	West Bengal	2006	2006-04-27	2006-04-17	2006-05-16	292
14	Manipur	2007	2007-02-13	2007-02-08	2007-02-23	60
14	Punjab	2007	2007-02-13	2007-02-13	2007-02-13	117
14	Uttarakhand	2007	2007-02-21	2007-02-21	2007-02-21	70
15	Uttar Pradesh	2007	2007-04-22	2007-04-07	2007-05-08	403
16	Goa	2007	2007-06-02	2007-06-02	2007-06-02	38
17	Gujarat	2007	2007-12-13	2007-12-11	2007-12-16	182
17	Himachal Pradesh	2007	2007-12-17	2007-11-14	2007-12-19	68
18	Meghalaya	2008	2008-03-03	2008-03-03	2008-03-03	60
18	Nagaland	2008	2008-03-05	2008-03-05	2008-03-05	60
18	Tripura	2008	2008-02-23	2008-02-23	2008-02-23	59
19	Karnataka	2008	2008-05-15	2008-05-10	2008-05-22	224
20	Chhattisgarh	2008	2008-11-17	2008-11-14	2008-11-20	90
20	Delhi	2008	2008-11-29	2008-11-29	2008-12-13	69
20	Jammu and Kashmir	2008	2008-12-11	2008-11-17	2008-12-24	76
20	Madhya Pradesh	2008	2008-11-27	2008-11-27	2008-11-27	230
20	Mizoram	2008	2008-12-02	2008-12-02	2008-12-02	40
20	Rajasthan	2008	2008-12-04	2008-12-04	2008-12-04	200
21	Andhra Pradesh	2009	2009-04-19	2009-04-16	2009-04-23	294
21	Orissa	2009	2009-04-19	2009-04-16	2009-04-23	147
21	Sikkim	2009	2009-04-30	2009-04-30	2009-04-30	31
22	Arunachal Pradesh	2009	2009-10-13	2009-10-13	2009-10-13	57
22	Haryana	2009	2009-10-13	2009-10-13	2009-10-13	90
22	Maharashtra	2009	2009-10-13	2009-10-13	2009-10-13	286
23	Jharkhand	2009	2009-12-05	2009-11-25	2009-12-18	81
24	Bihar	2010	2010-10-30	2010-10-21	2010-11-20	242
25	Assam	2011	2011-04-07	2011-04-04	2011-04-11	126
25	Kerala	2011	2011-04-13	2011-04-13	2011-04-13	140
25	Puducherry	2011	2011-04-13	2011-04-13	2011-04-13	28
25	Tamil Nadu	2011	2011-04-13	2011-04-13	2011-04-13	234
25	West Bengal	2011	2011-04-27	2011-04-18	2011-05-10	294
26	Goa	2012	2012-03-03	2012-03-03	2012-03-03	38
26	Manipur	2012	2012-01-28	2012-01-28	2012-01-28	60
26	Punjab	2012	2012-01-30	2012-01-30	2012-01-30	117

26	Uttar Pradesh	2012	2012-02-19	2012-02-08	2012-03-03	403
26	Uttarakhand	2012	2012-01-30	2012-01-30	2012-01-30	70
27	Himachal Pradesh	2012	2012-11-04	2012-11-04	2012-11-04	68
28	Gujarat	2012	2012-12-15	2012-12-13	2012-12-17	162
29	Meghalaya	2013	2013-02-23	2013-02-23	2013-02-23	60
29	Nagaland	2013	2013-02-23	2013-02-23	2013-02-23	60
29	Tripura	2013	2013-02-14	2013-02-14	2013-02-14	59
30	Karnataka	2013	2013-05-05	2013-05-05	2013-05-05	224
31	Chhattisgarh	2013	2013-11-17	2013-11-11	2013-11-19	90
31	Delhi	2013	2013-12-04	2013-12-04	2013-12-04	69
31	Madhya Pradesh	2013	2013-11-25	2013-11-25	2013-11-25	230
31	Mizoram	2013	2013-11-25	2013-11-25	2013-11-25	40
31	Rajasthan	2013	2013-12-01	2013-12-01	2013-12-13	200
32	Arunachal Pradesh	2014	2014-04-09	2014-04-09	2014-04-09	49
32	Orissa	2014	2014-04-13	2014-04-10	2014-04-17	147
32	Sikkim	2014	2014-04-12	2014-04-12	2014-04-12	31
33	Andhra Pradesh	2014	2014-05-04	2014-04-30	2014-05-07	293
34	Haryana	2014	2014-10-15	2014-10-15	2014-10-15	90
34	Maharashtra	2014	2014-10-15	2014-10-15	2014-10-15	285
35	Jammu and Kashmir	2014	2014-12-07	2014-11-25	2014-12-20	76
35	Jharkhand	2014	2014-12-08	2014-11-25	2014-12-20	74

NOTE: The table shows the group ID, state, year, the first polling date, average polling date, last polling date, and the number of constituencies for each State Assembly election.

SI 2-6. OPTIMIZATION OF THE NEAR-FAR MATCHING

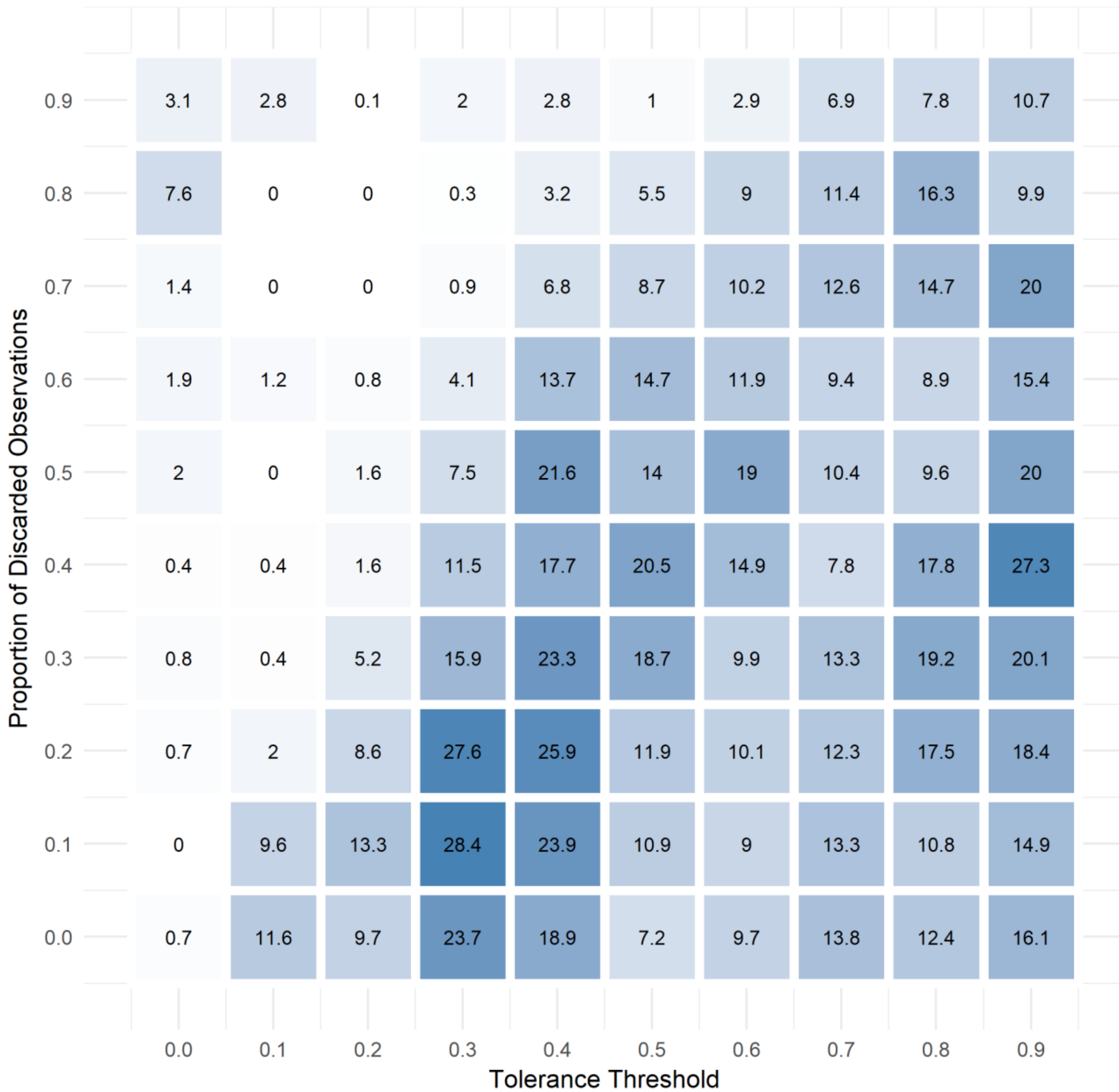
In the grid search of the tuning parameters, I repeat the near-far matching for a range of τ and ρ values and calculate the strength of the instrument and covariate balance metrics for each. The parameter τ is the threshold values above which the penalty is applied. The parameter ρ is the proportion of discarded observations in the matching (in order to omit hardest-to-match observations). Because raw τ values are different depending on the scale of d_{ij} (the difference in the values of an instrumental variable), I use quantile of unique values of $|d_{ij}|$ as a grid-search range of τ. By using unique values, the quantile values are also ensured to be unique. This is beneficial as we do not repeat the matching for the same value of τ. For instance, for

$$|d_{ij}| = (0, 0, 0, 0, 0, 0, 0, 0, 0, 0, 1, 1, 2, 2, 2, 3, 3, 4, 5, 5, 6, 7, 22, 25, 40, 41, 42),$$

the unique quantile values are $(0, 1.2, 2.4, 3.6, 4.8, 6, 10, 23.2, 34, 40.8)$. Let me denote the unique quantile as τ'.

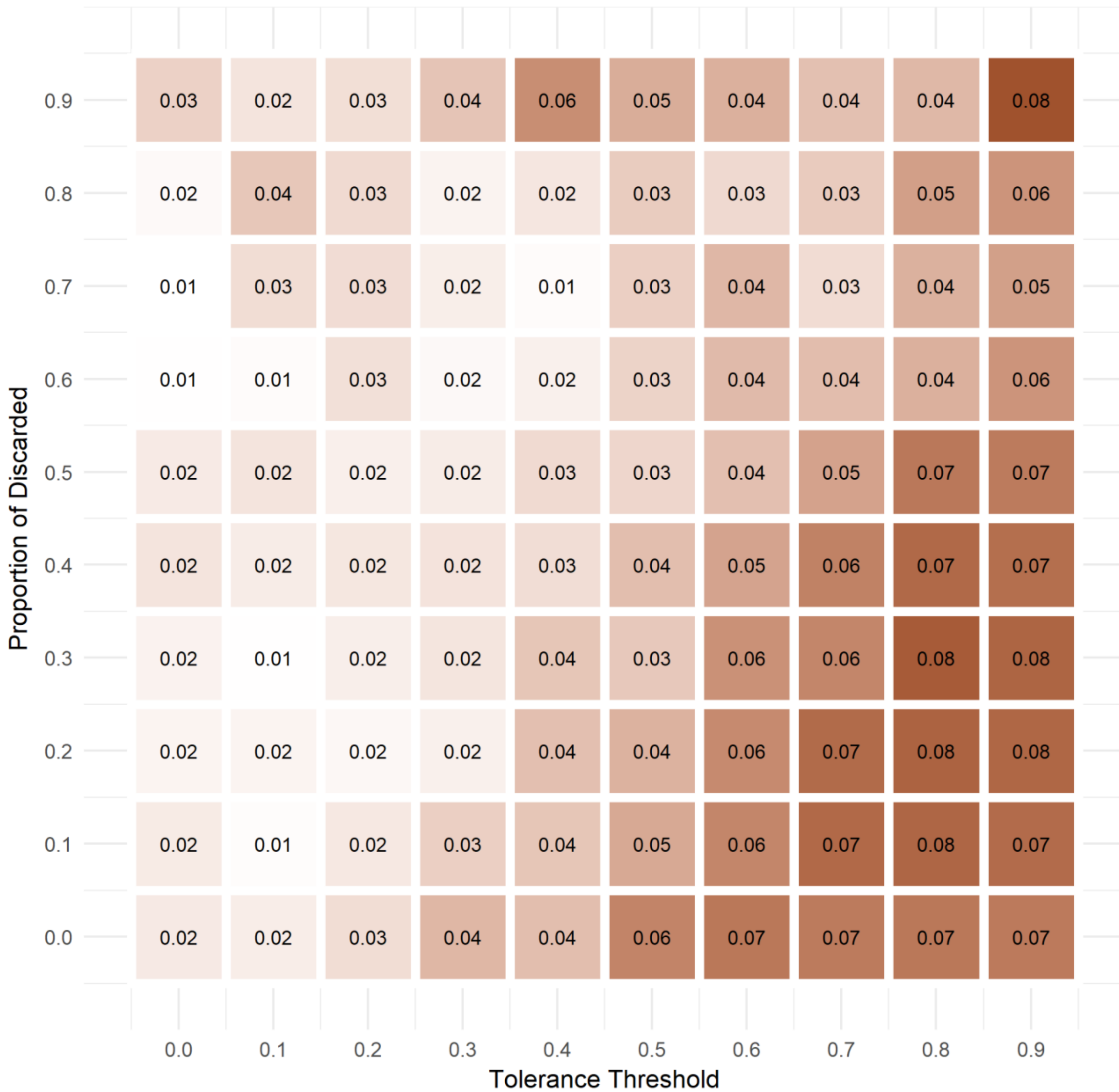
Figure A2-6-1 to Figure A2-6-3 shows the first-stage F statistics, absolute means of the covariate differences, and averages of the variance ratios of the covariates for a range of tuning parameters τ' and ρ . In Figure A2-6-1, there are two "hills" in which the F statistics are locally maximum: $(\tau', \rho) \in \{(0.3, 0.1), (0.9, 0.4)\}$. In Figure A2-6-2 and A2-6-3, these cells tend to have relatively worse covariate balances, but the imbalances do not exceed the recommended ranges. Taking the sample sizes into account, I choose $(\tau', \rho) = (0.3, 0.1)$ as the optimal value and conduct an additional analysis with tuning values $(\tau', \rho) = (0.9, 0.4)$.

