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Causes and Consequences of State Violence against Civilians: The Rohingya of Myanmar*

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Abstract

While the United Nations describes Myanmar’s oppression of the Rohingya as “a textbook example of ethnic cleansing” (UN, 2017), the state maintains that the violence was idiosyncratic and not motivated by anti-Rohingya animus. We assemble existing and original large-sample data to evaluate these claims. First, we document systematic economic motives: violence against minority civilians increased in places suitable for rice cultivation when rice prices were high. Correspondingly, in an original representative survey of Rohingya refugees in Bangladesh we find substantial losses of agricultural land, inputs, and inventories. Next, using a vector auto-regression approach, we find that state violence was consistent with Rohingya-specific animus. The state attacked substantially more than the Rohingya militia, targeted civilians disproportionately relative to other ethnic conflicts in Myanmar, and leveraged nationalist religious ideology. Finally, we document high rates of trauma exposure and depression among Rohingya refugees. Together, these results strongly rebut the government’s narrative and illustrate how quantitative tools can shed light on episodes of ethnic cleansing.

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1 Introduction

Episodes of violence against civilians exact deep human costs. Their origins and existence are frequently politically contested, and they often take place in contexts of limited data availability. These characteristics make representative evidence both essential and challenging to assemble. In this paper, we demonstrate how researchers can leverage diverse data sources and quantitative methods to shed light on violence against civilians. We study Myanmar’s treatment of the Rohingya people, described by observers as “the most persecuted minority in the world” suffering “a textbook example of ethnic cleansing” (UN, 2017). The latest period of violence began in August 2017 and displaced over one million refugees (UNHCR, 2021).

The nature of the conflict is hotly contested. In November 2019, the Gambia initiated proceedings against Myanmar in the International Court of Justice (ICJ), alleging that the state’s actions were “genocidal in character”. Plaintiffs cited primary accounts of victims and secondary accounts of observers to support the lawsuit (Gambia v. Myanmar, 2019). The Myanmar government has described this evidence as “inaccurate” and “exaggerated” (Suu Kyi, 2020a) and claimed that the violence was idiosyncratic, symmetric to militia attacks, not civilian-targeted, and not shaped by anti-Rohingya ideology. The former head of state, Aung San Suu Kyi, stated that it was “not easy to establish clear patterns of events”, that there was “fear on both sides”, and that there was no “specific intent to physically destroy the targeted group” (Suu Kyi, 2020b). The current head of state, Min Aung Hlaing, stated that “the government in office [took] great care in solving the problem” (UN Fact-Finding Mission, 2018, paragraph 753).

This paper develops several hypotheses about the causes and consequences of the conflict. First, in contrast to the government’s claim that conflict events are idiosyncratic, we theorize that resource expropriation motivated state violence against civilians, including the Rohingya. Second, in addition to economic motives, we argue that a specific religiously-motivated animus shaped the state’s prosecution of the Rohingya conflict, despite the state’s assertions to the contrary. Finally, we show that state-led violence was associated with substantially impaired mental health and economic opportunities among displaced Rohingya people.

We rigorously evaluate these hypotheses by applying statistical methods to representative data from existing and original sources. First, we show that expropriation motivated state violence. Our approach leverages price fluctuations for the local staple crop, rice. We find that state violence against ethnic minority civilians in Myanmar increased in areas most suitable

for rice cultivation when rice prices rose - precisely the times and places when the returns to resource appropriation were highest. We also find that state-perpetrated looting and property damage increased in those times and places. The same patterns hold when we analyze only events targeting Rohingya civilians or only events in Rakhine state, the state with the largest Rohingya population.

Second, we find direct evidence of expropriation from an original, representative survey of Rohingya refugee households in Cox's Bazar, Bangladesh, which is home to approximately 900,000 Rohingya refugees, representing over 84% of Rohingya displaced from Myanmar. We find losses of agricultural assets consistent with widespread looting: 79.1% of households lost agricultural land, 55.8% lost machinery, 60.1% lost draft animals, and 33.8% lost crop inventories.

Third, we test whether state violence displayed anti-Rohingya animus. We apply vector auto-regression (VAR) techniques analogous to those used by [Jaeger and Paserman \(2008\)](#) and [Haushofer et al. \(2010\)](#) on daily conflict data from Myanmar covering January 2010 - March 2020. We establish several novel facts, each consistent with Rohingya-specific animus.

(1) After bilateral clashes between state forces and the Rohingya militia, state forces responded more persistently and violently than the militia: in the thirty days after a clash, state forces responded with 6.02 unilateral attacks, whereas the Rohingya militia responded with only 0.58 cumulative attacks. This asymmetry is not observed in other ethnic conflicts. (2) State forces disproportionately targeted civilians in the Rohingya conflict, compared to other ethnic conflicts; in the thirty days after a clash, there were 59.49 excess civilian Rohingya deaths, whereas the analogous figures for other major ethnic conflicts were both statistically zero. (3) Anti-Rohingya demonstrations led by nationalist Buddhist groups systematically preceded state violence, indicating ideological influence. The same pattern was not present for other ethnic conflicts.

In the last section, we return to our large-sample, representative survey of Rohingya refugees to evaluate the human consequences of the violence. Our findings are consistent with the Gambia's ICJ suit and confirm that the primary and secondary accounts cited by plaintiffs are statistically representative. Nearly 85 percent of Rohingya refugees living in Cox's Bazar experienced at least one traumatic event.¹ Over half of respondents reported a near-death experience and over a third lost a family member or friend to murder; Myanmar's state forces

¹ We use the definition of trauma from the Harvard Trauma Questionnaire (HTQ), which includes torture, witnessing murder or death, and forced separation from family.

were most frequently named the perpetrators. Furthermore, over 28% of Rohingya refugees met the symptom criteria for depression, a high rate even compared to other forcibly displaced populations assessed with the same diagnostic tool. For example, [Georgiadou et al. \(2018\)](#) found a depression rate of 14.5% among Syrian refugees in Germany.

The central contribution of this paper is to provide a framework for generating representative, systematic, and generalizable evidence about violence against civilians, even when events are politically contested and take place in contexts with limited secondary data. Applying this framework to the Rohingya Crisis, we establish evidence of the Myanmar government’s systematic economic motives; its widespread appropriation of agricultural resources, its asymmetric responses to the Rohingya militia relative to other ethnic militias; its targeting of Rohingya civilians; and its ideological influences. This approach complements existing research by producing statistically precise conclusions about the conflict as a whole and revealing temporal patterns in violence difficult to detect via other tools.

2 Theory

2.1 Commodity Prices and Conflict

Our first empirical exercise links commodity prices to conflict. Prior work highlights two mechanisms by which these concepts are linked. On one hand, increases in the price of labor-intensive products, like agricultural goods, tend to pacify violence by increasing the opportunity cost of fighting ([Dal Bó and Dal Bó, 2011](#); [Dube and Vargas, 2013](#); [Besley and Persson, 2011](#); [Brückner and Ciccone, 2010](#), meta-analysis by [Blair et al., 2021](#)). On the other hand, increases in the price of capital-intensive commodities, such as oil, tend to increase violence by augmenting the spoils of conflict, a phenomenon called the “rapacity” channel ([Ross, 2004a](#); [Collier and Hoeffler, 2004](#); [Lujala et al., 2005](#); [Fearon, 2005](#); [Humphreys, 2005](#); [Le Billon, 2013](#); [Angrist and Kugler, 2008](#)). The net effect of commodity prices on conflict is therefore theoretically ambiguous.

[McGuirk and Burke \(2020\)](#) study these mechanisms in the context of food prices. The model predicts that in food-producing regions, rising returns to food expropriation are countered by rising opportunity costs of forgoing farm work. In contrast, in net-consuming regions, rising returns to expropriation are not muted by rising opportunity costs. In our context, we theorize that the state behaves like a net consumer, but one that can project power and initiate conflict

all over the country.

We blend these results with insights about power asymmetries from [Mitra and Ray \(2014\)](#). We theorize that the massive power imbalance between the state and comparatively weak ethnic forces shapes the relationship between commodity prices and ethnic persecution in Myanmar. In [Mitra and Ray \(2014\)](#)'s model, two unequally-resourced groups compete for resources. Increases in the weak group's resources place them at greater risk of violence by raising the stronger group's returns to expropriation.

Formally, we hypothesize:

Hypothesis 1 *State-led violence against civilians will be relatively more intense in rice-suitable regions during periods of high rice prices.*

Hypothesis 2 *Targets of state violence will report the seizure of agricultural assets.*

These hypotheses are also motivated by works on the strategic nature of violence against civilians ([Lichtenheld, 2020](#); [Steele, 2017](#); [Esteban et al., 2015](#), surveys by [Valentino, 2014](#) and [Balcells and Stanton, 2021](#)), which identify a diverse set of potential strategic motives, including the erosion of civilian opposition to the state ([Valentino et al., 2004](#); [Downes, 2017](#); [Fjelde and Hultman, 2014](#)). We propose an additional strategic motive for violence against civilians: the state's desire to acquire material resources. There are several reasons the state may be inclined to acquire material resources by force, like greater capacity or the ability to make transfers to favored constituents. Another could be forward-looking concerns about a group's capacity to oppose the government; the state may decide to attack an opposition group when that group is weak but growing since the group may have difficulty credibly committing to future peace ([Powell, 2006](#)).

2.2 Rohingya-Specific Animus

We propose that the violence inflicted upon Rohingya civilians was driven by group-specific animosity. This idea is supported by previous research and journalism, which has identified strong anti-Rohingya sentiment shaped by nationalist, ethno-centric, and religious ideology.

Myanmar has witnessed the pervasive use of hate speech concerning the nationality of Rohingya people. The term "Bengali" is commonly employed to challenge the Myanmar citizenship of the Rohingyas ([Rahman, 2019](#); [Stecklow, 2018](#)). In 2014, the government ruled that Rohingya

individuals could only participate in a national census if they identified as Bengali (Albert and Maizland, 2020). This terminology was also adopted by powerful leaders, including Min Aung Hlaing, current head of state, who stated that “the Bengali problem was a long-standing one” (UN Fact-Finding Mission, 2018, paragraph 753).

Religious themes have also shaped anti-Rohingya rhetoric, with nationalist Buddhist monks propagating anti-Muslim views directed at the Rohingya community (International Crisis Group, 2019; Wade, 2013). They “framed Muslims as posing both a personal threat and a threat to the Buddhist-majority nation” and used “images of ISIS brutality... to suggest all Muslims are potential terrorists” (Fink, 2018).