Figure A2-6-1. Grid-search of Tuning Parameters: First-stage F Statistics



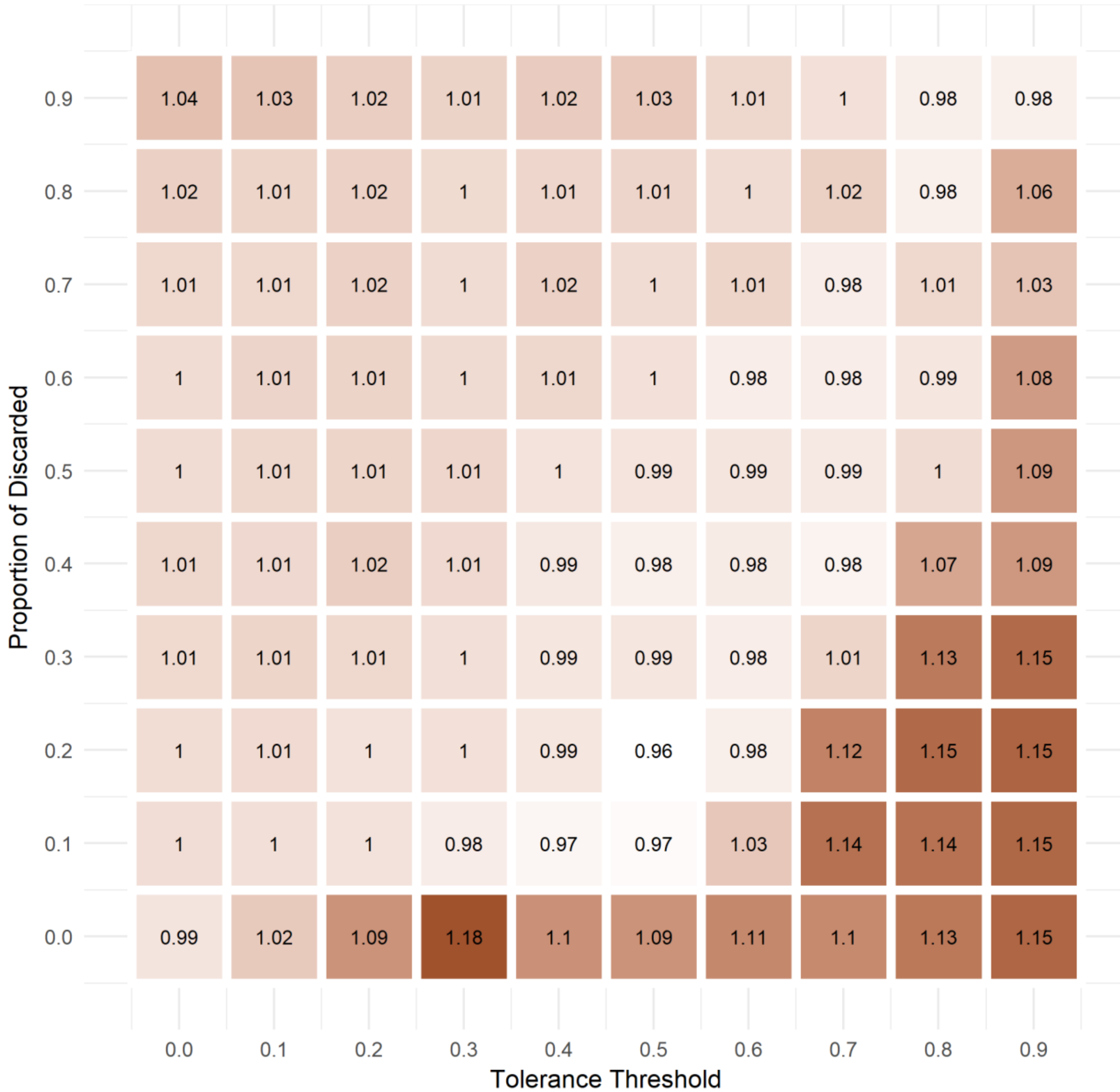
NOTE: Each value in the cell is a first-stage F statistic. The column is the proportions of discarded observations in the near-far matching (ρ). The row is the threshold values (τ'; decile of the unique values of d_{ij}). A high first-stage F statistic indicates a strong instrument. As a rule of thumb, the first-stage F statistic less than 10 is a warning sign of a weak instrument.

Figure A2-6-2. Grid-search of Tuning Parameters: Absolute Means of Covariate Differences



NOTE: Each value in the cell is the mean of the absolute standardized differences of the covariates. The column is the proportions of discarded observations in the near-far matching (ρ). The row is the threshold values (τ'; decile of the unique values of d_{ij}). A small absolute mean indicates good covariate balance. As a rule of thumb, the absolute mean more than 0.2 is a warning sign of covariate imbalance.

Figure A2-6-3. Grid-search of Tuning Parameters: Means of Variance Ratios



NOTE: Each value in the cell is the mean of variance ratios of the covariates. The column is the proportions of discarded observations in the near-far matching (ρ). The row is the threshold values (τ'; decile of the unique values of d_{ij}). A variance ratio around 1 indicates good covariate balance. As a rule of thumb, the variance ratio more than 2 or less than 0.5 is a warning sign of covariate imbalance.

When I conduct the near-far matching with tuning parameters $\tau' = 0.9$ and $\rho = 0.4$, the Hodge-Lehmann estimate of the causal effect of turnout on the onset of protest becomes 1.08 with the 95% confidence interval [0.20, 2.37]. The TSLS with covariates and standard error clustered for each constituency is 0.98 with a confidence interval of [0.02, 1.94]. The corresponding Anderson-Rubin confidence interval is [0.18, 2.33].

SI 2-7. COVARIATE BALANCE BEFORE AND AFTER NEAR-FAR MATCHING

The following table (Table A2-7-1) presents the balance statistics without and with the near-far matching.[4] The standardized differences are the average differences between the treated and control units divided by the standard deviation. The variance ratio is the variance among treated observations divided by that among control observations. As a rule of thumb, the standardized difference should be between -0.2 and 0.2 and ideally between -0.1 and 0.1, and the variance ratio should be between 0.5 and 2 (Rosenbaum 2009; Rubin 2001). As seen in the first two columns of Table A2-7-1, even though the rainfall deviation is expected to be randomly assigned, in a finite sample, there exist differences between those of abnormal rain and those of normal weather. Indeed, the constituencies that have a larger population, more scheduled castes and tribes, and less urban residents tend to receive treatments. The large standardized differences also indicate that linear regressions cannot adjust the differences without heavily relying on parametric assumptions (Rubin 2001). In contrast, as shown the last two columns of Table A2-7-1 and also Figure A2-7-1, the near-far matching properly adjusts the remaining imbalances.

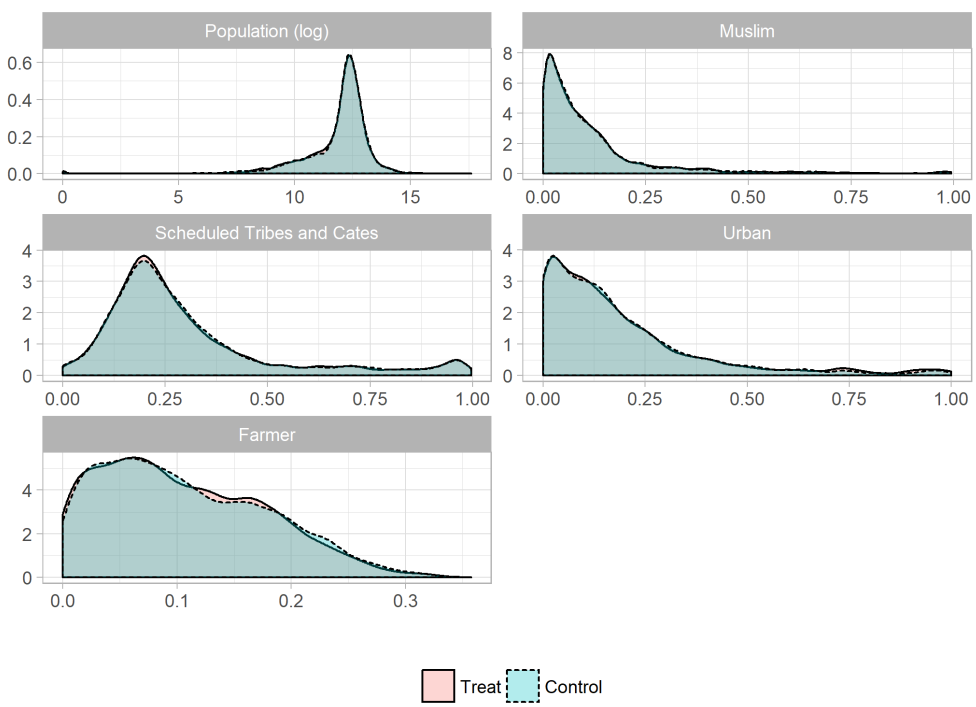
[4] For non-matching, I use a dummy variable for positive rainfall deviation versus zero or negative deviation. The balance statistics of the near matching are similar to those of the near-far matching.

Table A2-7-1. Covariate Balance without and with the Near-far Matching

	No Matching		Near-far Matching	
	Standardized Difference	Variance Ratio	Standardized Difference	Variance Ratio
Population (log)	**1.51**	**0.44**	0.01	1.04
Muslim	0.18	1.72	-0.01	0.97
Scheduled castes and tribes	**0.39**	1.14	0.02	1.00
Urban	**-0.27**	0.65	-0.06	0.88
Farmer	-0.02	0.95	0.07	1.03

NOTE: The table shows the balance statistics for the data without and with near-far matching. The standardized differences are the average differences between the treated and control units divided by the standard deviation. The variance ration is the variance among treated observations divided by that among control observations. The treated and control units are those of positive rainfall deviations and the rest of observations for the results without matching. As a rule of thumb, the standardized difference should be between -0.2 and 0.2. The variance ratio should be between 0.5 and 2. If the values are outside of these ranges, they are **bolded**.

Figure A2-7-1. Covariate Balance after the Near-far Matching



NOTE: Density plots of the covariates for treated and control units (after the near-far matching).

SI 2-8. OTHER ESTIMATES

This section provides the results of the OLS and TSLS estimates with and without the near-far matching.

OLS Estimate

As seen in Table A2-8-1, the naïve OLS estimate is close to zero. The naïve OLS model includes the covariates and election-group fixed effects with standard errors clustered for constituencies.[5]

Table A2-8-1. OLS Estimate of the Effect of Turnout on Protest

Point Estimate	95% Confidence Interval
-0.012	[-0.04, 0.02]

NOTE: The table shows the OLS estimate with the covariates, election-group fixed effects, and standard error clustered for constituencies. $n = 11{,}500$.

TSLS Estimates without Near-far Matching

I also estimate the TSLS with different specifications of the first-stage regressions without the nearfar matching (the other specifications are the same as the OLS: the controls for the covariates and election-group fixed effects with clustered standard errors). In particular, I create polynomials and cubic splines of the rainfall deviations and use them as instrumental variables. Unlike the other regression-based approach, these transformations do not harm the desirable properties of the TSLS (thus, there is no concern with so-called forbidden regression). While

[5] For the purpose of the comparison with the results of the nearfar matching, in which constituencies are matched within each election group, I include the election-group fixed effects. For details of the election groups, see SI 2-5.

polynomial can be susceptible to overfitting, the spline transformation is more robust to overfitting. When the order of polynomial or cubic spline is 1, the first-stage regression is equivalent to the linear regression of turnout on the rainfall deviation variable (as well as the covariates and the election-group fixed effects). As the polynomial or spline order increases, it adds more flexibility to the model while decreasing the parsimony. Since the F statistics penalize overly complicated models, we can usually find a reasonable value of a polynomial or spline order that maximizes the first-stage F statistic.

As seen in Table A2-8-2 and A2-8-3, while the TSLS estimates with inflexible first-stage specifications are unstable and even negative with very small first-stage F statistics, the TSLS estimates are positive and stable with sufficiently high orders of the cubic splines. As seen in Table A2-8-2, the polynomial regressions do not greatly improve the instrument's power. By contrast, as seen in Table A2-8-3, the spline regressions increase the F statistics close to the conventional threshold $F = 10$. These results are consistent to the fact that spline regressions are less susceptible to overfitting than polynomial regressions (Beck, Katz, and Tucker 1998). In any case, the results indicate that if one could find a first-stage specification that can strongly predict turnout, the weak-instrument biases become sufficiently small, and hence the regression-based estimates are similar to the non-parametric estimates in the main paper. When rainfall is a weak predictor of turnout, however, the weak-instrument biases become so large that the estimated effects become even negative.

Table A2-8-2. TSLS Estimate with Polynomial First-stages (No Near-far Matching)

Polynomial order	Estimate	95% CI	First-stage F
1	-0.95	[-1.92, 0.02]	2.50
2	-0.10	[-0.87, 0.68]	4.92
3	0.14	[-0.64, 0.93]	4.76
4	**0.29**	**[-0.41, 0.99]**	**5.51**
5	0.36	[-0.20, 0.93]	5.48
6	0.34	[-0.16, 0.85]	4.35

NOTE: The table shows the TSLS estimates with the covariates, election-group fixed effects, and standard error clustered for constituencies. In each of the regressions (row), rainfall instruments with a different order of polynomial are used. The first to fourth columns show the orders of the first-stage polynomial, the second-stage estimate of the effect of turnout on the onset of protests, the corresponding confidence intervals, and the first-stage F statistics. The optimal polynomial order is **bolded**. $n = 11{,}500$.

Table A2-8-3. TSLS Estimates with Spline First-stages (No Near-far Matching)

Spline order	Estimate	95% CI	First-stage F
1	-0.95	[-1.92, 0.02]	2.50
2	-0.01	[-0.79, 0.77]	5.11
3	0.43	[-0.14, 1.01]	6.89
4	**0.81**	**[0.25, 1.38]**	**9.27**
5	0.82	[0.27, 1.38]	8.88
6	0.83	[0.28, 1.39]	8.01

NOTE: The table shows the TSLS estimates with the covariates, election-group fixed effects, and standard error clustered for constituencies. In each of the regressions (row), rainfall instruments with a different order of cubic spline are used. The first to fourth columns show the orders of the first-stage splines, the second-stage estimate of the effect of turnout on the onset of protests, the corresponding confidence intervals, and the first-stage F statistics. The optimal spline order is **bolded**. $n = 11{,}500$.

TSLS Estimates with Near-far Matching

Finally, I also estimate the TSLS and corresponding Anderson-Rubin confidence interval after the near-far matching to account for potential temporal autocorrelation. As seen in the following table (Table A2-8-4), the results are similar to the main estimate.

Table A2-8-4. TSLS Estimate with Near-far Matching

Point Estimate	95% Confidence Interval	
	Conventional	Anderson-Rubin
1.63	[0.04, 3.21]	[0.46, 5.13]

NOTE: The table shows the TSLS estimate, its conventional confidence interval, and Anderson-Rubin confidence interval with the covariates, election-group fixed effects, and standard error clustered for constituencies. $n = 11{,}500$.

SI 2-9. POLLING-DAY PROTEST

As a falsification test (a test that "falsifies" the claim that rainfall deviation affects poling-day protest and thus it violates the exclusion restriction), I conduct a Fisher exact test for the contingency table of the treatment status and the onset of polling-day protest. Table A2-9-1 is the contingency table. The corresponding p-value is 1, providing no evidence that rainfall deviation would affect the risks of polling-day protest.

Table A2-9-1. Contingency Table of Treatment Status and Polling-day Protest

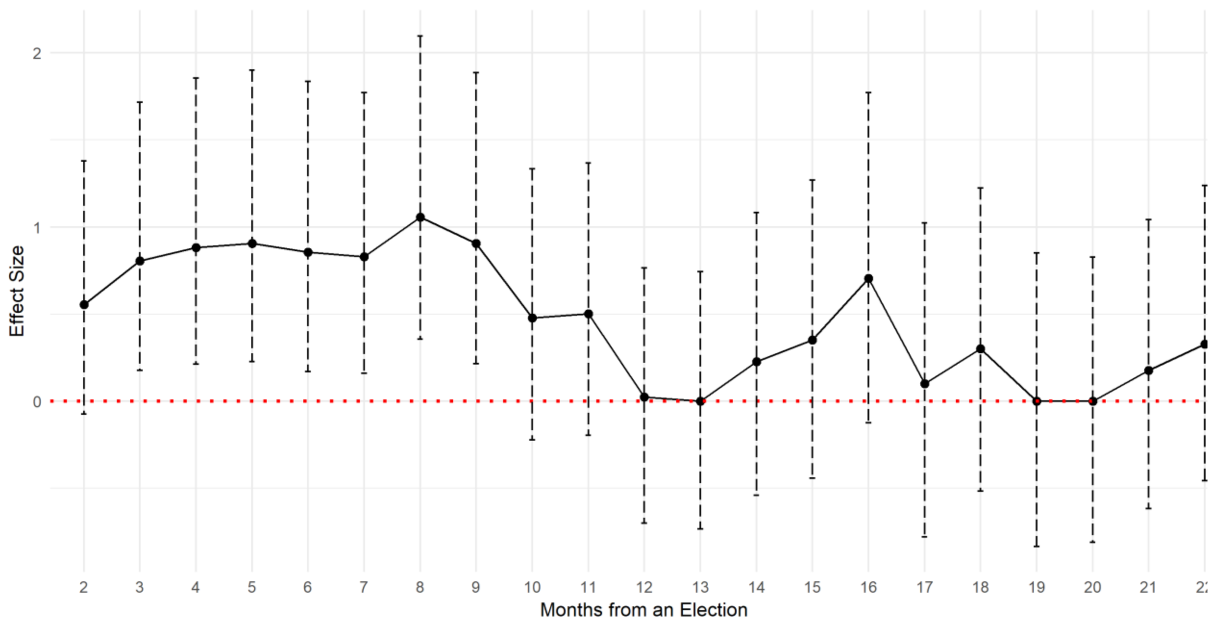
		Polling-day Protest	
		No Protest	Protest
Treatment Status	Control	5162	3
	Treated	5163	2

Note: $n = 10{,}330$.

SI 2-10. DIFFERENT TIME WINDOWS FOR POST-ELECTION PERIODS

In the main analysis, I define the outcome variable as the onset of protest within one year after polls. Because there is no particular reason to use the one-year time window, I also conduct an additional analysis. In particular, I estimate the causal effects with moving time windows; the first estimate is the effect on the onset of protest between a polling day and four months after the polling, the second is the effect on the onset of protest between the first month and five months after the polling day, and so forth. I repeat this procedure until I estimate the effect on the onset of protest between twenty and twenty-four months after the poll. The horizontal axis in Figure A2-10-1 shows the middle month of a time window. The points and bars in Figure A2-10-1 are the Hodge-Lehman estimates and 95% confidence intervals. Note that the effect sizes become smaller than the main estimate as a protest probability within four months is smaller than the corresponding probability within one year.

Figure A2-10-1. Rolling Estimates with Different Time Windows



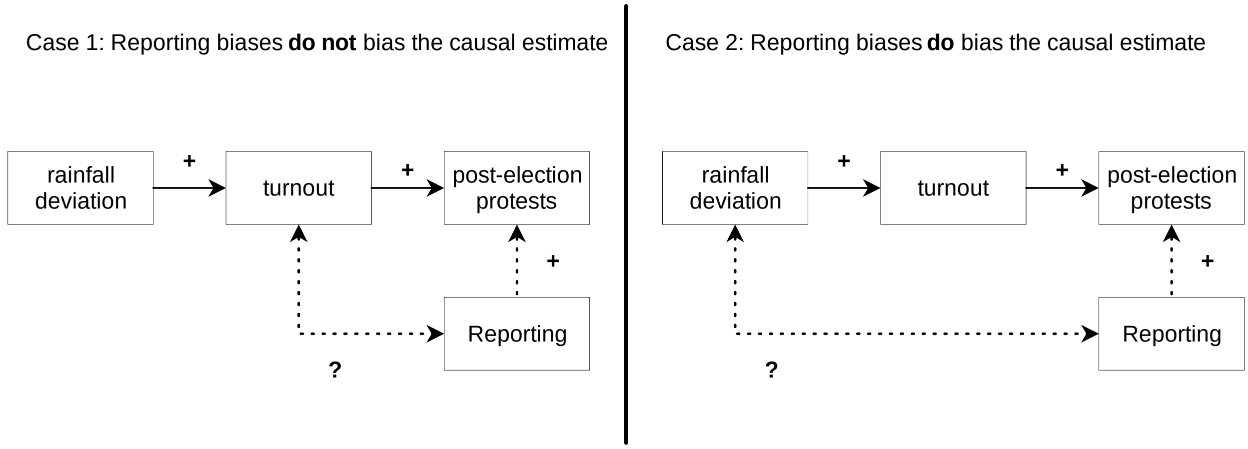
NOTE: The figure shows the results of the rolling estimates. On each month in the horizontal axis, I estimate the effect of turnout on the onset of protest within two months before and after the month. The points and bars are the estimates and 95% confidence intervals of the Hodge-Lehman estimator. The red dotted line indicates null effect.

The above figure (Figure A2-10-1) shows that the effects are pronounced from two to nine months after elections. It usually takes several months until governments devise new policies after elections. Moreover, as time passes, the information revealed by the elections will become obsolete and its effect on the protest will disappear. Admittedly, it is difficult to specify the exact time until a government revises its policies and the time until the information becomes obsolete, but those results (three to nine months after polling) are, at least, not unreasonable.

SI 2-11. REPORTING BIASES AND PLACEBO TESTS

If the satellite-based rainfall measure were to be contaminated by reporting biases for some reason, the reporting biases could explain the main finding. As seen in Figure A2-11-1, without any correlation between the instrumental variable (rainfall deviation) and reporting biases in the ICEWS dataset, the reporting biases do not bias the causal estimate. The mere existence of reporting biases or correlations between the explanatory variable (turnout) and the reporting biases is not sufficient to bias the causal estimate. Moreover, because reporting frequencies are likely to be positively correlated with the onset of post-election protests, the causal estimate would be spurious only when the rainfall deviation is also *positively* correlated with reporting frequencies. Otherwise, the causal estimate would understate the true causal effect of turnout on protests, and thus the true causal effect would be larger than the estimated.

Figure A2-11-1. Causal Diagrams in Presence of Reporting Biases



NOTE: Causal diagrams with presence of reporting biases. In the left pane, reporting biases are associated with turnout but not with rainfall deviation, which does not bias the causal estimate. By contrast, in the right pane, reporting biases correlate with rainfall deviation, which does bias the IV estimate.

Although I do not find compelling reasons for such a possibility, I also conduct a placebo tests by replacing the outcome variables to the incidence of protests one-year *before* polling. The pre-election protests should not be affected by turnout but can still positively correlate with reporting biases. Thus, in absence of correlations between rainfall deviation and reporting biases, we should see no discernible relationship between the instrument and the pre-election protests. By contrast, when there is a correlation between rainfall deviation and reporting biases, there should be a correlation between the rainfall deviation and the pre-election protests as well.

The following table (Table A2-11-1) shows the estimate of a regression of the incidence of pre-election protests on the instrumental variable. As seen in the table, there is no evidence for possible relationship between protests and rainfall deviation. Moreover, the point estimate is negative, suggesting that the true causal estimate would be even stronger if there would happen to be reporting biases.