The state has denied that Rohingya-specific animus underlies the conflict. The former head of state, Aung San Suu Kyi, stated that there was “fear on both sides” and that there was no “specific intent to physically destroy the targeted group” (Suu Kyi, 2020b). She also stated that state violence was intended to address “insurgents,” rather than target a group identity (Simons and Beech, 2021).

We evaluate these claims in the data. Conceptually, we test whether the state used violence asymmetrically against Rohingya people and whether the state used violence more against the Rohingya than other ethnic groups. Formally,

Hypothesis 3 *Under power asymmetries and targeted animus against the Rohingya, observed state-initiated conflict events would be more numerous and deadly than Rohingya-militia-initiated conflict events after a bilateral clash between state forces and a Rohingya militia.*

Hypothesis 4 *If targeted ethnic animus against the Rohingya is present, then the state will initiate more and deadlier conflict events after a bilateral clash with Rohingya militias compared to their clashes with other ethnic militias.*

Additionally, we test whether anti-Rohingya violence was shaped by known proponents of animus: nationalist Buddhist monks. Observers have noted that these religious elites have espoused demeaning rhetoric and coordinated protests against the Rohingya (Fink, 2018; Wade, 2013; Lee, 2021; Maung, 2023; C4ADS, 2016).

Hypothesis 5 *Anti-Muslim protests organized by religious elites systematically precede state-led violence against Rohingya civilians, but not against other minority ethnic groups.*

This would test a potential link between the most visible source of anti-Rohingya animus and state violence. This test relates to previous scholarship, which finds elites can drive violence

against civilians in diverse settings (Brass, 1997; Snyder, 2000). Coordination between religious elites and the state could take various forms. Coordination could be explicit, e.g. with the state inviting anti-Muslim protests to build popular support for planned violence against the Rohingya. Alternatively, it could be implicit, e.g. with religious leaders using protests to pressure the state into executing a policy of violence against the Rohingya.

2.3 Economic and Mental Health Consequences of Displacement

This section of our analysis focuses on two domains of the human costs of violence and displacement. First, we theorize that displaced Rohingya people will have lost productive assets and have reduced access to labor markets, lowering wealth and earnings.

Hypothesis 6 *Displaced Rohingya people report losses of labor income and wealth.*

Hypothesis 7 *Displaced Rohingya people experience a decline in employment and earnings.*

Second, if the narrative-based evidence presented at the ICJ by Gambia is representative, then we hypothesize that our large sample data will reveal a substantial mental health burden systematically linked to violence and displacement.

Hypothesis 8 *Displaced Rohingya people report high levels of exposure to traumatic events and high rates of depression.*

The rest of the paper is organized as follows. Section 3 describes the background and setting of the study. Section 4 describes the causal links between price shocks and conflict. Section 5 provides direct evidence of resource appropriation. Section 6 characterizes the VAR evidence. Section 7.2 provides evidence of the human costs on displaced people. Section 8 concludes.

3 Background

Ethnic conflicts and persecution have been present within Myanmar since its founding in 1948, fueled by minority groups' desire for autonomy and democratization and the state's opposition to those goals (South, 2008). For decades, Myanmar's armed forces, the Tatmadaw, have clashed with diverse ethnic groups, including the Rohingya, Kachin, Rakhine, Karen, Palaung, and Shan (Koning, 2019; SHRF and SWAN, 2002; ACLED, 2019).

The Rohingya ethnic minority has faced particular disenfranchisement and marginalization. In 1962, the state stripped Rohingya people of Myanmar citizenship (Albert and Maizland, 2020). Moreover, the Rohingya face cultural narratives of perpetual foreignness and are disproportionately targeted by hate speech (Stecklow, 2018). Rohingya, who are historically Muslim, also face severe discrimination based on religion, frequently led by nationalist Buddhist groups (McPherson, 2017).

In late August 2017, the state’s persecution of the Rohingya escalated dramatically. The Arakan Rohingya Salvation Army (ARSA), an armed group claiming to represent the Rohingya, attacked thirty police posts and an army base. More than seventy people were killed during the initial attacks, including twelve members of Myanmar’s security forces (Ramzy, 2017). In response, the military attacked Rohingya civilians, displacing over seven hundred thousand people (Human Rights Watch, 2017). The vast majority fled to neighboring Bangladesh, which currently hosts nearly one million refugees in camps in Cox’s Bazar. Rohingya refugees described indiscriminate killing, mass sexual violence, and arson by state forces (Human Rights Council, 2018).

Due to this episode of violence, over 70,000 acres of agricultural land formerly farmed by displaced Rohingya were harvested by the government (Phyo, 2022, 2023; Lewis et al., 2017; AFP, 2017). The harvested rice was worth approximately \$5.3 million USD (Global New Light of Myanmar, 2017), and the harvest took place with assistance from the government and corporations (Myanmar News Agency, 2017; MAPCO, 2017; Eleven Media Group, 2017). Proceeds from the appropriated rice were “deposited in a bank account as part of the national budget” (Global New Light of Myanmar, 2017), and appropriated land was settled by non-Rohingya civilians (France-Presse”, 2018; McPherson, 2020; Myint, 2017; Frontier Myanmar, 2018). Moreover, Rohingya property rights to the land were effectively severed (Lewis et al., 2017) and the military claimed ownership of the lost land (Aung, 2022). In short, rice and rice-producing assets of the displaced Rohingya were appropriated directly by the state and majority groups.

4 Resource Expropriation as Motive: Commodity Prices

In this section, we test whether resource expropriation motivated violence against civilians across Myanmar and against the Rohingya specifically. To do so, we devise an identification strategy that isolates externally-generated increases in the returns to resource appropriation.

We combine temporal variation in international commodity prices with regional variation in rice-growing suitability in Myanmar.

Rice is the most important staple crop in Myanmar, constituting approximately sixty percent of the country’s total cultivated area and about one-third of agricultural value produced (Young et al., 1998). Wetland cultivation is substantially more common than dry land cultivation and accounts for 80% of all land used for rice planting (United States Foreign Agriculture Service, Rangoon, 2020). Wetland rice requires particular topographical and climatological conditions, and suitability varies within Myanmar. We discuss and depict this variation in Section 4.1.

About one-tenth of Myanmar’s rice crop is exported. By value, rice represented 4.3% of Myanmar’s total exports in 2019 (Comtrade, 2020). Domestic market integration, as measured by regional price co-movement, is fairly high and facilitated by intermediaries like collectors and wholesalers operating across regions (Lwin, 2005).²

4.1 Data

We construct township-level wet rice suitability using the FAO’s *Global Agro-Ecological Zones* v.3.0 (GAEZ) database (FAO, 2012b). There are 330 townships in Myanmar, each with an average of 163,000 residents. The suitability dataset combines climate, soil, and terrain data to assess local suitability for 49 different crops at a 5-arc-minute resolution for the entire world. For our main analysis, we use GAEZ’s wetland rice suitability index³, which varies from 0 to 100, with 0 representing regions least suitable for wetland rice cultivation and values of 100 representing regions ideal for wetland rice cultivation. The distribution of the rice suitability variable is shown visually in Figure 1. Sub-Figure 1a plots the suitability index at the finest available resolution for all of Myanmar, while Sub-Figure 1b plots the suitability index for Rakhine State only.

We obtain domestic and international rice prices from the FAO’s *Food Price Monitoring and Analysis Tool* (FPMA) (FAO, 2012a). We download international rice prices for the years 2010-

² We also explored the extent of rice cultivation by ethnic group to understand whether the Rohingya might reasonably be targeted as rice prices rise. While there is, to our knowledge, no microdata from Myanmar that contains data on both agriculture and ethnicity, we are able to glean information from journalistic sources. The Rohingya population is mostly concentrated in the three northern townships: Maungdaw, Buthidaung and Rathedaung, where Rohingya comprise about 80% of the population (International, 2004). In these towns, an overwhelming majority of paddy is Rohingya-cultivated. For example, in Maungdaw, at least 70,000 out of 74,000 acres were cultivated by Rohingya farmers (Min Aung Khine, 2018).

³ Specifically, we use wetland rice suitability for low level inputs, rain-fed irrigation, and average climatic conditions for the period 1961-1990

2020, which represent 13 different commodity sub-types.⁴ We also obtain data on domestic rice prices in Myanmar for three different strains of rice.⁵ We aggregate these 16 different time series into two price indices, international and domestic. To do so, we average over the 13 international price series and the 3 domestic price series, after normalizing each series by subtracting its own mean and dividing by its own standard deviation. The international and domestic price series are plotted in Figure 2.

We obtain conflict data for January 2010 - March 2020 from the Armed Conflict Location and Event Data Project (ACLED) (Raleigh et al., 2010), which encodes conflict events, as well as their dates, participants, fatalities, and locations. Events include battles, remote violence (e.g., land mines), attacks, protests, riots, and strategic developments, such as peace talks or ceasefire agreements. ACLED researchers source information from secondary sources, primarily local and national newspapers. We describe the data in greater detail in the Online Appendix section A. In this section, we focus on events of violence against civilians, violence against civilians perpetrated by state forces, as well as property crimes, which include both looting and property damage. Though it would be ideal to measure looting alone, the data do not permit this disaggregation. The beginning of our study period is determined by ACLED’s coverage of Myanmar, while the ending corresponds to the month of the first COVID case in Myanmar, after which we have reason to believe food price and conflict dynamics may no longer reflect patterns during other times (Gregorioa and Ancog, 2020; Arouna et al., 2020; Laborde et al., 2020). Table 1 presents summary statistics for key variables within our baseline sample.

4.2 Empirical Strategy

Conceptually, we test whether increases in rice prices differentially increased violence in townships that are more suitable for rice cultivation. This relationship is theoretically ambiguous. On one hand, increases in rice prices could increase conflict through a variety of channels. First, it could become more attractive to loot, therefore increasing conflict via a "rapacity" channel. Second, increased food prices could drive individuals to violence via a "desperation" channel.

⁴ India Rice (25% broken), Pakistan Rice (25% broken), Thailand Rice (25% broken), Vietnam Rice (25% broken), Thailand Rice (5% broken), Vietnam Rice (5% broken), Uruguay Rice (5% broken, long grain), Pakistan Rice (Basmati Ordinary), Thailand Rice (Fragrant 100%), Thailand Rice (Parboiled 100%), Thailand Rice (Thai 100% B), Thailand Rice (Thai A1 Super), US Rice (US Long Grain 2.4). We omit glutinous rice because it is an imperfect substitute for standard rice, and we omit U.S. medium-grain rice because less than 26% of rice cultivated in Myanmar is medium-grain (United States Foreign Agriculture Service, Rangoon, 2020).

⁵ The domestic prices are for Emata EHYV-FQ, Emata Manawthukha-FQ, and Emata Medium.