Table A2-11-1. The Effect of the Treatment on the Incidence of Pre-election Protests

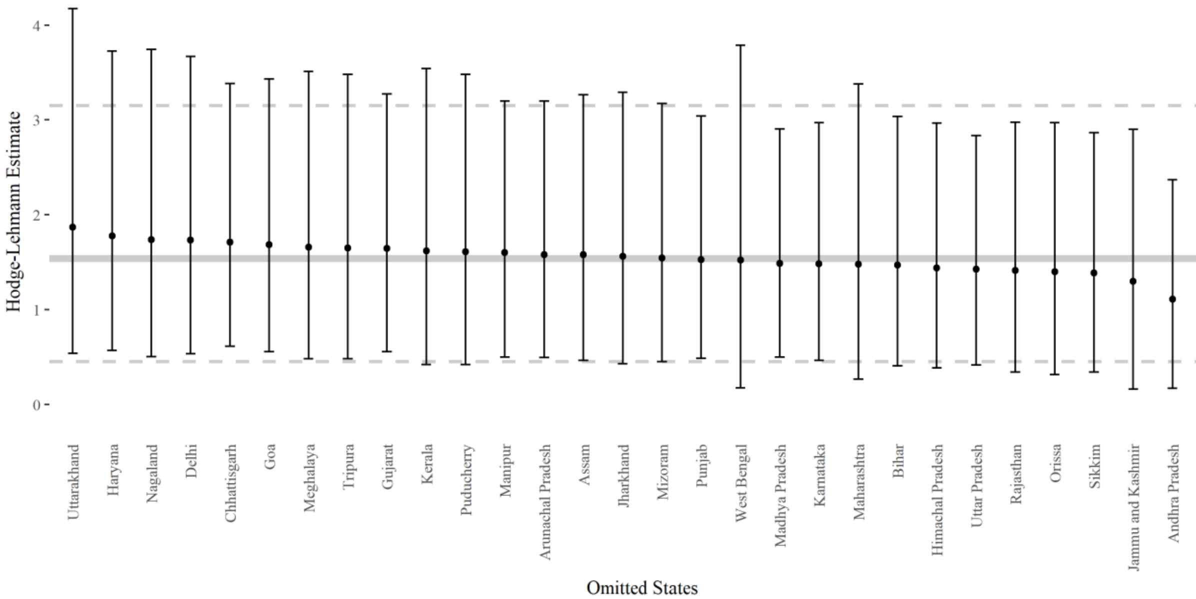
Point Estimate	95% Confidence Interval
-0.01	[-0.02, 0.001]

NOTE: The OLS estimate. In the near-far matching, 10% of observations are discarded for the purpose of better balance. $n =$ 11,322.

SI 2-12. LEAVE-ONE-STATE-OUT TESTS

Because the sample includes several states that are distinct in many respects, such as Jammu and Kashmir, Bihar, Sikkim, and Arunachal Pradesh, I repeat the analyses by omitting observations within each state. Figure A2-12-1 presents the Hodge-Lehmann estimates of the effect of turnout on protests when I drop a state from the sample. The results show that the effect of turnout is robust to presence or absence of a certain state.

Figure A2-12-1. The Leave-one-state-out Tests



NOTE: The figure shows the effect of turnout on the onset of protests when a certain state is omitted from the analysis. The horizontal and vertical axes are the names of the omitted states and the effect sizes. The points and error bars in the figure are the point estimates and corresponding 95% confidence intervals. The gray solid and dashed lines are the point estimate and corresponding 95% confidence interval when no state is dropped (the main estimate in the manuscript).

SI 2-13. LARGE VALUES IN AVERAGE RAINFALL DEVIATION

At the end of SI 2-4, I show that several constituencies take large values in average rainfall deviation, which is not surprising as there are only a few elections for each constituency, and hence if one election is held under heavy rainfall, the average rainfall deviation should take large values as well (by contrast, if there would be 100 or more elections, the mean rainfall deviation should converge to zero). To be sure, in this section, I drop those observations and re-estimate the effect of turnout on protest. In particular, based on the distribution of the average rainfall deviation in Figure A2-4-2, I drop constituencies whose absolute values of average rainfall deviation are over

0.5.[6] The following table (Table A2-13-1) shows the results. Although the effect size becomes slightly smaller, it still shows that high turnout has an effect to increase the risk of protests.

Table A2-13-1. The Effect of Turnout on Protest

Point Estimate	95% Confidence Interval
1.39	[0.26, 3.08]

NOTE: The Hodge-Lehmann estimate without constituencies whose absolute values of average rainfall deviation are over 0.5. In the near-far matching, 10% of observations are discarded for the purpose of better balance. $n = 9{,}920$. The first-stage F statistic is 25.46.

SI 2-14. CASE STUDY: THE SINGUR CONFLICT

In the bargaining model, I consider the following steps to protests;

(1) A high turnout rate and the resultant changes in the vote shares update the winning party's belief about their relative support bases;

(2) The updated information incentivizes the winning party to take hardline policies;

(3) The optimistic policies trigger protest, and the protest successfully forces the winning party to abandon the policies.

Since it is not possible to quantify all of these strategic actions or to make rigorous causal inferences about their relationship, I focus on the effect of turnout on the onset of protests in the main analysis, which is probably one of the most counterintuitive implications of the model. An analytical narrative however is helpful for understanding the formal model. To this end, I provide a focused case study of the Singur Conflict after the 2006 West State Assembly Election. The Singur Conflict can be best understood as a so-called "on-regression-line" case (high turnout and

[6] Those are 134 out of 4375 constituencies (before the 2008 delimitation) and 104 out of 4369 constituencies (after the delimitation).

protest), meaning that the case is representative of the theoretical and empirical models in the main paper. This case selection strategy is suitable for the purpose of the analytical narrative, that is to illustrate the strategic dynamics of the formal model. The following table (Table A2-14-1) is a timeline of the Singur conflict, which I detail in the following subsections.

Table A2-14-1. Timeline of the Singur Conflict and Elections

2005-07-23	The central government of India enacted the Special Economic Zone Act 2005.
2006-03-01	The schedule of the 2006 West Bengal State Assembly Election was announced.
2006-04-17	The first poll of the West Bengal State Assembly Election.
2006-05-08	The final poll of the West Bengal State Assembly Election.
2006-05-11	The results of the election were announced. The Left Front won the 2006 West Bengal Assembly Election.
2006-05-18	The government of West Bengal and Tata Motors jointly announced a Special Economic Zone plan at Singur, including construction of automobile factories.
2006-06-01	The first protest by over 3000 people at Singur.
2006-06-04	The Krishji Jami Raksha Committee, an organization of the protestors, was formed.
2006-07-17	The government of West Bengal issued notices of the land acquisition.
2006-07-26	The highway to Singur was blocked by the protestors.
2006-07-31	The government of West Bengal formally signed an agreement with Salim Group.
2006-08-08	A demonstration by about 5000 people at Singur.
2006-09-01	Over 100 villagers prevented the government officials from entering Singur.
2006-09-25	The government West Bengal started forceful land acquisition. A demonstration by about 5000 people at Singur.
	Escalation of the protest, including demonstration, general strikes, suicides, and deployment of armed police forces. Mamata Banerjee, the leader of the Trinamool Congress party, started a hunger strike and emerged as an opposition leader.
2008-10-03	Tata Motors announced their decision to move out from Singur.
	De-escalation of the protest. Although the government of West Bengal did not officially return the lands, the residents returned their home.
2011-05-11	The Trinamool Congress won the 2011 West Bengal Assembly Election. Mamata Banerjee was appointed as the Prime Minister of West Bengal.
2011-06-14	The government of West Bengal officially returned the lands at Singur.

Background

Singur is a small rural town of about 20,000 people, located about 40 kilometers northwest from Kolkata, the state capital of West Bengal. Over 90% of the population were Hindi, and the dominant economic sector was agriculture, including potatoes. After the central government of India enacted the Special Economic Zone (SEZ) Act in 2005, the Left Front, which had ruled West Bengal for nearly 30 years, started the preparation of the SEZ policies in West Bengal. Due to the proximity and highway access to Kolkata, Singur was considered as one of the six candidates of the SEZ. Although the West Bengal government did not publicly announce the Singur SEZ plan before the 2006 West Bengal State Assembly Election, they announced a similar plan at Nandigram, another rural town in West Bengal, creating a tension over the SEZ policies. No protest was however reported either at Singur or Nandigram before the 2006 election.

High Turnout and Surprising Victory

The West Bengal State Assembly Election in May 2006 ended with high turnout. The turnout rate increased from 75% in the last election to 81%, perhaps due to the increasing attention to the Nandigram dispute. Despite the tension, however, the Left Front won a surprising majority of the seats; the Communist Party of India (Marxist), the leading party of the leftist coalition, won 176 out of 279 seats (63%), which was an increase of 33 seats from the last election. In total, the Left Front secured 81% of the votes and 78% of the seats. Although the victory could be attributed to a number of factors, the skillful combination of land redistribution and industrial policies, strong party machineries, and absence of strong competitors were often cited as primary reasons (The Hindu 2006).[7] Interestingly, the landslide victory was surprising not only to general public but also

[7] The Hindu. (16 April 2006). "Why the Left will win West Bengal again." http://www.thehindu.com/todays-paper/tp-national/why-the-left-will-win-west-bengal-again/article3148130.ece (20 April 2018).

to the Left Front itself. After the election, the coalition leader Buddhadeb Bhattacharjee stated that the electoral results was "beyond all our expectations" (Outlook India 2006),[8] implying that the electoral victory was not something expected by their prior beliefs.

Government's Optimism

Given the high turnout, landslide victory, and public attention to the Nandigram conflict, it is not surprising that the government perceived the electoral victory as broad-based support for their SEZ policies. One week after the election (18 May 2006), the West Bengal government and Tata Motors jointly announced a new SEZ plan at Singur, including the acquisition of 10,000 acres of the land for the construction of automobile factories. Given the growing attention to the SEZ policies, this announcement was perceived as the government's strong commitment to the SEZ policies. In fact, retrospectively, a witness states that "the poll outcome was *wrongly interpreted* as a popular support in favour of the path followed by the LF for industrialisation" (Dinda 2013, 28).

Protest and the Left Front's Demise

The government's policy however triggered a series of protests at Singur. The first protest occurred in 1 June 2006 by over 3,000 landowners, and the protest entailed a series of demonstrations and strikes for several months. Although most of the protests are peaceful, the leftist government took hardline policies against the protestors, including the forceful land

[8] Outlook India. (11 May 2006). "Buddha Smiles."

https://www.outlookindia.com/website/story/buddha-smiles/231201(20 April 2018).

acquisition (25 September 2006), deployment of armed police (7 November 2006), and prohibition of public gathering at Singur (30 November 2006).

The government was however eventually forced to abandon the SEZ policies at Singur. The prohibition of assemblies was quashed by the Calcutta High Court on 14 February 2007. Tata Motors, which was increasingly concerned about the escalation of the protest and possible damages to its brand images, announced the relocation of the car manufacture plan on 3 October 2008. Not only failing to implement the policy, the Left Front was also forced to pay large, in fact fatal, political costs. The Singur conflict created the pro-industrialist image of the Leftist Front, alienating the large segment of the peasant voters. Moreover, although "the opposition parties had no leader who could match the charisma of Buddhadeb Bhattacharjee" (Outlook India 2006)[9] in the 2006 election, Mamata Banerjee, the leader of the Trinamool Congress party, emerged as a strong competitor out of the Singur movement. Not only did she organize a series of demonstrations and strikes, but also she committed herself to a hunger strike for a month, successfully creating a heroic image of an opposition leader. In the 2011 West Bengal State Assembly Election, "[t]he Trinamool Congress rose like a phoenix from the Singur and Nandigram movements chipping away at the hold of the Left Front to sweep into the portals of power in West Bengal after 34 years of uninterrupted Marxist rule" (The Economic Times 2011).[10] The Triamool

[9] Outlook India. (11 May 2006). "Buddha Smiles."

https://www.outlookindia.com/website/story/buddha-smiles/231201 (20 April 2018).

[10] The Economic Times. (13 May 2011). "Assembly election 2011 West Bengal: Trinamool Congress rises like phoenix." https://economictimes.indiatimes.com/news/politics-and-

Congress obtained 227 out of 296 seats, a dramatic increase by 197 seats from the last election. With the landslide victory, on 14 June 2011, Mamata Banerjee issued the land rehabilitation law to return the disputed land to their owners with compensation packages.

Discussion

Thus, at the end, the Singur SEZ policy was disastrous to the Left Front; they were not only forced to abandon the policy but also lost the election. Given these large costs, it would be hard to argue that the Left Front precisely understood that the consequences of the Singur SEZ policy. A more plausible explanation is that the Left Front miscalculated the strategic consequences. One source of such strategic miscalculation can be traced back to the high turnout and landslide victory in the 2006 election, which gave an impression that people would have unanimously supported the SEZ policies. Although it still remains unclear in the case of Singur conflict whether the high turnout or vote share was the main cause of the miscalculation, the landslide victory itself would have not been misinterpreted as broad-based support if the turnout rate would have been lower. In fact, the analysis in the main paper (Table 4) shows that a larger number of *loser* votes increases the risk of post-election protests.

Note that even though the Singur conflict involved a series of strikes and hunger strikes, the first incident was a peaceful demonstration. At the early stage of the conflict, the organizational support, especially from the Triamool Congress, was limited, and hence the contesters had few choices other than demonstration; given their peasantry occupations, strikes would be hard to

nation/assembly-election-2011-west-bengal-trinamool-congress-rises-like-phoenix/articleshow/8293363.cms (20 April 2018).

organize, and hunger strikes would also be too costly for them. Only after the Triamool Congress intervened the conflict and Mamata Banerjee exploited the case for her political aspiration, the opposition was able to organize and initiate strikes and hunger strikes. Thus, strikes and hunger strikes may or may not occur depending on particular political contexts, the onset of demonstration is less likely to be conditioned by these factors. This is consistent with the argument in the main paper (see *Causal Mechanisms I* subsection in the main paper).

Finally, in the case of the Singur conflict, the first protest occurred one month after the election. However, this fact does not necessarily contradict the findings in SI 2-10 that the turnout has a particular strong effect in three to nine months after elections, as the quantitative estimates are *average* effects. In fact, in the case of the Singur conflict, the government had a sufficient amount of time for policy making even before the election. After the central government of India enacted the Special Economic Zone Act 2005 (23 July), the West Bengal government started the preparation of the SEZ policy. This means that the West Bengal government was able to spend over eight months for the policy preparation even before the 2006 election. Therefore, if a government would have started the policy preparation after the election, or if opposition parties would have won the election and begun policy making, ceteris paribus, the new policy proposal should have been delayed by eight months. This means that if all conditions would have been the same, the protests should have occurred $8 + 1 = 9$ months after the election. The weighted average of the observed and counterfactual cases indicates $1(1 - w) + 9w \in [1{,}9]$ months ($w \in [0{,}1]$ is the proportion of counterfactual cases), which is roughly consistent with the quantitative finding. Thus, even though we need to be careful of generalizing these stylistic facts, there is a good reason to believe that the case of the Singur conflict is not inconsistent with the quantitative findings.

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Appendix III: Supporting Information for "It Never Rains But It Storms: Armed Conflict and Maritime Piracy as Strategic Substitutes"

SI 3-1. FORMAL ANALYSIS

The problem is maximizing $u = (b_G - y_G)y_G - c_G y_G + (b_M - y_M)y_M - c_M y_M$ subject to a resource constraint $y_G + y_M \leq r$ and $y_G, y_M \geq 0$. I solve the optimization problem by using the Karush–Kuhn–Tucker (KKT) conditions (Kuhn and Tucker 1951). The Lagrangian is;

$$L = (b_G - y_G)y_G - c_G y_G + (b_M - y_M)y_M - c_M y_M - \lambda(y_G + y_M - r).$$

Then, the KKT conditions are;

$$\begin{cases} \frac{\partial L}{\partial y_G} \leq 0,\ y_G \frac{\partial L}{\partial y_G} = 0,\ \text{and } 0 \leq y_G \leq r - y_M \\ \frac{\partial L}{\partial y_M} \leq 0,\ y_M \frac{\partial L}{\partial y_M} = 0,\ \text{and } 0 \leq y_M \leq r - y_G. \\ \frac{\partial L}{\partial \lambda} \leq 0,\ \lambda \frac{\partial L}{\partial \lambda} = 0,\ \text{and } \lambda \geq 0 \end{cases}$$

1. Interior solutions: $\lambda = 0$

1.1. When $y_G = y_M = 0$

The KKT condition is $y_G = y_M = 0, c_G > b_G$, and $c_M > b_G$.

1.2. When $y_G > 0$ and $y_M = 0$

The KKT condition is $y_G = \frac{b_G - c_G}{2}, y_M = 0, b_G - 2r < c_G \leq b_G$, and $c_M > b_M$.

1.3. When $y_G = 0$ and $y_M > 0$

The KKT condition is $y_G = 0, y_M = \frac{b_M - c_M}{2}, c_G > b_G$, and $b_M - 2r < c_M \leq b_M$.

1.4. When $y_G, y_M > 0$

The KKT condition is $y_G = \frac{b_G - c_G}{2}, y_M = \frac{b_M - c_M}{2}, c_G \leq b_G, c_M \leq b_M$, and $c_G + c_M > b_G + b_M - 2r$.

2. Boundary solutions: $\lambda > 0$

2.1. When $y_G = r$ and $y_M = 0$

The KKT condition is $y_G = r, y_M = 0, 0 < c_G < b_G - 2r$, and $c_G - c_M \leq b_G - b_M - 2r$.

2.2. When $y_G = 0$ and $y_M = r$

The KKT condition is $y_G = 0, y_M = r, 0 < c_M < b_M - 2r$, and $c_G - c_M \geq b_G - b_M + 2r$.

2.3. When $y_G, y_M > 0$

The KKT condition is reduced to $y_G = \frac{b_G - b_M - c_G + c_M + 2r}{4}, y_M = \frac{-b_G + b_M + c_G - c_M + 2r}{4}, \lambda = \frac{b_G + b_M - c_G - c_M - 2r}{2}$, and $x_G, x_M, \lambda > 0$. The later inequalities give binding conditions $b_G - b_M - 2r < c_G - c_M < b_G - b_M + 2r$ and $c_G + c_M < b_G + b_M - 2r$.

In summary, rebels' optimal choices are;

$$(y_M, y_G) = \begin{cases} (0,0) & \text{if} \quad c_G > b_G \text{ and } c_M > b_G \\ \left(0, \frac{b_G - c_G}{2}\right) & \text{if} \quad b_G - 2r < c_G \leq b_G \text{ and } c_M > b_M \\ \left(\frac{b_M - c_M}{2}, 0\right) & \text{if} \quad c_G > b_G \text{ and } b_M - 2r < c_M \leq b_M \\ \left(\frac{b_M - c_M}{2}, \frac{b_G - c_G}{2}\right) & \text{if} \quad c_G \leq b_G, c_M \leq b_M, \text{ and } c_G + c_M > b_G + b_M - 2r \\ (0, r) & \text{if} \quad 0 < c_G < b_G - 2r \text{ and } c_G - c_M \leq b_G - b_M - 2r \\ (r, 0) & \text{if} \quad 0 < c_M < b_M - 2r \text{ and } c_G - c_M \geq b_G - b_M + 2r \\ \left(\frac{-b_G + b_M + c_G - c_M + 2r}{4}, \frac{b_G - b_M - c_G + c_M + 2r}{4}\right) & \text{if} \quad b_G - b_M - 2r < c_G - c_M < b_G - b_M + 2r \text{ and } c_G + c_M < b_G + b_M - 2r \end{cases}.$$

The corresponding outcomes $(Y_M, Y_G) = \left(\mathrm{I}(y_M > 0), \mathrm{I}(y_G > 0)\right)$ are;

$$(Y_M, Y_G) = \begin{cases} (0,0) & \text{if} \quad c_G > b_G \text{ and } c_M > b_G \\ (0,1) & \text{if} \quad c_G \leq b_G \text{ and } (c_M > b_M \text{ or } c_G - c_M \leq b_G - b_M - 2r) \\ (1,0) & \text{if} \quad (c_G > b_G \text{ or } c_G - c_M \geq b_G - b_M + 2r) \text{ and } c_M \leq b_M \\ (1,1) & \text{if} \quad c_G \leq b_G, c_M \leq b_M, \text{ and } b_G - b_M - 2r < c_G - c_M < b_G - b_M + 2r \end{cases}.$$

SI 3-2. DESCRIPTIVE STATISTICS

This supporting information provides basic information of the data that are used in the main analysis. The following table (Table A3-2-1) lists the countries and periods that are selected based on the criteria I detailed in the paper.

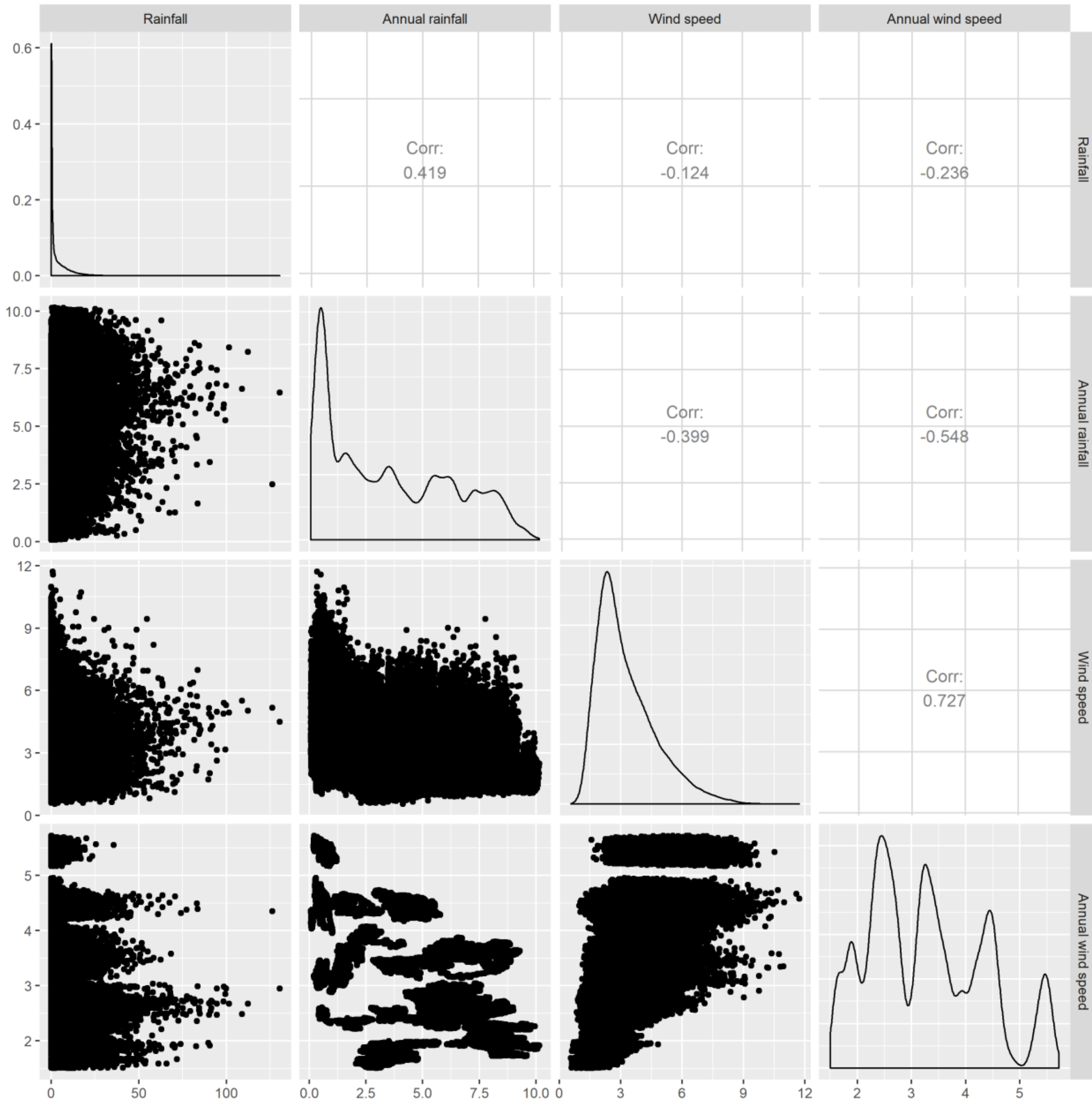
Table A3-2-1. List of Countries

Country	n	Beginning	End	Country	n	Beginning	End
Algeria	5845	12/31/2000	12/31/2016	Liberia	1056	12/31/2000	11/21/2003
Angola	3288	12/31/2000	12/31/2009	Libya	1745	2/28/2012	12/7/2016
Bangladesh	5845	12/31/2000	12/31/2016	Malaysia	4498	12/31/2000	4/24/2013
Colombia	5843	12/31/2000	12/29/2016	Mauritania	3946	12/31/2000	10/20/2011
Congo	5824	12/31/2000	12/10/2016	Mozambique	5783	12/31/2000	10/30/2016
DRC	5845	12/31/2000	12/31/2016	Myanmar	5845	12/31/2000	12/31/2016
Ivory Coast	2778	9/19/2003	4/27/2011	Nigeria	5845	12/31/2000	12/31/2016
Egypt	5845	12/31/2000	12/31/2016	Peru	3652	12/31/2000	12/30/2010
Eritrea	952	12/31/2000	8/9/2003	Philippines	5845	12/31/2000	12/31/2016
Haiti	1420	12/31/2000	11/19/2004	Senegal	4017	12/31/2000	12/30/2011
India	5845	12/31/2000	12/31/2016	Sierra Leone	355	12/31/2000	12/20/2001
Indonesia	1747	12/31/2000	10/12/2005	Somalia	5845	12/31/2000	12/31/2016
Iran	5834	12/31/2000	12/20/2016	Sri Lanka	3108	12/31/2000	7/4/2009
Iraq	5845	12/31/2000	12/31/2016	Thailand	5845	12/31/2000	12/31/2016
Kenya	5845	12/31/2000	12/31/2016	Yemen	5845	12/31/2000	12/31/2016

NOTE: The table lists the countries that are selected based on the criteria which I detailed in the paper. The table also provides the numbers of observations, earliest dates, and last dates of the observations for the countries.