Figure 1: Wetland Rice Suitability

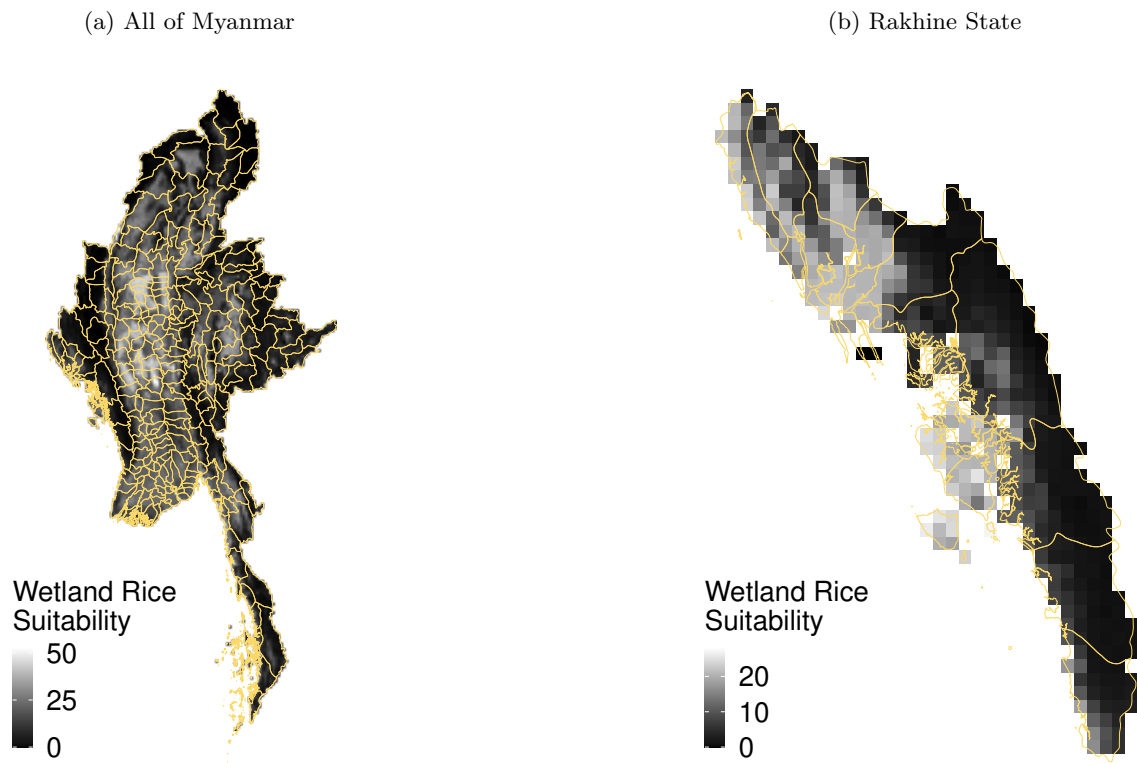


Figure 2: International and Myanmar Domestic Rice Prices Over Time

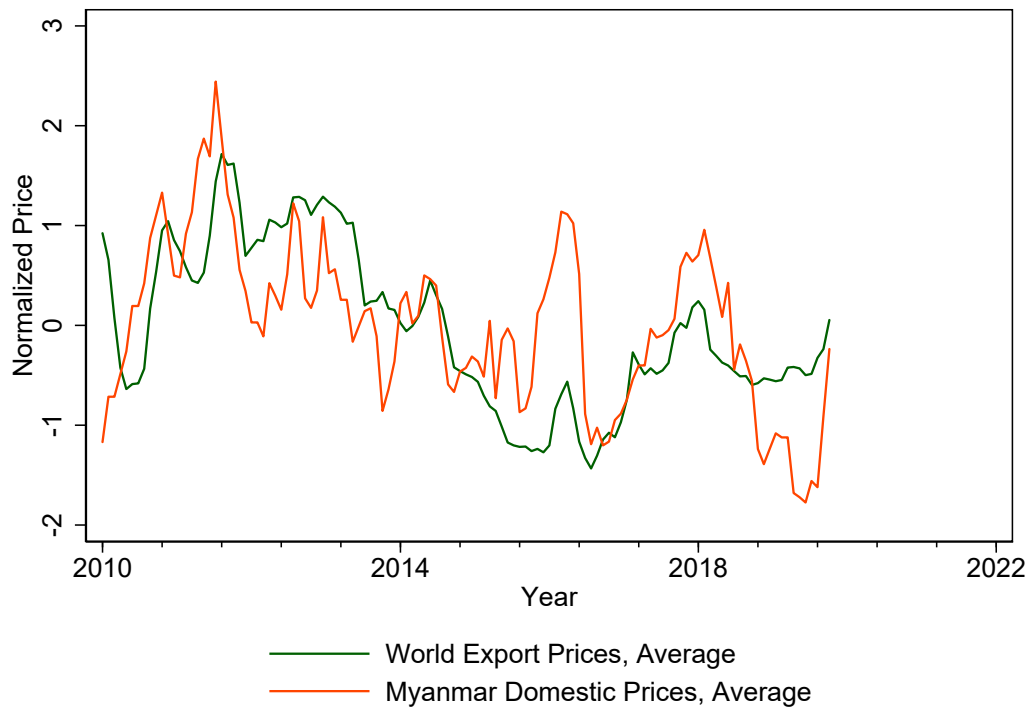


Table 1: Summary Statistics

	(1)	(2)	(3)
	N	Mean	Std. Dev.
Violence against civilians	34,243	0.033	0.41
Violence against civilians by state forces	34,243	0.024	0.35
Looting/property damage	34,243	0.005	0.098
Violence against Rohingya civilians	34,243	0.005	0.27
Wetland rice crop suitability index	34,243	16.6	8.9
L. Myanmar domestic rice price	33,677	0.006	0.832
L. Rice export price, world average	33,677	-0.049	0.801
Latitude (Y)	34,243	19.93	3.1
Longitude (X)	34,243	95.7	1.6
Township population	34,243	172178.5	157175.4
Indicator for onshore oil/gas deposit	34,243	0.293	0.455
Indicator for mineral resources	34,243	0.106	0.308
Indicator for being on coast	34,243	0.163	0.369

Notes: Observations are at the township and month-year level and cover all of Myanmar.

Finally, higher rice prices could increase government tax revenues or the relative resources of majority groups, driving more conflict through a "capacity" channel. On the other hand, rice is a labor-intensive crop, so price increases should increase the opportunity cost of conflict for participants employed in agriculture.

Our analysis constitutes a joint test of i) whether economic forces underlie civilian persecution in Myanmar and if they do, ii) whether the pacifying or conflict-increasing mechanisms dominate. In equation 1, the coefficient of interest is β . If $\beta = 0$, we fail to reject the null hypothesis that economic forces do not drive ethnic violence in Myanmar. If $\beta < 0$, we reject the null and find that the opportunity cost mechanism dominates. If $\beta > 0$, we reject the null and find that the conflict-increasing mechanisms dominate.

Our main estimating equation is:

$$y_{i,t} = \beta \text{Suitability}_i \times \text{Price}_{t-1}^{MMR} + \tau_t + \alpha_i + \mathbf{X}_i \times \tau_t + \varepsilon_{i,t}, \quad (1)$$

where i indexes townships and t indexes month-years, like January 2010. Our analysis covers the time frame January 2010 - March 2020. The specification includes township fixed effects α_i and month-year fixed effects τ_t . The township fixed effects capture time-invariant differences across townships, such as persistent differences in geography, ethnic diversity, income, or local institutions. Month-year fixed effects control for national trends that affect all townships

similarly. The vector \mathbf{X}_i , which we interact with month-year fixed effects, includes three time-invariant township characteristics: categorical variables for integer latitude and longitude and an indicator variable for whether a township is on the coast. All standard errors are clustered at the township level to allow for serial correlation over time and within townships.

We use three township-year conflict measures $y_{i,t}$: a binary indicator for violence against civilians, another for violence against civilians perpetrated by state forces (defined as armed forces or police), and a third for looting/property damage. These outcome variables are assumed to be a function of the interaction of a time-invariant measure of wetland rice suitability, $Suitability_i$, and the one-year lag of domestic price of rice in Myanmar, $Price_{t-1}^{MMR}$. Our hypothesis of interest is whether $\beta > 0$: in months with high rice prices, townships that are more suitable for rice cultivation are more likely to experience conflict. The uninteracted suitability variable is absorbed by township fixed effects, while the uninteracted price variable is absorbed by month-year fixed effects.

The treatment of interest β represents the difference in conflict intensity across years with high rice prices versus low rice prices, for townships with high rice suitability compared to townships with low rice suitability. The parallel trend assumption is that high- and low-suitability townships would have experienced the same changes in conflict due to rice prices, had their suitability levels been the same.

The main challenge for identification of the coefficient of interest, β , is that $Suitability_i \times Price_{t-1}^{MMR}$ is potentially correlated with other factors that could co-determine conflict, or conflict itself. For example, Myanmar domestic rice prices could increase when conflict occurs in rice-producing regions because that violence directly reduces supply. To address these issues, we instrument $Price_{t-1}^{MMR}$ in our baseline specification with $Price_{t-1}^{INTL}$, international rice prices. Because Myanmar exports about 9% of its total rice output and represents under 5% of global supply, we assume that Myanmar is a global price taker, such that international rice prices are exogenous to events in Myanmar that may co-vary with local conflict.

Though using international rice prices addresses some omitted variables, there are nonetheless limitations to this approach. For example, rice prices may be correlated with international oil or gas prices, and the locations of oil reserves in Myanmar may be correlated with areas suitable for wetland rice cultivation. We address this concern and others via robustness checks in Section 4.4.

4.3 Results

Panel A of Table 2 presents the baseline estimates of equation (1). Each column represents a separate township-month regression. The dependent variables are (1) an indicator for whether any incident of violence against civilians; (2) an indicator for violence against civilians perpetrated by state forces; and (3) an indicator for looting or property destruction. These regressions use international rice prices as an instrument for Myanmar domestic prices.

The first row reports the coefficient of interest, β . We find that across all three measures of conflict, the coefficient of interest is positive and statistically significant at the $p < 0.05$ level or lower. Taken together, these results document that higher domestic rice prices increase the likelihood of civilian persecution in more rice-suitable townships of Myanmar. State forces clearly perpetrate some of this violence, and the violence is accompanied by looting and property crimes, indicating economic motives for violence. Our instrument is strong, with a Cragg-Donald Wald F statistic of 13,478 and a Kleibergen-Papp Wald F statistic of $5.4 \times E^{17}$.

We interpret these results by computing how outcomes would change in a township with average rice suitability when rice prices increase from their 25th to 75th percentile value. We find that violence against civilians would increase by 0.141 events (on a mean of 0.02), violence against civilians by the state would increase by 0.084 events (on a mean of 0.03), and looting would increase by 0.096 events (on a mean of 0.005).

Table 3, reports the baseline's corresponding OLS and reduced form regressions. In Panel A, we directly estimate equation (1) without instrumenting for Myanmar domestic rice prices. For all three outcomes, domestic rice prices increase the propensity for conflict in more rice-suitable regions. In Panel B, we report export prices instead of Myanmar domestic prices. The main results emerge in each specification.

4.3.1 The Rohingya and Rakhine State

In this subsection, we examine whether resource expropriation motivates violence against the Rohingya ethnic group specifically. To do so, we perform several tests. In Table 4, we re-estimate equation (1) with two new outcome variables. For incidents of violence against civilians, ACLED contains information on the perpetrating party. We can therefore construct two additional Rohingya-specific outcome variables: one that indicates whether any violence against Rohingya civilians occurred, and one that indicates whether any Rohingya-affiliated militia engaged in

Table 2: The Effect of Rice Price and Suitability on Conflict in Myanmar

Dependent Variable:	(1) Violence Against	(2) Violence Against Civilians - State	(3) Looting & Property
L. Myanmar Domestic Rice Price	0.000656** (0.000304)	0.000390** (0.000196)	0.000451*** (0.000168)
Observations	34,243	34,243	34,243
Kleibergen-Paap rk Wald F	5.E+17	5.E+17	5.E+17
Cragg-Donald Wald F	13,478	13,478	13,478
<i>Sample Y Mean</i>	<i>0.033</i>	<i>0.024</i>	<i>0.005</i>
<i>Total Townships</i>	<i>286</i>	<i>286</i>	<i>286</i>
<i>Sample Townships</i>	<i>283</i>	<i>283</i>	<i>283</i>
<i>Total Month-years</i>	<i>123</i>	<i>123</i>	<i>123</i>
<i>Sample Month-years</i>	<i>121</i>	<i>121</i>	<i>121</i>

Notes: Observations are at the township-month-year level. Outcome variables are binary measures of whether an incident type occurred. Myanmar average domestic rice price is instrumented with world average rice export price, and both series are measured at the month-year level. Wet rice suitability is measured at the township level. All regressions control for township fixed effects and month-year fixed effects, an indicator for coastal townships interacted with month-year fixed effects, integer latitude interacted with month-year fixed effects, and integer longitude interacted with month-year fixed effects. Standard errors are clustered at the township level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

violence against civilians. We find that violence targeting Rohingya civilians increases when the rice shock is large, with $p < 0.1$. In contrast, violence against civilians perpetrated by Rohingya groups does not respond.

We also use a complementary approach to study Rohingya persecution specifically; we test whether the result is more powerful in Rakhine state, the home of nearly all Rohingya in Myanmar and the location of the 2017 wave of violence (IOM, 2021).

Table 5 reports three separate regressions in columns. Each column's two coefficients correspond to the rice shock interacted with an indicator for Rakhine state and the shock interacted with an indicator for the rest of Myanmar. Column (1) reveals that violence against civilians increases in response to the rice price and suitability interaction in Rakhine state, but not in the rest of Myanmar. The Rakhine point estimate and the difference between the two coefficients are both precise at the $p < 0.01$ level. Column (2) corroborates this pattern for violence against civilians perpetrated by state forces: both the point estimate for the interaction with Rakhine state and the difference between the two coefficients is again precise at the $p < 0.01$ level. Turn-

Table 3: The Effect of Rice Price and Suitability on Conflict: OLS and Reduced Form

Dependent Variable:	(1) Violence Against Civilians	(2) Violence Against Civilians - State Forces	(3) Looting & Property Damage
Panel A: OLS			
L. Myanmar Domestic Rice Price	0.000683** (0.000275)	0.000514** (0.000200)	0.000234*** (8.58e-05)
Observations	34,243	34,243	34,243
R-squared	0.272	0.245	0.210
Panel B: Reduced Form			
L. Rice Export Price, World Avg.	0.000370** (0.000173)	0.000219** (0.000109)	0.000253*** (9.42e-05)
Observations	34,526	34,526	34,526
R-squared	0.271	0.244	0.210

Notes: Observations are at the township-month-year level. Outcome variables are binary measures of whether an incident type occurred. Domestic and international rice prices are measured at the month-year level. Wet rice suitability is measured at the township level. All regressions control for township fixed effects and month-year fixed effects, an indicator for coastal townships interacted with month-year fixed effects, integer latitude interacted with month-year fixed effects, and integer longitude interacted with month-year fixed effects. Standard errors are clustered at the township level. *** p<0.01, ** p<0.05, * p<0.1

Table 4: The Effect of Rice Price and Suitability on Conflict: Rohingya Events

Dependent Variable:	(1)	(2)
	Violence Against Civilians	
	Rohingya Target	Rohingya Perpetrator
L. Myanmar Domestic Rice Price	9.78e-05* (5.19e-05)	3.61e-06 (2.00e-05)
Observations	34,243	34,243
<i>Y Mean</i>	<i>0.000</i>	<i>0.000</i>
<i>Total Townships</i>	<i>286</i>	<i>286</i>
<i>Sample Townships</i>	<i>283</i>	<i>283</i>
<i>Total Month-years</i>	<i>123</i>	<i>123</i>
<i>Sample Month-years</i>	<i>121</i>	<i>121</i>

Notes: Observations are at the township-month-year level. The sample covers all of Myanmar. Outcome variables are binary measures of whether an incident type occurred. Myanmar average domestic rice price is instrumented with world average rice export price, and both series are measured at the month-year level. Wet rice suitability is measured at the township level. All regressions control for township fixed effects and month-year fixed effects, an indicator for coastal townships interacted with month-year fixed effects, integer latitude interacted with month-year fixed effects, and integer longitude interacted with month-year fixed effects. Standard errors are clustered at the township level. *** p<0.01, ** p<0.05, * p<0.1

Table 5: The Effect of Rice Price and Suitability on Conflict: Rakhine State

Dependent Variable:	(1) Violence Against Civilians	(2) Viol. Against Civilians State Forces	(3) Looting & Property Damage
L. Myanmar Domestic Rice Price × Wet Rice Suitability × Rakhine State	0.00645*** (0.00168)	0.00504*** (0.00140)	0.00277** (0.00123)
L. Myanmar Domestic Rice Price × Wet Rice Suitability × Other States	0.000520 (0.000341)	0.000275 (0.000211)	0.000519** (0.000210)
Observations	34,001	34,001	34,001
<i>Y Mean, Rakhine States</i>	<i>0.0900</i>	<i>0.0700</i>	<i>0.0200</i>
<i>Y Mean, Other States</i>	<i>0.0200</i>	<i>0.0100</i>	<i>0.000</i>
Test p-value: Rakhine State = Other States	0.0006	0.0009	0.0726

Notes: Observations are at the township-month-year level. Outcome variables are binary measures of whether an incident type occurred. Myanmar average domestic rice price is instrumented with world average rice export price, and both series are measured at the month-year level. Wet rice suitability is measured at the township level. All regressions control for township-Rakhine fixed effects and month-year-Rakhine fixed effects, an indicator for coastal townships interacted with month-year fixed effects, integer latitude interacted with month-year fixed effects, and integer longitude interacted with month-year fixed effects. Standard errors are clustered at the township level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

ing to column (3), looting and property damage, we find that both terms are precisely different from zero, but they are still statistically different at the $p < 0.01$. Overall, this table provides striking evidence that economic factors drive persecution in Rakhine state substantially, and more than in the rest of Myanmar.

As we did for the nationwide results, we compute the extent our outcomes would change in a township with average rice suitability when rice prices rise from their 25th to 75th percentile value. We find that Rakhine violence against civilians would increase by 11.3%, violence against civilians by the state would increase by 8.8%, and looting would increase by 4.9%. Importantly, these values are an order of magnitude larger than the nationwide ones, suggesting that persecution in Rakhine state is particularly responsive to economic incentives.

Taken together, the results in Tables 4 and 5 strongly suggest that economic interests motivate Rohingya persecution in Myanmar, contradicting the government’s claims that the violence was idiosyncratic.

4.4 Robustness

We now turn to tests of the sensitivity of our baseline estimates. As we discussed above, one challenge is that rice suitability may be correlated with township-level characteristics that

change the propensity of conflict in a manner correlated with rice prices. While we have ruled out a number of potential concerns in the baseline specification by including fixed effects and a vector of controls, there may still be omitted variables capable of generating a spurious result. To further examine the importance of this possibility, we check the sensitivity of our estimates to the inclusion of additional control variables.

The locations of natural resources could be potential omitted variables, and related work (Ross, 2004b; Collier and Hoeffler, 2004; Fearon, 2005; Humphreys, 2005; Le Billon, 2013) has documented how natural resource shocks can spark local conflicts. If rice suitability is correlated with the location of oil deposits or mineral resources, the international price of those resources, when correlated with the international price of rice, could generate potentially spurious results. Of course, this set of omitted variables would not undermine our broader point that microeconomic forces are a driver of conflict in this region and time period, but it would threaten the validity and interpretation of our particular results. We, therefore, construct two indicator variables: one for the existence of an oil reserve within a township (Oslo (PRIO), 2020), and one for the existence of any mining resources within a township (Schulz and Joseph, 2005).

In Panel A of Table 6, we interact these indicators with month-year fixed effects and include them as controls, which allow oil- and mineral-endowed locations to exhibit different conflict propensities for each month in our sample. The propensity for all three conflict types still increases in the face of the rice shock, and the coefficients are extremely similar in magnitude to those in 2.

To understand whether our results are sensitive to the timing of the price measure, we re-run the results in Table 2 with three alternative timing choices: contemporaneous, a two-month lag, and a three-month lag. We report these results in Panels B, C, and D of Table 6, with each panel reporting the coefficient of interest for each lag period. We find that for the contemporaneous and two-month lag specifications, the original results hold precisely and with consistent magnitudes. For a lag of three months, the coefficients remain positive but decline slightly in magnitude and most outcomes lose precision. This pattern indicates the effect decays over time.