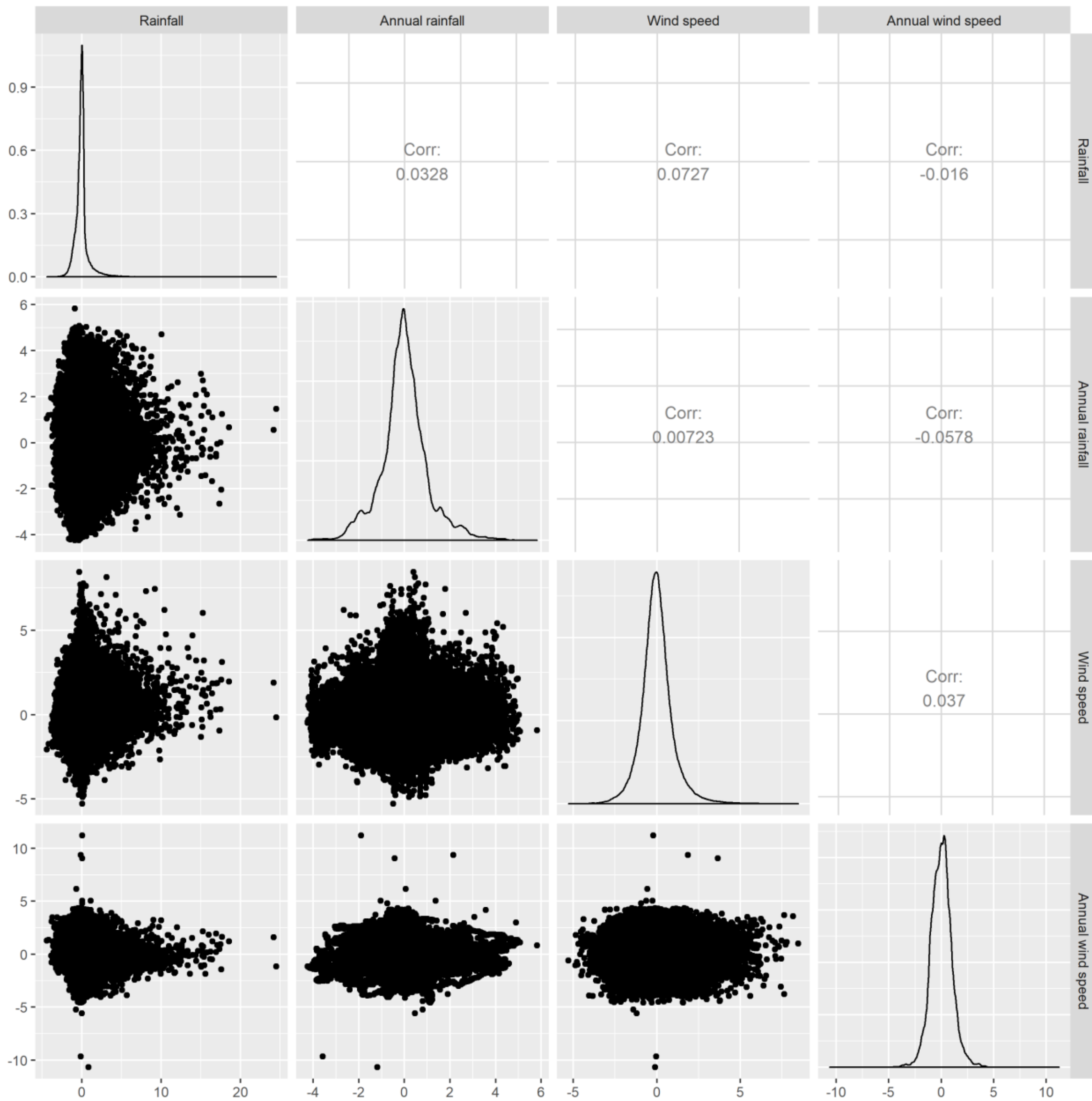
The following figures (Figure A3-2-1 and Figure A3-2-2) show descriptive statistics of the weather variables before and after they are de-meaned, deseasoned, and standardized. As seen in the relationship between annual rainfall and wind speed in Figure A3-2-1, without transformation, the variables have differences across countries, resulting in non-smooth distribution and bivariate relationships.

Figure A3-2-1. Descriptive Statistics (before transformation)



NOTE: The figure shows the descriptive of statistics of the weather variables before they are de-meaned, deseasoned, and standardized. The diagonal panes are density plots of the variables. The panes in the lower corner are bivariate scatter plots of the variables. The panes in the upper corner show the correlation coefficients of the variables.

Figure A3-2-2. Descriptive Statistics (after transformation)



NOTE: The figure shows the descriptive of statistics of the weather variables after they are de-meaned, deseasoned, and standardized. The diagonal panes are density plots of the variables. The panes in the lower corner are bivariate scatter plots of the variables. The panes in the upper corner show the correlation coefficients of the variables.

Finally, the following table (Table A3-2-2) is a contingency table of violence and piracy (before they are de-meaned, de-seasoned, and standardized). As seen in the table, although there are a fair numbers of violent and piracy events, it is extremely rare to have both violence and piracy.

Table A3-2-2. Contingency Table

		$piracy_{it}$	
		0	1
$violence_{it}$	0	119,659 (90.77%)	22,278 (1.73%)
	1	9,445 (7.16%)	449 (0.34%)

NOTE: The table is a contingency table of $violence_{it}$ and $piracy_{it}$. The parentheses are proportions in the sample

SI 3-3. DIFFERENT THRESHOLDS OF COASTAL DISTANCES

In the paper, I use 100 kilometers as a threshold for determining the coastal land and sea areas. In this robustness check, I conduct additional analyses with two different thresholds: 50 and 200 kilometers. The following table (Table 1) shows the results of the analyses.

Table A3-3-1. Results with Different Thresholds of Coastal Distances

	50-kilometer threshold		200-kilometer threshold	
Outcome	$violence_{it}$	$piracy_{it}$	$violence_{it}$	$piracy_{it}$
$rain_{it}$	-0.0038 [-0.0100, 0.0024]	0.0039 [0.00032, 0.0074]	-0.0044 [-0.0110, 0.0021]	0.00073 [-0.0031, 0.0045]
$wind_{it}$	-0.0020 [-0.0092, 0.0053]	-0.0050 [-0.0106, 0.0007]	-0.0053 [-0.0159, 0.0053]	-0.0041 [-0.0096, 0.0015]
$rain_{it}wind_{it}$	-0.0054 [-0.0093, -0.0015]	0.0065 [-0.0026, 0.0156]	-0.0032 [-0.00642, 0.0001]	0.006 [-0.0016, 0.0122]

NOTE: The table shows the OLS estimates of the standardized coefficients. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, cubic splines of common time trends. The control variables are the past-one-year averages of ground rainfall and ocean wind speed, and their interaction. $n = 131{,}831$ with 30 coastal conflict countries for 2001-2016 period.

With the 200-kilometer threshold, the p-value of the interactive effect on violence (third column of Table) increases to 0.055. This can explained by the fact that with the 200-kilometer threshold, the violent events include those in far inland areas, which are less relevant to piracy or ocean weather conditions.

SI 3-4. RESULTS WITH THE GTD AND MPELD MEASURES

Although I use the UCDP GED and MPD as main data sources of violence and piracy in the main analysis, it is also useful to check the robustness of the findings with different datasets. To this end, I present the results with the Global Terrorism Database (GTD; LaFree and Dugan 2007) and the Maritime Piracy Event and Location Dataset (MPELD; Daxecker and Prins 2013) in this supporting information. As seen in Table A3-4-1, the main results hold even with those alternative measures.

Table A3-4-1. Results with Alternative Measures of Violence and Piracy

Outcome	$violence_{it}$ (GTD)	$piracy_{it}$ (MPELD)
$rain_{it}$	-0.0019 [-0.0115, 0.0076]	0.0033 [-0.0031, 0.0097]
$wind_{it}$	-0.0056 [-0.0158, 0.0045]	-0.0061 [-0.0142, 0.0021]
$rain_{it}wind_{it}$	-0.0049 [-0.0088, -0.0009]	0.0075 [0.0006, 0.0143]

NOTE: The table shows the OLS estimates of the standardized coefficients. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, cubic splines of common time trends. The control variables are the past-one-year averages of ground rainfall and ocean wind speed, and their interaction. $n = 131{,}831$ with 30 coastal conflict countries for 2001-2016 period.

When I use the MPELD measure for $piracy_{it}$, the interactive effect becomes positive and statistically significant. However, as I mentioned in the paper, this does not necessarily mean that there would be complementary relationship between violence and piracy. Since the MPELD does not identify the origins of pirates and includes pirate attacks that occurred within 100 kilometers from coastal lines of a country, one possible explanation would be that while rebels in country i

are less or no more likely to engage in neither ground nor marine military activities with stormy weather, it actually invites an even larger number of foreign pirates who try to exploit the void, resulting in an increase in reported piracy events. Formally establishing this argument or rigorously testing the hypothesis is beyond the scope of this paper, so I leave it as a task for future studies. For the purpose of this paper, it is sufficient to state that the information about pirates' origins is critical for the main analysis.

SI 3-5. CONTROLLING FOR TEMPERATURE

In the paper, I do not include temperature as a control variable, as rainfall can causally affect temperature and hence controlling for temperature can cause a bias due to the posttreatment control. It is, however, likely that temperature correlates with rainfall and also affects violence and piracy, creating a backdoor path, $rain \leftarrow climate\ system \rightarrow temperature \rightarrow violence$. In this supporting information, I therefore control for daily minimum and maximum temperatures in order to account for possible omitted variable biases. The temperature data come from NOAA Climate Prediction Center (CPC) Global Temperature Monitoring dataset, which is available daily at a spatial resolution of 0.5-degree by 0.5-degree grid cells (NOAA 2018). As seen Table A3-5-1, controlling for temperature does not alter my findings.

Table A3-5-1. Results with Controlling for Temperature

Outcome	$violence_{it}$	$piracy_{it}$
$rain_{it}$	-0.0053 [-0.0132, 0.0026]	0.0016 [-0.0026, 0.0058]
$wind_{it}$	-0.0038 [-0.0125, 0.0048]	-0.0059 [-0.0122, 0.0004]
$rain_{it}wind_{it}$	-0.0040 [-0.0065, -0.0015]	0.0061 [-0.0022, 0.0144]

NOTE: The table shows the OLS estimates of the standardized coefficients. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, cubic splines of common time trends. The control variables are the past-one-year averages of ground rainfall and ocean wind speed, their interaction, and daily minimum and maximum temperature. $n = 131{,}831$ with 30 coastal conflict countries for 2001-2016 period.

SI 3-6. DIFFERENT MOVING AVERAGES OF WEATHER VARIABLES

In the paper, I control for past-one-year averages of rainfall and ocean wind speed to account for the effect of long-term weather conditions, such as those on crop production and economic growth (Miguel, Satyanath, and Sergenti 2004). Controlling for long-term weather conditions is useful to limit the focus to day-to-day tactical variation. I use the one-year-averages as they always include every season in a year regardless of countries. However, it is useful to show the results with different moving averages of the weather variables. The following table (Table A3-6-1) shows the results with controls for past-one-month or past-one-week averages of the rainfall and ocean wind speed. As seen in the table, the main results hold even with these controls.

Table A3-6-1. Control for Past-one-month or Past-one-week Mean of Rainfall and Wind

	Past-one-month Averages		Past-one-week Averages	
Outcome	$violence_{it}$	$piracy_{it}$	$violence_{it}$	$piracy_{it}$
$rain_{it}$	-0.0032 [-0.0092, 0.0028]	0.0019 [-0.0018, 0.0057]	-0.0035 [-0.0098, 0.0028]	0.00220 [-0.0012, 0.0056]
$wind_{it}$	-0.0019 [-0.0105, 0.0066]	-0.0032 [-0.0091, 0.0027]	-0.0021 [-0.0117, 0.0075]	-0.0045 [-0.0104, 0.0013]
$rain_{it}wind_{it}$	-0.0042 [-0.0069, -0.0014]	0.0051 [-0.0027, 0.0130]	-0.0045 [-0.0075, -0.0016]	0.0060 [-0.0025, 0.0145]

NOTE: The table shows the OLS estimates of the standardized coefficients. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, cubic splines of common time trends. The control variables are the past-one-month (first and second columns) or past-one-week (third and fourth columns) averages of ground rainfall and ocean wind speed, and their interaction. $n = 131{,}831$ with 30 coastal conflict countries for 2001-2016 period.