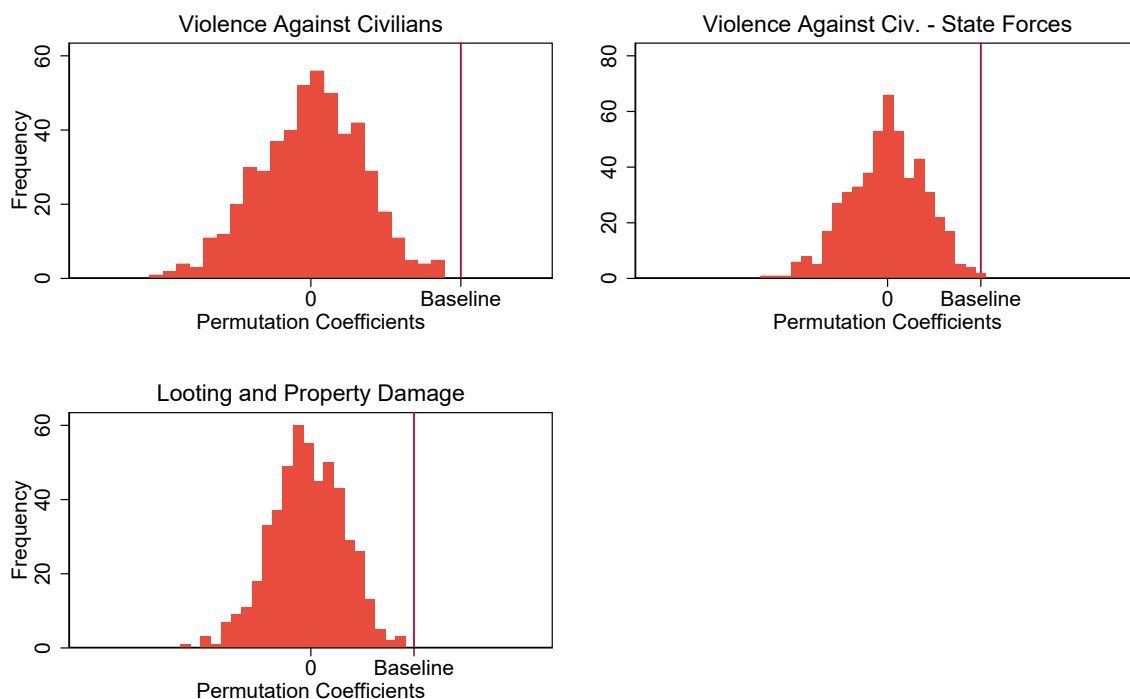
Because rice prices are fairly volatile, we re-run equation (1) with a three-month moving average measure of the rice prices over months $t-1$, t , and $t+1$. Panel E of Table 6 demonstrates that our main results are stable: all the coefficients are positive, of similar magnitude, and of

Table 6: The Effect of Rice Price and Suitability on Conflict: Robustness

Dependent Variable:	(1) Violence Against Civilians	(2) Viol. Against Civilians State Forces	(3) Looting & Property Damage
Panel A: Control for Natural Resources			
L. Myanmar Domestic Rice Price * Wet Rice Suitability	0.000606* (0.000321)	0.000326* (0.000192)	0.000446** (0.000174)
Observations	34,243	34,243	34,243
Panel B: Contemporaneous Price			
Myanmar Domestic Rice Price * Wet Rice Suitability	0.000678** (0.000317)	0.000379* (0.000206)	0.000443*** (0.000166)
Observations	34,526	34,526	34,526
Panel C: Two-Month Lagged Price			
L2. Myanmar Domestic Rice Price * Wet Rice Suitability	0.000556* (0.000286)	0.000343** (0.000174)	0.000410** (0.000162)
Observations	33,960	33,960	33,960
Panel D: Three-Month Lagged Price			
L3. Myanmar Domestic Rice Price * Wet Rice Suitability	0.000419 (0.000271)	0.000250 (0.000164)	0.000432*** (0.000158)
Observations	33,677	33,677	33,677
Panel E: Three-Month Moving Average of Price			
3mma Domestic Rice Price * Wet Rice Suitability	0.000603** (0.000303)	0.000343* (0.000194)	0.000443** (0.000173)
Observations	33,394	33,394	33,394

Notes: Observations are at the township-month-year level. Outcome variables are binary measures of whether an incident type occurred. Myanmar average domestic rice price is instrumented with world average rice export price, and both series are measured at the month-year level. Wet rice suitability is measured at the township level. All regressions control for township fixed effects and month-year fixed effects, an indicator for coastal townships interacted with month-year fixed effects, integer latitude interacted with month-year fixed effects, integer longitude interacted with month-year fixed effects, a dummy for onshore oil reserves interacted with month-year fixed effects, and a dummy for mineral resources interacted with month-year fixed effects. Standard errors are clustered at the township level. *** p<0.01, ** p<0.05, * p<0.1

Figure 3: Random Permutation Test



Imputed p-values: 0.000, 0.001, 0.000. Iterations: 500. Binary outcome variables.

equal or greater statistical precision.

Finally, we implement a placebo test to check whether the distribution of rice suitability across townships could generate spurious results. To do so, we perform a random permutation test by creating counterfactual rice suitability maps for Myanmar. For each township, we assign a new suitability level randomly drawn from the observed distribution and use this value to estimate a counterfactual baseline coefficient. We perform 500 iterations for each outcome variable and plot the counterfactual coefficients in histograms in Figure 3. The proportion of counterfactual values lying above the baseline estimate yields an implicit p-value: the values are all < 0.01 .

5 Resource Expropriation as Motive: Lost Assets

In this section, we find further evidence that systematic economic motives drove the state's Rohingya persecution. We find that a large majority of Rohingya refugees were deprived of agricultural capital (land, livestock, and machinery). We also find direct evidence of crop

Table 7: Change in Agricultural Asset Holdings

	(1)	(2)	(3)	(4)
	Draft Animal	Unpowered Ag. Equipment	Ag. Land	Crop Inventory
Never Owned	0.399	0.441	0.122	0.662
Lost	0.601	0.558	0.791	0.338
Gained	0.000	0.001	0.087	0.000

Notes: Shares reported for the refugee population in the CBPS. Assets lost and gained computed relative to July 2017. ‘Lost’ includes permanently lost property and property for which respondents still consider themselves the true owners.

appropriation.

We rely on a novel representative survey of Rohingya refugees fleeing from Myanmar to Cox’s Bazar, Bangladesh: the Cox’s Bazar Panel Survey (Baird et al., 2021). The survey also included a representative sample of the local host population. Together, the refugee and host samples covered 5,020 households and 9,393 individuals, divided almost equally between refugee camps (2,493 households) and host communities (2,527 households) across Cox’s Bazar. Online Appendix D describes the data in detail.

Surveys covered a range of topics at the household and individual levels, including assets, income, employment, demographics, trauma exposure, and mental health. Among the 4,616 Rohingya refugees surveyed, 3,914 left Myanmar during or after 2017. Survey response rates were high, with 0.2 percent non-response for the household questionnaire and 3.8 percent non-response for the adult questionnaire (The World Bank Group, 2019).

5.1 Agricultural Assets and Inventories

In Table 7, we report how displacement changed agricultural asset holdings among the refugee population displaced in August 2017. We find that 60.1% of refugees lost draft animals, 55.8% of refugees lost unpowered agricultural equipment, and 79.1% of refugees lost agricultural land. Within these categories, 0.0%, 0.0%, and 52.4% still reported themselves as the true owners of the lost property, though they had no enforceable rights (Lewis et al., 2017). We also found direct evidence of crop appropriation, with 33.8% of refugees reporting lost crop inventory.

These data show that Rohingya refugees were largely farmers before displacement and were substantially deprived of agriculture-related assets. These facts strongly support the theory that state violence was motivated systematically by appropriation.

6 Anti-Rohingya Animus: Vector Auto-Regressions

In this section, we test for evidence of Rohingya-specific animus in the state’s use of violence. We use a vector auto-regression (VAR) framework analogous to those in [Jaeger and Paserman \(2008\)](#) and [Haushofer et al. \(2010\)](#). We find that, in the Rohingya conflict, state forces used asymmetrically more violence than militias and that state violence targeted civilians. Neither pattern was present in other major ethnic conflicts in Myanmar. We also find that anti-Rohingya demonstrations by religious elites (Buddhist monks) were associated with anti-Rohingya violence, indicating that ideology shaped the violence.

We use daily conflict event data from ACLED, which we introduce in Subsection 4.1 and detail in Online Appendix Subsection A. In these data, we identify all events from ethnic conflicts between Myanmar’s government and militias that represent the Rohingya, Kachin, and Rakhine ethnic groups. Our data cover January 1, 2010 through May 30, 2020. We describe the coding of ethnic militias in Online Appendix Subsection A.

For each ethnic conflict, we construct six time series. They capture 1) the number of bilateral events between state forces and the ethnic militia, 2) the number of unilateral state force events, 3) the number of unilateral ethnic militia events, 4) fatalities generated by events from series 1, 5) fatalities generated by events from series 2, and 6) fatalities generated by events from series 3. Together, series 1, 2, and 3 comprise the universe of ACLED events that mention the relevant ethnic group and series 4, 5, and 6 comprise the universe of fatalities associated with those events.

An example of a series 1 event is a clash between the Rohingya militia and Myanmar’s army in Pyaungpit village on October 11, 2016.⁶ An example of a series 2 event is when the Myanmar army burned portions of Laung Don village in Maungdaw township on September 15, 2017.⁷ An example of a series 3 event is when the Rohingya militia killed two individuals suspected of being state informants on September 30, 2016.⁸

⁶ ACLED event code MMR3928.

⁷ ACLED event code MMR4734.

⁸ ACLED event code MMR3902.

6.1 Empirical Impulse Response Functions

The goal of this exercise is to identify how each conflict time series responds to a particular precipitating event, or “impulse”. In our case, we treat a bilateral event between state forces and an ethnic militia as the precipitating event. To test whether this impulse is associated with further events or fatalities in the next 30 days, we use reaction functions of the form:

$$\begin{pmatrix} \textit{Bilateral Events}_t \\ \textit{Militia Events}_t \\ \textit{State Events}_t \\ \textit{Bilateral Event Fatalities}_t \\ \textit{Militia Event Fatalities}_t \\ \textit{State Event Fatalities}_t \end{pmatrix} = \mathbf{A}_1 \begin{pmatrix} \textit{Bilateral Events}_{t-1} \\ \textit{Militia Events}_{t-1} \\ \textit{State Events}_{t-1} \\ \textit{Bilateral Event Fatalities}_{t-1} \\ \textit{Militia Event Fatalities}_{t-1} \\ \textit{State Event Fatalities}_{t-1} \end{pmatrix} + \dots + \mathbf{A}_k \begin{pmatrix} \textit{Bilateral Events}_{t-k} \\ \textit{Militia Events}_{t-k} \\ \textit{State Events}_{t-k} \\ \textit{Bilateral Event Fatalities}_{t-k} \\ \textit{Militia Event Fatalities}_{t-k} \\ \textit{State Event Fatalities}_{t-k} \end{pmatrix} + \mathbf{B}\mathbf{X} + \mathbf{e}_t \quad (2)$$

We estimate the VAR and compute orthogonal impulse response functions (IRFs) that represent the excess units of series j observed k days after one additional bilateral conflict event. We use a Cholesky decomposition to orthogonalize disturbances in our time series, which requires an assumption of the sequence of these variables. In our baseline analysis, we use the order presented in equation (2). The results are robust to alternative orders.

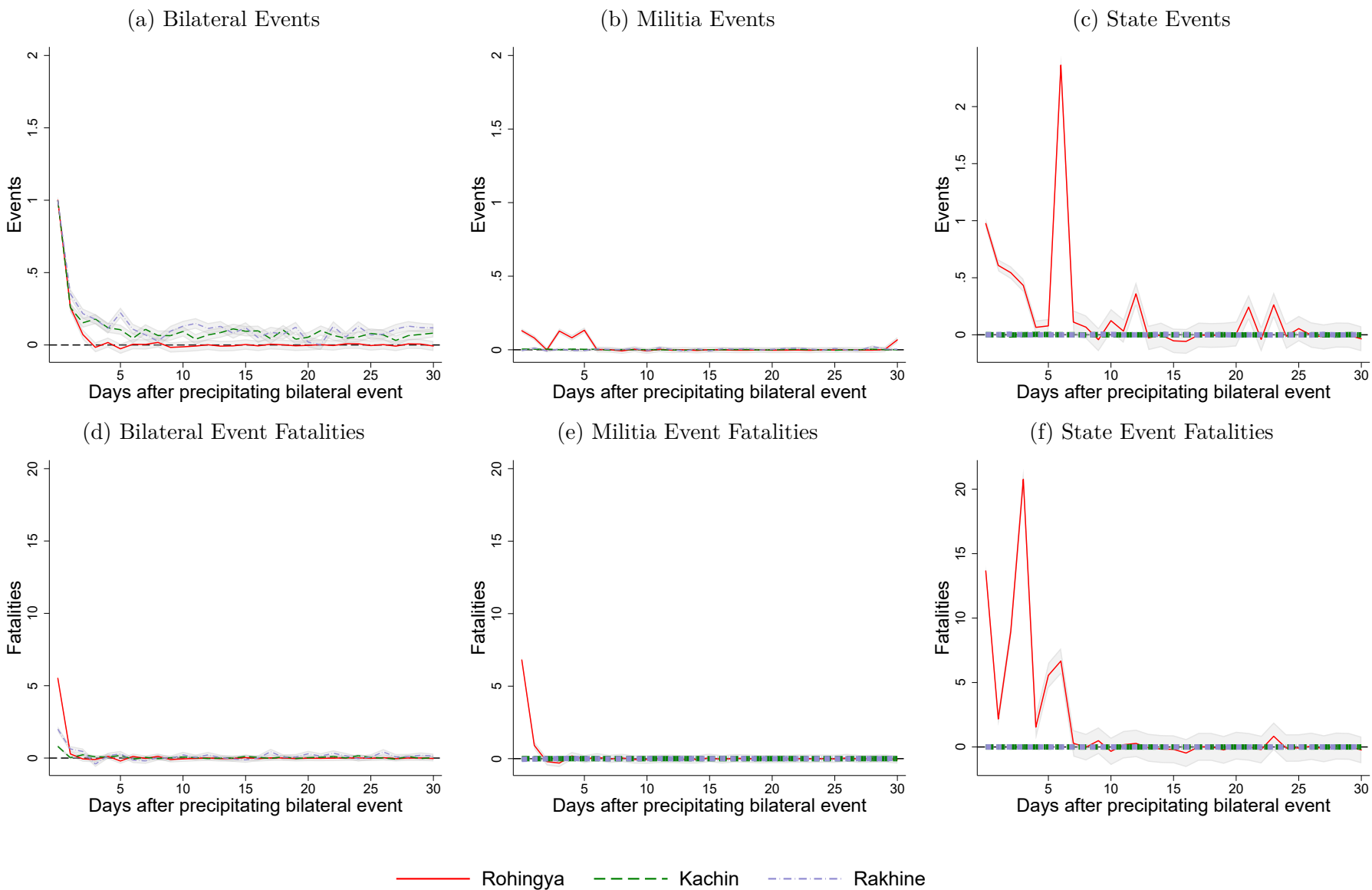
The IRFs are depicted in Figure 4, with red solid lines for the Rohingya VAR, green dashed lines for the Kachin VAR, and blue dashed lines for the Rakhine VAR. For each IRF, we depict a shaded 95% confidence band. Sub-figure 4a reports the persistence of the impulse through time. Bilateral events in the Kachin and Rakhine ethnic conflicts exhibit persistence, with 3.6 ($p < 0.01$) and 4.4 ($p < 0.01$) bilateral events in the 30 days after the impulse. In contrast, Rohingya conflict events exhibit less persistence, with just 1.3 ($p < 0.01$) bilateral events after

an impulse. Several other patterns emerge.

Asymmetry. The Rohingya conflict exhibits strong asymmetry between the ethnic militia and state forces. In Sub-figure 4b, we observe just 0.58 excess unilateral militia events ($p < 0.01$) in the month after the initial impulse. On the other hand, Sub-figure 4c shows that unilateral state events increase much more, with a cumulative increase of 6.02 events ($p < 0.01$). Similarly, for fatalities, Sub-figure 4e shows that Rohingya militias unilaterally generate only 7.12 ($p < 0.01$) fatalities in the month after the first clash, compared to 59.49 ($p < 0.01$) fatalities unilaterally due to state forces, as seen in Sub-figure 4f.

Civilian targeting. Sub-figure 4f demonstrates that state forces target Rohingya civilians much more than other minority civilians. By definition, fatalities in unilateral state force events are civilians. We see that, in the month after an impulse, Rohingya civilian fatalities increase by 59.49 ($p < 0.01$), whereas cumulative Kachin and Rakhine civilian fatalities do not significantly increase ($p = 0.94$ and $p = 0.60$, respectively).

Figure 4: Impulse Response Functions to Single Bilateral Event



Ideology. We also test an argument made by numerous qualitative sources: that state forces' Rohingya persecution was motivated and enabled by nationalist Buddhist ideology (Fink, 2018; Van Klinken and Aung, 2017; Zin, 2015; Weber and Stanford, 2017; Fink, 2018; Mahmud et al., 2019). During our study period, nationalist Buddhist groups organized events and spread messages demonizing the Rohingya people. For example, in October 2012, Buddhist monks led anti-Rohingya demonstrations in Rakhine state, which preceded severe sectarian violence (Zin, 2015).

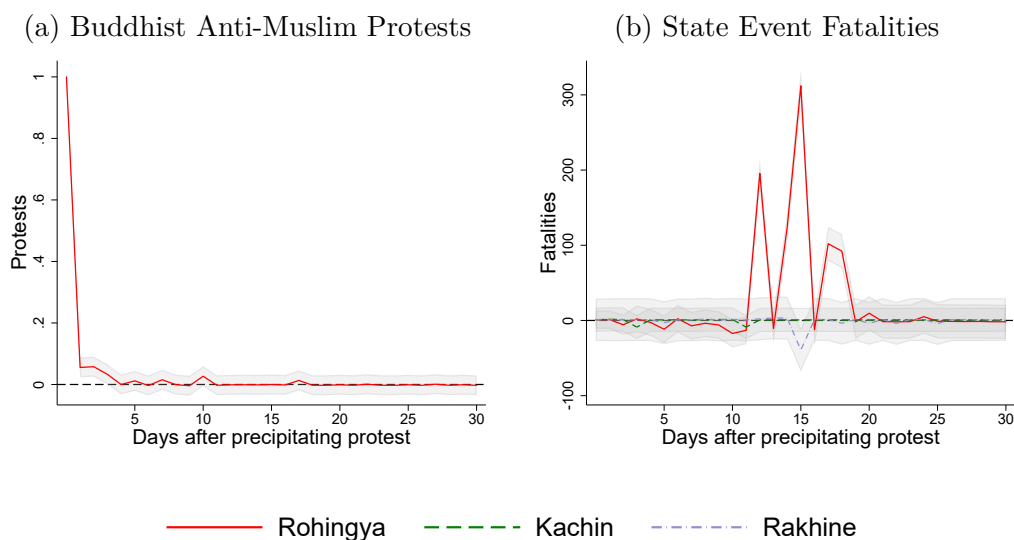
To quantitatively evaluate this hypothesis, we estimate another VAR with two series: anti-Rohingya protests held by Buddhist groups and ethnic civilian fatalities from unilateral state force events. In our sample of conflict events, there are 42 anti-Rohingya protests held by Buddhist groups. These protests oppose Myanmar citizenship for the Rohingya people and support military action against them (ACLEDD, 2019).

$$\begin{aligned} \begin{pmatrix} Buddhist\ Protests_t \\ State\ Event\ Fatalities_t \end{pmatrix} &= \mathbf{A}_1 \begin{pmatrix} Buddhist\ Protests_{t-1} \\ State\ Event\ Fatalities_{t-1} \end{pmatrix} \\ &+ \dots + \mathbf{A}_k \begin{pmatrix} Buddhist\ Protests_{t-k} \\ State\ Event\ Fatalities_{t-k} \end{pmatrix} + \mathbf{B}\mathbf{X} + \mathbf{e}_t \end{aligned} \quad (3)$$

In Figure 5, we present response functions representing excess units of a given series observed k days after one anti-Rohingya Buddhist protest. Figure 5a depicts the temporal persistence of protests; we find that 1.18 protests ($p < 0.01$) follow in the 30 days after a protest (including the initial event). In sub-figure 5b, we find that, in the month after an anti-Rohingya Buddhist protest, Rohingya civilian fatalities increase by 741.3 with $p < 0.01$. The increase takes place 12 to 18 days after the initial protest event. We repeat the process with Kachin and Rakhine civilian fatalities as placebos; in sub-figure 5b we find that those fatalities do not respond to Buddhist protests.

The relationship between Buddhist protests and Rohingya fatalities can be attributed to multiple mechanisms, though all imply that the conflict is shaped by ideology. One possibility is that religious groups initially organize protests, fostering popular support for attacks. The state could take advantage of this shift in public sentiment and time violence in the following weeks. Alternatively, the state could coordinate protests in advance of attacks, strategically

Figure 5: Buddhist Anti-Muslim Protests



increasing the benefits or minimizing the costs associated with violence. In the first case, protests are civilian-led, while in the second case, protests are state-led. We remain neutral on the relative importance of these channels, but qualitative evidence supports both (Zin, 2015; Van Klinken and Aung, 2017; Cheesman, 2017).