SI 3-7. CONTROLLING FOR COUNTRY-SPECIFIC TRENDS

In this supporting information, I present the results with controls for cubic splines of country-specific trend variables, which can account for unobserved time-varying heterogeneities within countries. For instance, even though the common trend variable in the main model can account for climate changes and global warming, each country might have a different climate change trend. In this case, the country-specific trend variables can account for the heterogeneities within countries. The following table (Table A3-7-1) indicates that this does not change the results.

Table A3-7-1. Results with Controlling for Country-specific Trends

Outcome	$violence_{it}$	$piracy_{it}$
$rain_{it}$	-0.0045 [-0.0115, 0.0024]	0.0030 [-0.0006, 0.0066]
$wind_{it}$	-0.0040 [-0.0117, 0.0038]	-0.0060 [-0.0123, 0.0003]
$rain_{it}wind_{it}$	-0.0048 [-0.0083, -0.0013]	0.0057 [-0.0025, 0.0139]

NOTE: The table shows the OLS estimates of the standardized coefficients. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, cubic splines of common time trends, and cubic splines of country-specific trends. The control variables are the past-one-year averages of ground rainfall and ocean wind speed, and their interaction. $n = 131{,}831$ with 30 coastal conflict countries for 2001-2016 period.

SI 3-8. CONTROLLING FOR COUNTRY-YEAR FIXED EFFECTS

Although it is unlikely that covariates, such as GDP per capita and democracy indexes, would affect the weather variable and bias the causal estimates, it is nevertheless useful to account for possible effects of these country-year covariates. To this end, I conduct an analysis with country-year fixed effects. The following table (Table A3-8-1) indicates that the results are robust to the inclusion of the country-year fixed effects. This is not surprising given the fact that the explanatory variables are exogenous to those country-year covariates.

Table A3-8-1. Results with Controlling for Country-year Fixed Effects

Outcome	$violence_{it}$	$piracy_{it}$
$rain_{it}$	-0.0036 [-0.0086, 0.0015]	0.0038 [0.0000, 0.0075]
$wind_{it}$	-0.0051 [-0.0124, 0.0022]	-0.0059 [-0.0117, 0.0002]
$rain_{it}wind_{it}$	-0.0060 [-0.0100, -0.0019]	0.0061 [-0.0021, 0.0143]

NOTE: The table shows the OLS estimates of the standardized coefficients. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, cubic splines of common time trends, and country-year fixed effects. The control variables are the past-one-year averages of ground rainfall and ocean wind speed, and their interaction. $n = 131{,}831$ with 30 coastal conflict countries for 2001-2016 period.

SI 3-9. INCLUSION OF ZERO-VARIANCE COUNTRIES

In the main analysis, I exclude the 14 coastal conflict countries that have zero variance in $violence_{it}$ or $piracy_{it}$ in the main analysis. As I detail in the footnote 17 in the paper, piracy or rebels' violence are near-impossible in these countries, and these cases do not fit well to my theoretical argument. Nonetheless, it is also useful to check the robustness of the findings to the inclusion of those cases. As seen in Table A3-9-1, even though the inclusion of the zero-variance cases attenuates the within-country variances in the outcome variables and decreases the statistical significance ($p = 0.0567$), the main results hold.

Table A3-9-1. Results with All Coastal Conflict Countries

Outcome	$violence_{it}$	$piracy_{it}$
$rain_{it}$	-0.0049 [-0.0105, 0.0008]	0.0020 [-0.0008, 0.0048]
$wind_{it}$	-0.0022 [-0.0081, 0.0038]	-0.0032 [-0.0068, 0.0005]
$rain_{it}wind_{it}$	-0.0021 [-0.0042, 0.0001]	0.0045 [-0.0011, 0.0102]

NOTE: The table shows the OLS estimates of the standardized coefficients. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, and cubic splines of common time trends. The control variables are the past-one-year averages of ground rainfall and ocean wind speed, and their interaction. $n = 189{,}696$ with 44 coastal conflict countries for 2001-2016 period.

SI 3-10. COUNT VARIABLES

In the main analysis, I use the dichotomized outcomes of rebels' violence and piracy attacks. As I mentioned in the theory section, the dichotomization is useful for developing a *typology* of substitution. Furthermore, since the continuous effort levels y_G and y_M are complicated non-linear functions of c_G and c_M, we can only weakly identify the continuous functions in an empirical analysis. In addition, the counts of violent and piracy events have skewed distributions, which make the estimation of the interactive models sensitive to outliers. Despite these facts, it is still useful to provide the results with the count variables. The following table (Table A3-10-1) shows the estimates coefficient values when I use the counts of rebels' violence and piracy attacks.

Table A3-10-1. Results with Count Variables

Outcome	$violence\ (count)_{it}$	$piracy\ (count)_{it}$
$rain_{it}$	-0.0052 [-0.0109, 0.0005]	0.0008 [-0.0031, 0.0046]
$wind_{it}$	-0.0037 [-0.0118, 0.0043]	-0.0077 [-0.0174, 0.0019]
$rain_{it}wind_{it}$	-0.0047 [-0.0106, 0.0011]	0.0060 [-0.0009, 0.0128]

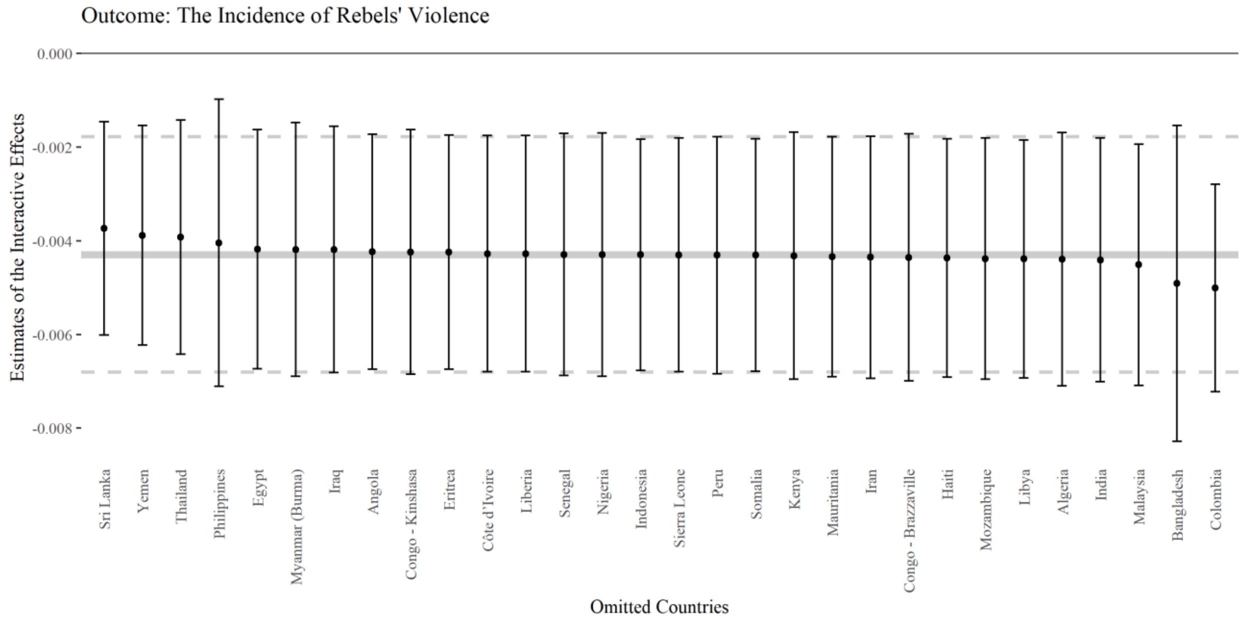
NOTE: The table shows the OLS estimates of the standardized coefficients. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, and cubic splines of common time trends. The control variables are the past-one-year averages of ground rainfall and ocean wind speed, and their interaction. $n = 131{,}831$ with 30 coastal conflict countries for 2001-2016 period.

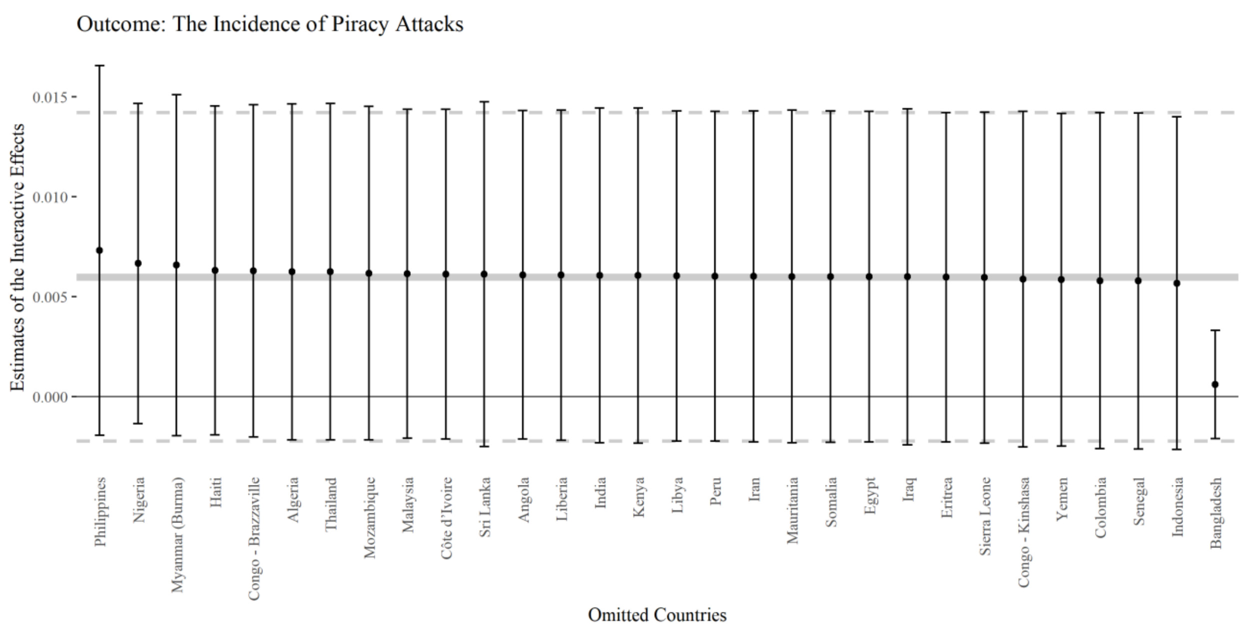
The p-values for the interactive effects are 0.105 and 0.084 for violence and piracy respectively. Consistent with my expectation, these results suggest that the interactive effects might exist but it is difficult to empirically detect the interactive effects. These are not surprising given the potentially complicated functional forms of the interactive effects, and the skewness of the count variables.

SI 3-11. LEAVE-ONE-COUNTRY-OUT TESTS

One may be concerned about whether a few outliers, such as Somalia and Nigeria, drive the empirical findings. I address this concern by conducting leave-one-country tests, in which I drop each country and re-estimate the regression models. The following figures (Figure A3-11-1) shows the estimates of the coefficients for the interaction terms (β_3 and γ_3 in the equation in the main paper) when each country in the horizontal axis is omitted from the sample. Regardless of the omitted countries, ground and ocean weather conditions interactively affect the incidence of rebels' violence, while there is no such evidence for the incidence of piracy attacks. When I drop Bangladesh, the point estimate about the interactive effect on $piracy_{it}$ becomes near zero, and the confidence interval becomes narrower. This is because Bangladesh is one of the three countries within which the interactive effect is positive and statistically significant, and Bangladesh has the largest number of piracy incidences per year among those three countries (the other two countries are Colombia and Yemen). This implies that the seemingly positive interactive effect is driven by the outlier, and if we remove the outlier, the confidence interval becomes even closer to zero. These results provide further evidence for the "no" interactive effect on $piracy_{it}$, which is consistent with the upward substitution hypothesis.

Figure A3-11-1. Leave-one-country-out Tests





NOTE: The figures show the estimates of the coefficients for the interaction terms when each country in the horizontal axis is omitted from the sample. In the first and second figures, the outcome variables are the incidences of rebels' violence and piracy attacks respectively. The point estimates and corresponding 95% confidence intervals are the dots and error bars. The gray solid and dashed horizontal lines are the estimates and confidence intervals when no country is dropped.