7 The Consequences of Violence and Displacement

In this section, we explore the consequences of ethnic violence among a representative population of Rohingya Muslim refugees. Drawing again on the Cox’s Bazar Panel Survey (CBPS), we examine both economic and psychological well-being (Baird et al., 2021).⁹ We find sharp declines in refugees’ economic well-being and high rates of trauma exposure and depression. After displacement, refugee employment rates fell by 58.8% while earnings fell by 59.2%. Over 85% of refugees displaced in 2017 reported trauma experience, and over 28% met the criteria for clinical depression.

⁹ See Online Appendix D for detailed descriptions of the CBPS data.

7.1 Employment and Earnings

Employment of refugees declined substantially after displacement. Among those displaced in 2017, employment rates fell from 47.3% before to 19.5% after displacement. Among male adults, employment rates fell from 75.9% to 37.6%. Among females, employment rates fell from 25.2% to 5.6%. Declines in employment were limited to refugees, suggesting that changes in refugee employment are not easily explained by aggregate shocks or a sharp increase in local labor supply due to displacement.¹⁰

Refugee earnings also decreased after displacement. When we plot the cumulative distribution of incomes among the refugee population before and after displacement in August 2017 in Figure 6a, we find that the distribution shifted substantially left. Median annual earnings fell from 284 USD to 106 USD. For reference, we compare these earnings changes to the host population, which is poor by global standards with a median annual income of 567 USD in 2019. Figure 6b plots the cumulative distribution of earnings among refugee and host individuals before and after the 2017 mass displacement. We find a gap between median annual earnings among refugees and host individuals of 307 USD before displacement. This gap widened to 461 USD after displacement, an increase of 50%.

Were these earnings gaps due to different population compositions between refugee and host communities? To test this possibility, we regress earnings onto age, age squared, gender, and level of education for the pre-2017 host population.¹¹ We then compute the residual of each individual's earnings after netting out the contribution of these observable characteristics. The distributions of residualized earnings are plotted in Figure 7a. We find, strikingly, that the potential earnings of refugee and host populations were very similar before displacement. Using the same coefficients for demographics, we then compute the earnings residuals for both populations in 2019. We plot these distributions in Figure 7b. We find that, after displacement and conditional on demographics, refugee populations earned substantially less than host populations. The timing of this gap strongly suggests that displacement harmed earnings. Many forces could drive this decrease, including low to no access to work or poor physical and mental health.

¹⁰ Overall employment for members of the host community fell just 0.1 percentage points over the same period, with small decreases among males being mostly offset by small increases among females.

¹¹ The educational categories are: did not complete primary, completed primarily only, and completed secondary.

Figure 6: Earnings Distributions

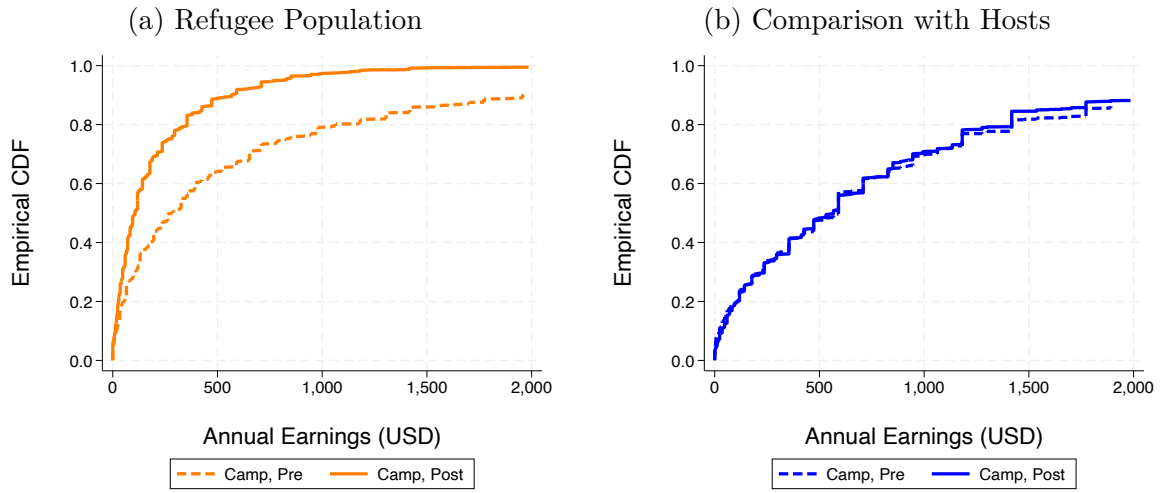
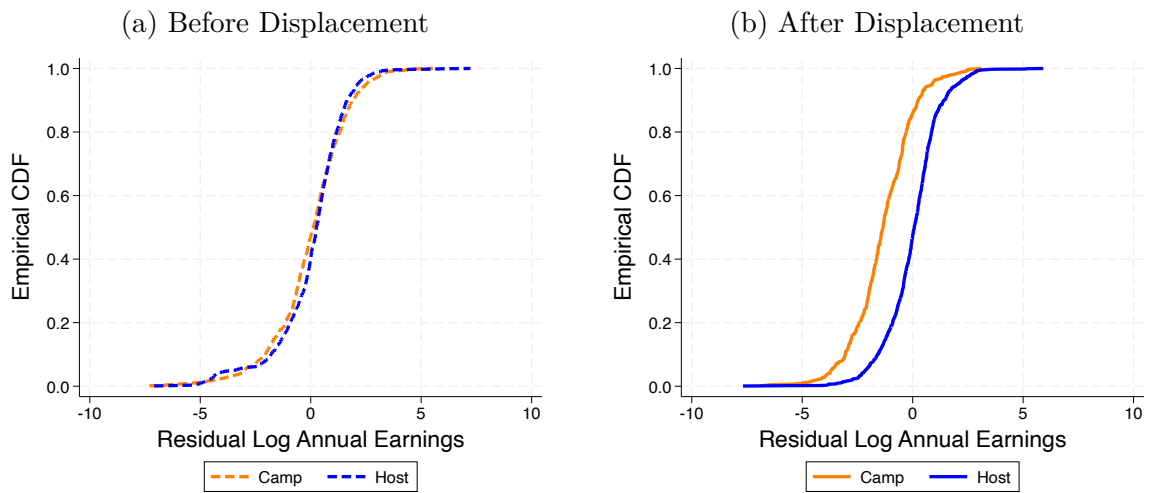


Figure 7: Residualized Earnings Distributions



Note: Earnings conditional on age, age squared, sex, and educational attainment category.

7.2 Mental Health

In this subsection, we document aggregate rates of trauma and depression among the Rohingya refugee population. Using the Harvard Trauma Questionnaire, we measure exposure to traumatic events and symptoms of post-traumatic stress (Mollica et al., 1998; Mollica et al., 1998). Table 8 shows that over 87% of refugees from the 2017 influx and 83% of those who arrived earlier experienced at least one traumatic event. Approximately half of the respondents report surviving a near-death experience and approximately one-fourth had a family member or friend murdered.

Table 8: Trauma Exposure and Depression Rates among Rohingya Refugees

	(1)	(2)	(3)
	Units: Percent of Population		
	Rohingya (2017 Influx)	Rohingya (Pre-2017)	P-value of Difference
Trauma exposure			
At least one trauma event	87.17	83.21	0.07
More than five trauma events	31.33	21.68	<0.01
By event type			
Imprisonment	13.88	17.36	0.09
Serious injury	32.88	34.37	0.60
Combat situation	46.21	29.78	<0.01
Rape or sexual abuse	5.74	4.13	0.18
Forced isolation from others	32.12	18.23	<0.01
Being close to death	54.72	43.36	<0.01
Forced separation from family	24.73	23.47	0.66
Murder of family or friend	35.32	28.74	0.05
Unnatural death of family or friend	32.29	26.35	0.03
Murder of stranger or strangers	10.54	7.63	0.13
Lost or kidnapped	10.36	8.73	0.35
Torture	45.52	34.86	<0.01
Depression	28.94	32.64	0.14
Sample size	3,914	702	

Notes: This table reports lifetime trauma events, measured using the Harvard Trauma Questionnaire. Depression is defined as a PHQ-9 Questionnaire score of 10 or higher. We report events that the respondent experienced. Significance: *** p<0.01, ** p<0.05, * p<0.1

Next, we report rates of depression among the refugee population. We measure symptoms of depression using the 9-item version of the Patient Health Questionnaire (PHQ-9).¹² Table

¹²This scale has been validated (Kroenke et al., 2001; Kroenke et al., 2010) and is widely used in clinical settings and population surveys. Each item captures the presence of the nine Diagnostic and Statistical Manual of Mental Disorders criteria for major depressive disorder in the two weeks prior to the survey. Responses range from "not at all" to "nearly every day." Scores are increasing in the likelihood and severity of the disorder.

8 shows that 28.94% of post-2017 arrivals and 32.64% of pre-2017 arrivals (29.61% overall) screened positive for depression. These values are high compared to rates in other groups: 8% of the general population in the U.S. (Goodwin et al., 2022) and 14.5% of Syrian refugees in Germany (Georgiadou et al., 2018)¹³.

8 Conclusion

Throughout history, instances of violence against civilians have inflicted severe human suffering. These events are commonly politically disputed and typically occur in situations where there is a scarcity of data. These attributes make it difficult – yet crucial – to gather representative evidence. In this study, we illustrate how researchers can utilize various data sources and quantitative techniques to shed light on this phenomenon.

We study Myanmar’s treatment of the Rohingya people, one of the most persecuted minorities in the world. The Rohingya crisis has been hotly contested, with international organizations describing it as a “a textbook example of ethnic cleansing” (UN, 2017) while Myanmar’s government maintains that the violence was idiosyncratic and not characterized by anti-Rohingya animus.

We find that the data contradict the claims made by Myanmar’s government. We find that resource expropriation systematically motivated Myanmar’s use of violence against civilians, disproving the government’s assertion that the events were idiosyncratic. State violence against civilians took place more when rice prices were high, in places suitable for rice cultivation. The patterns persist when focusing specifically on the Rohingya population or events in Rakhine state. Additionally, Rohingya refugees reported significant losses of agricultural assets, such as farmland, machinery, draft animals, and crop inventories.