SI 3-12. MECHANISM: FISHERY

As I discuss in the manuscript, a possible alternative explanation of my findings is fishery; rainfall decreases rebel violence especially when the ocean is windy, for rough seas may limit opportunities for fishery industries and people might alternatively engage in violence. Although I am skeptical on this view as switching from non-violent (fishing) to violent activities (violence) is usually more difficult than switching between violent activities (piracy and violence), I also conduct an additional analysis to test this possibility. I collect data on phytoplankton absorption coefficient, which is a measure of phytoplankton abundance in the ocean (Flückiger and Ludwig 2015). Since phytoplankton abundance is a predictor of fish abundance and does not affect operational costs of piracy activities, I can exclude the alternative explanation if the results hold even after controlling for phytoplankton abundance. Note that this causal mechanism test can suffer posttreatment control bias as ocean wind can affect phytoplankton abundance.

The phytoplankton data are derived from the MODIS Aqua products. I calculate the average phytoplankton absorption coefficients within 100 kilometers from coastal lines. Due to the MODIS's limited coverage, the values of the phytoplankton variable are missing for 43,489 observations (32.99% of the sample; the within-100-kilometer average). Since the missing values are due to almost-random variation in the satellite orbits, the missing-completely-at-random assumption is not implausible,[1] and hence I drop those observations of missing values. In the following analysis, the phytoplankton variable and its interaction with ground rainfall are added

[1] In fact, even with list-wise deletion, the estimates of the regression models are very similar to the main results.

to the regression model.[2] As seen in Table A3-12-1, even when I account for the effect of phytoplankton abundance, it does not change the main findings.

Table A3-12-1. Results with Phytoplankton Abundance

Outcome	$violence_{it}$	$piracy_{it}$
$rain_{it}$	-0.0094 [-0.0214, 0.0026]	-0.0001 [-0.0084, 0.0083]
$wind_{it}$	-0.0051 [-0.0167, 0.0066]	-0.0062 [-0.0136, 0.0012]
$plankton_{it}$	-0.0070 [-0.0153, 0.0013]	-0.0003 [-0.0069, 0.0063]
$rain_{it}wind_{it}$	-0.0116 [-0.0225, -0.0008]	0.0023 [-0.0023, 0.0070]
$rain_{it}plankton_{it}$	0.0037 [-0.0056, 0.0129]	-0.0035 [-0.0088, 0.0017]

NOTE: The table shows the OLS estimates of the standardized coefficients. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, and cubic splines of common time trends. The control variables are the past-one-year averages of ground rainfall and ocean wind speed, and their interaction. $n = 88{,}342$ with 30 coastal conflict countries for 2001-2016 period.

SI 3-13. MECHANISM: TACTICAL OR MONETARY INCENTIVES

One may also be interested in whether the main findings are driven by rebels' tactical or monetary incentives. As I discussed in the theory section of the main paper, the "profits" that the rebel group received can be the tactical control over territory and navigation or the monetary gain from looting and ransom activities. I test these causal mechanisms by disaggregating the outcome variables. In particular, the variable $violence_{it}$ is disaggregated into three variables— $violence_{state,it}$, $violence_{nonstate,it}$ and $violence_{onesided,it}$ —which correspond to the incidences of rebels' violence against a government, non-government organizations, and civilians respectively. If the

[2] Due to missing values, I cannot calculate the past-one-year average of the phytoplankton variable without massive interpolation, hence I do not include the variable.

substitution is driven by rebels' *tactical* choices, the interactive effect should exist on $violence_{state,it}$ and $violence_{nonstate,it}$, as these activities involve transfers of territorial controls. In contrast, the monetary explanation predicts that the interactive effect exists on $violence_{onesided,it}$ through which rebels can loot civilians' possessions and demand ransoms. If both mechanisms exist, we should see the interactive effects on both variables.

Since $violence_{onesided,it}$ can potentially affect rebels' control over territories as well (Kalyvas 2006), the comparison of the different outcomes may not constitute a "shape" test for the causal mechanisms. I therefore disaggregate the GTD-based outcome variable to those relating to tactical objectives ($violence_{tactical,it}$; unarmed and armed assaults, bombing, facility destruction, and assassination) and those relating to monetary gains ($violence_{monetary,it}$; kidnapping and hijacking). The tactical explanation expects a negative interactive effect on the former variable, while the monetary explanation predicts a negative interactive effect on the latter variable.

I also disaggregate $piracy_{it}$ to those that involve looting activities or ransom demands ($piracy_{monetary,it}$) and those without those actions ($piracy_{tactical,it}$). The MPD contains whether each piracy attack involves looting activities or ransom demands (Coggins 2012). While the downward and equivalent substitutions expect the interactive effect on those variables, the upward substitution, for which I find evidence in the empirical analysis, does not expect an interactive effect on any of those variable. The following table (Table A3-13-1) summarizes the effects on the disaggregated outcomes.

Table A3-13-1. Results with Disaggregated Outcome Variables

Outcome	UCDP GED $violence_{state,it}$	UCDP GED $violence_{nonstate,it}$	UCDP GED $violence_{onesided,it}$
$rain_{it}$	-0.0045 [-0.0118, 0.0028]	-0.0021 [-0.0080, 0.0037]	-0.0027 [-0.0094, 0.0040]
$wind_{it}$	-0.0038 [-0.0148, 0.0072]	0.0021 [-0.0032, 0.0075]	-0.0029 [-0.0105, 0.0047]
$rain_{it}wind_{it}$	-0.0026 [-0.0045, -0.0008]	-0.0026 [-0.0055, 0.0004]	-0.0014 [-0.0047, 0.0020]
Outcome	**GTD $violence_{tactical,it}$**	**GTD $violence_{monetary,it}$**	
$rain_{it}$	-0.0013 [-0.0097, 0.0071]	-0.0021 [-0.0062, 0.0019]	
$wind_{it}$	-0.0043 [-0.0146, 0.0060]	-0.0025 [-0.0122, 0.0071]	
$rain_{it}wind_{it}$	-0.0039 [-0.0084, 0.0006]	0.0010 [-0.0030, 0.0010]	
Outcome	**MPD $piracy_{tactical,it}$**	**MPD $piracy_{monetary,it}$**	
$rain_{it}$	0.0020 [-0.0028, 0.0069]	0.0015 [-0.0027, 0.0056]	
$wind_{it}$	-0.0030 [-0.0080, 0.0021]	-0.0039 [-0.0091, 0.0013]	
$rain_{it}wind_{it}$	0.0028 [-0.0003, 0.0060]	0.0062 [-0.0024, 0.0147]	

NOTE: The table shows the OLS estimates of the standardized coefficients. The 95% confidence intervals (based on clustered standard errors and corrected degrees of freedom) are in brackets. The models include country fixed effects, country-month-day fixed effects, and cubic splines of common time trends. The control variables are the past-one-year averages of ground rainfall and ocean wind speed, and their interaction. $n = 131{,}831$ with 30 coastal conflict countries for 2001-2016 period.

As seen in Table A3-13-1, the interactive effects are statistically significant only for the outcomes relating to tactical objectives. The coefficients of the interactive terms are statistically significant for $violence_{state,it}$ and somewhat significant on $violence_{nonstate,it}$ ($p = 0.086$) but not for $violence_{onesided,it}$ ($p = 0.397$). Similarly, when I used the GTD-based variables, there exists a weakly significant interactive effect on $violence_{tactical,it}$ ($p = 0.097$), while the corresponding effect on $violence_{monetary,it}$ is far from the conventional threshold of statistical significance ($p = 0.319$).

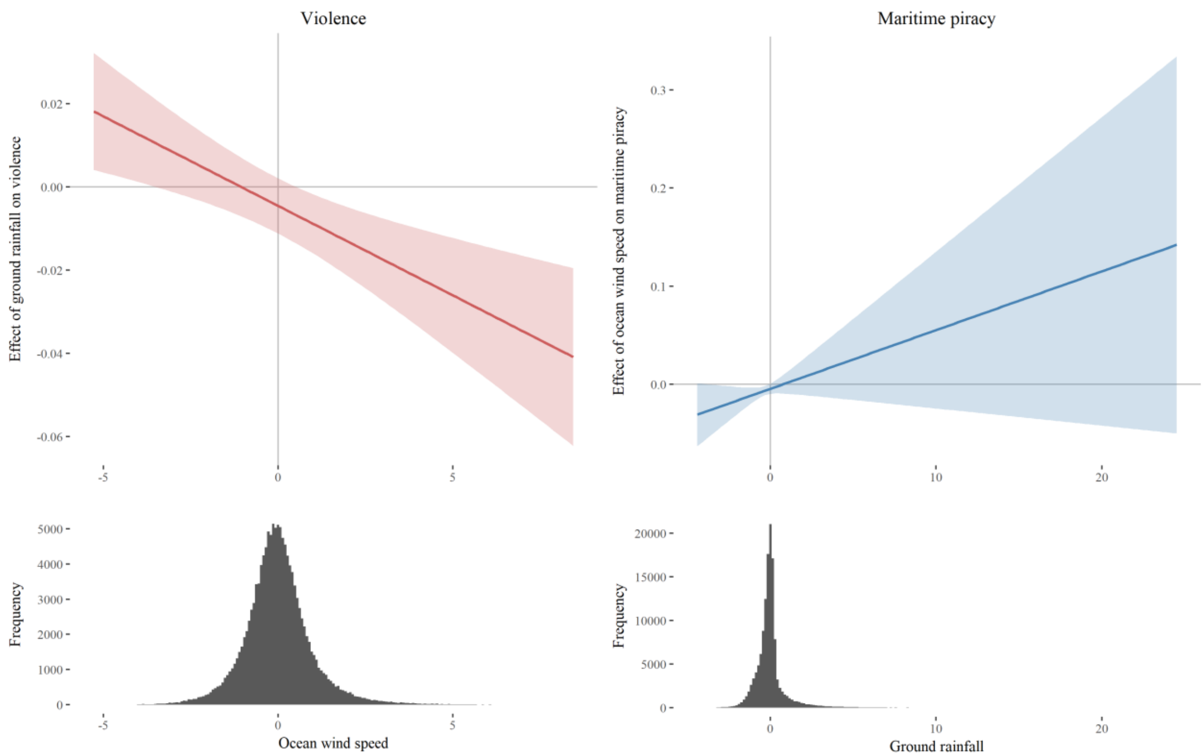
Finally, consistent with the upward substitution hypothesis, the coefficient of the interaction term is not statistically different from zero for $piracy_{monetary,it}$ ($p = 0.148$). By contrast, the interactive effect on $piracy_{tactical,it}$ is positive and weakly significant ($p = 0.075$). This implies that ocean wind decreases the likelihood of *tactical* piracy attacks when it is sunny. One possible explanation for the positive interactive effect would be that rainy weather may prevent pirates from sailing out to the ocean and hence that there is no piracy attacks regardless of ocean wind speed. However, we should not over-interpret this result. As I explained in Supporting information 11, the weak positive interactive effect is driven by Bangladesh. Indeed, when I drop the outlier, the weak statistical significance disappears ($p = 0.294$), and the confidence interval becomes closer to zero $[-0.0008, 0.0026]$. Overall, those results provide some support for the tactical mechanism and the upward substitution, while there is no such evidence for the monetary mechanism.

SI 3-14. MARGINAL EFFECT PLOTS

In the paper, I do not present the marginal effect plots partly because I use linear models, in which the coefficients of the interaction terms have direct interpretation, and also because the marginal effect plots are misleading especially in a regression of $piracy_{it}$. In this supporting information, I present the marginal effect plots and explain why the plots can be potentially misguiding.

As seen in Figure A3-14-1, the marginal effect of rainfall (top-left pane) is steadily decreasing as ocean wind speed become stronger, and its confidence interval is within a reasonable range. By contrast, the marginal effect of ocean wind speed (top-right pane) is increasing with an explosively large confidence intervals. Although the point estimates might suggest sizable interactive effects, such an interpretation is misguided because the confidence intervals are very large and we therefore cannot make meaningful inferences from the data. A reason for those results lies in the skewness of the distribution of the conditioning variables (bottom panes of A3-14-1). As seen in the bottom-right pane, ground rainfall has a skewed distribution. As a result, the interactive effects on piracy is influenced by a few outliers of extremely heavy rainfall, providing a misleading picture of the conditioning effect. In contrast, the distribution of ocean wind speed is fairly close to a normal distribution, which makes the marginal effect at the edge values of $wind_{it}$ representative of the data and limits the confidence interval within a reasonable range.

Figure A3-14-1. Marginal Effect Plots



NOTE: The top left plot shows the marginal effect of rainfall on the incidence of violence with respect to ocean wind speed. The top right plot shows the marginal effect of wind speed on the incidence of maritime piracy with respect to rainfall. The values of the conditioning variables (horizontal axis) are from the minimum to maximum of the variable. The envelopes are 95% confidence intervals. The bottom figures are histograms of ocean wind speed and ground rainfall. All of the variables are de-meaned, de-seasoned, and standardized.

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