We also found evidence of anti-Rohingya animus. After clashes between state forces and Rohingya militias, state forces perpetrated sustained and one-sided violence against Rohingya civilians. We found that these patterns were different from other ethnic conflicts in Myanmar. We also found evidence that anti-Rohingya ideology facilitated violence: nationalist Buddhist protests preceded state attacks against Rohingya civilians.

Following previous studies, we use a cutoff score of 10 as a marker for depression.

¹³The CBPS and (Georgiadou et al., 2018) define depression as Patient Health Questionnaire (PHQ-9) scores of 10 or higher. (Goodwin et al., 2022) defines major depressive episodes as five or more diagnostic symptoms from the National Comorbidity Survey module

Finally, we document the persecution's consequences for survivors. Rohingya refugees experienced severe declines in employment and earnings after displacement. They also suffered high rates of depression and repeated exposure to severe traumatic experiences.

Overall, the evidence presented in this paper strongly supports allegations made against Myanmar in the International Court of Justice. The conflict was systematic, asymmetric, and civilian-focused. It was shaped by anti-Rohingya ideology and exacted severe material and psychological costs on displaced people. It is clear that urgent action is needed to address the persecution and ensure the protection of the rights and well-being of the Rohingya people.

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Online Appendix

A ACLED dataset description

The Armed Conflict Location and Event Data Project (ACLED) dataset records episodes of political violence and social unrest, including battles, killings, and protests, that involve either the government, rebel groups, militias, or civilians (Raleigh et al. 2010). The data also include information on the severity of the events, measured by the number of associated fatalities.

Based on this information, we construct three measures of conflict intensity at the country-week level: (i) the number of conflict events that occurred, (ii) a dummy for whether any conflict event occurred, and (iii) the number of casualties associated with these events.

The dataset contains information from local, regional, and international news sources, as well as NGO and government agency publications, security alerts, and published texts or books. These include the Integrated Regional Information Network (IRIN), Relief Web, Factiva, and humanitarian agencies. A team of ACLED researchers monitors these sources and encodes events that belong to their classification system. Once an event is added, a researcher and a manager jointly review the coding and source materials. A third reviewer performs a quality check to ensure adherence to ACLED rules. After final approval by a data manager, the events are published.

ACLED reports fatalities only when directly included in source material. If multiple sources report different numbers or provide a vague estimate, ACLED records the lowest number. Reports mentioning "civilians" or "unknown" fatalities with no further details are coded as ten fatalities. "Dozens" of fatalities are recorded as 12 deaths, "hundreds" as 100 deaths, and "massacres" as 100 deaths. If an event occurs across multiple days or locations, the total fatalities are divided by the duration and assigned to each day, unless sources specify otherwise.

When we define our sample, we consider events that involve either Myanmar state forces, armed ethnic militias, or both. We define bilateral events as those that involve both state forces and ethnic militias simultaneously. Battles comprise nearly all these events and are defined by ACLED as "a violent interaction between two politically organized armed groups at a particular time and location".

We define unilateral events as those that involve either state forces or an ethnic militia, but not both. These events can take many forms, including remote attacks, property destruction

and looting, and violence against civilians. Civilians are, by definition, unarmed individuals who are not engaged in political violence. In our sample, perpetrators of violence against civilians must be part of an armed group.

B ACLED Variable definition

Conflict event. An event is an episode of social conflict or political violence. We focus on unilateral military interventions or bilateral clashes between armed groups. We study four main types of events: battles, riots and protests, violence against civilians, and remote violence (e.g., missile attacks, landmine explosions, etc.)

Violence against civilians (VAC). We create a binary variable that takes on value one when an event represents a deliberate act of violence and was perpetrated by “an organized political group such as a rebel, militia or government force against unarmed non-combatants.” In our data, VAC is the only type of event in which civilians are listed as actor—that is, we do not observe events in which civilians are perpetrators. It includes events involving violence against peaceful demonstrators. In these cases, victims are classified as “protesters,” to distinguish them from civilians who are not participating in a demonstration. Violence perpetrated by rioters against civilians is also coded as VAC.

While some of the victims may be perpetrators in a different event, in our data, we define violence against civilians as events in which the victims are unarmed and unable to “defend themselves or engage in violence.” VAC events also include instances of potential or actual bodily harm, such as bombing, shooting, torture, rape, or mutilation, or accosting victims (for instance, kidnapping). VAC excludes incidents in which no members of the target population are physically harmed (for example, looting or burning of buildings involving no injuries, or destruction of sacred spaces). These events are classified as “non-violent activity by a conflict actor.”

Battles. ACLED defines battles as “a violent interaction between two politically organized armed groups at a particular time and location.” In our data, we observe three types of sub-events classified as battles, depending on the outcome of the event: (i) armed clash, (ii) government regains territory, and (iii) non-state actor overtakes territory.

Strategic developments. This category includes, among others, violent events carried out

as an intimidation tool but where civilians are not physically harmed—for instance, building destruction. These events may help understand conflict patterns by describing grievances and hostilities preceding an outbreak of violence. This type of event is not necessarily comparable across time and contexts and are better understood as setting-specific early warnings. For instance, ACLED coded Rohingya village arson by military forces as “strategic development” events, to register changes in the spatial patterns of this conflict.¹⁴

Explosions and remote violence. These are “one-sided violent events in which the tool for engaging in conflict creates asymmetry by taking away the ability of the target to respond.” Examples of tools include explosive devices, such as bombs, grenades, improvised explosive devices (IEDs), artillery fire or shelling, missile attacks, heavy machine-gun fire, air or drone strikes, or chemical weapons. In cases where explosions or remote violence are part of an ongoing battle, both events are merged and coded as a single battle event. ACLED considers the following sub-events: use of chemical weapons; air and drone strikes; suicide bombs; shelling, artillery, and missile attacks; remote explosives, landmines, and IEDs; and grenades.

Fatalities. ACLED records estimated casualties only when reported by sources. If multiple sources report different fatalities or provide a vague estimate, ACLED records the lowest number. When the exact number of fatalities is unavailable, ACLED records a minimal number. Reports mentioning “civilians” or “unknown” with no further reference result in a total of 10, “dozens” are recorded as 12, “hundreds” as 100, and “massacres” as 100 fatalities. If an event occurs across multiple days or locations, the total number of fatalities is divided by the duration.

B.1 Perpetrators of Violence

Myanmar’s state armed forces. The Tatmadaw is the official name of Myanmar’s armed forces. In our data, we observe three actors belonging to this umbrella organization: the Army, Police Forces, and the Frontier Forces or “Na Sa Kha”.¹⁵ We include the Government of

¹⁴Other examples from Myanmar include: (i) property destruction: residential units, vehicles, police outposts, offices, etc., (ii) land confiscation and looting; (iii) discovery of mass graves, (iv) reports that ethnic militias prevented male refugees from leaving the country in an attempt to recruit them, (v) announcements of conditional ceasefires, peace talks, security measures, including curfews, state of emergency, martial law, clearance operations, and internet bans, (vi) charges against members of armed groups or civilians under the Counter-Terrorism Law, (vii) internal displacement, usually as a result of nearby fighting between armed groups but sometimes without a clear cause being registered, and (viii) occupation of schools, religious sites, and community halls by armed or military groups.

¹⁵In the ACLED dataset, the frontier forces are named “Border Guard Forces,” and are generally classified as a branch of either the Army or the Police Forces.

Myanmar, whenever it appears cited as an actor, but exclude pro-government militias from the definition. Violence against civilians is perpetrated, primarily, by the Tatmadaw.¹⁶

Ethnic and religious militias. We focus our analysis on the six ethnic (or religious) armed groups that represent either: (i) the most victimized minority groups, and/or (ii) actors involved in a large fraction of episodes of violence between 2010 and 2020. In particular, we include armed groups or militias¹⁷ that are involved in at least 100 conflict-related events, or that claim to represent the interest of minority groups that were the target of at least 100 interventions (Table A1).

¹⁶We also observe in the data several attacks against ethnic armies and civilians perpetrated by non-state actors. These are labeled as “unidentified militias.” A number of pro-government, non-state actors, labeled in the ACLED data set as “unidentified militias,” are responsible for many violent events involving ethnic groups—both civilians and militias. While numerous over the 10-year period for which we have data (more than 200 hundred events), the number of attacks on each of the ethnic groups under study is small. Rohingya civilians, the most frequently-victimized group, were attacked 10 times between 2010-2020. We thus lack the power to detect statistically significant relationships between these and other types of events and fatalities.

¹⁷Militias include individuals who belong to an ethnic group and are armed at the time when a conflict event takes place. In our data, civilians are always unarmed and unable to defend themselves.

Table A1: Militias claiming to defend the interest of each ethnic group

Ethnic group	Armed groups
Kachin	Includes the Kachin Independence Army (KIA), also known as the Kachin Independence Organisation (KIO), and the Northern Alliance (NA-B), a military coalition including the Arakan Army (AA), the Kachin Independence Army (KIA), the Myanmar National Democratic Alliance Army (MNDAA) and the Ta'ang National Liberation Army (TNLA).
Karen	Includes the Democratic Karen Buddhist Army (DKBA), the Karen National Liberation Army (KNU), previously known as the Karen National Defence Organisation (KNLA), and the Karen National Defence Organisation (KNDO)
Palaung	Includes the Palaung State Liberation Front (PSLF), which is the armed wing of the Ta'ang National Liberation Army (TNLA).
Ethnic Rakhine	Includes groups labeled as "Rakhine ethnic militia," the Arakan Army (AA), which is the armed wing of the United League of Arakan (ULA), and the The Arakan Liberation Army (ALA), which is the armed wing of the Arakan Liberation Party (ALP).
Rohingya & Muslims	Combines the two known Muslim militias: the Arakan Rohingya Salvation Army (ARSA) and the Muslim Militia. The latter includes Muslims who are not Rohingya.
Shan	Includes the The Shan State Army - South (SSA-S), which is the armed wing of the Restoration Council of Shan State (RCSS), and The Shan State Army-North (SSA-N), also known as Shan State Army/Special Region 3 (SSA/SR-3), which is the armed wing of the Shan State Progress Party (SSPP).

Table A2: Number of anti-Muslim protests and violent incidents against Rohingya civilians, by state/division

State/Division	Geography of anti-Muslim violence			
	Anti-Muslim protests (monks)	Anti-Muslim protests (all other)	Tatmadaw-Rohingya civilians VAC incidents	Tatmadaw-Rohingya civilians Non-VAC incidents
Ayeyarwady		1		3
Bago		1		
Magway		1		
Mandalay	5	2		2
Rakhine	23	27	141	41
Sagaing	1	2		
Tanintharyi			2	
Yangon	13	2		4
Total	42	36	141	52

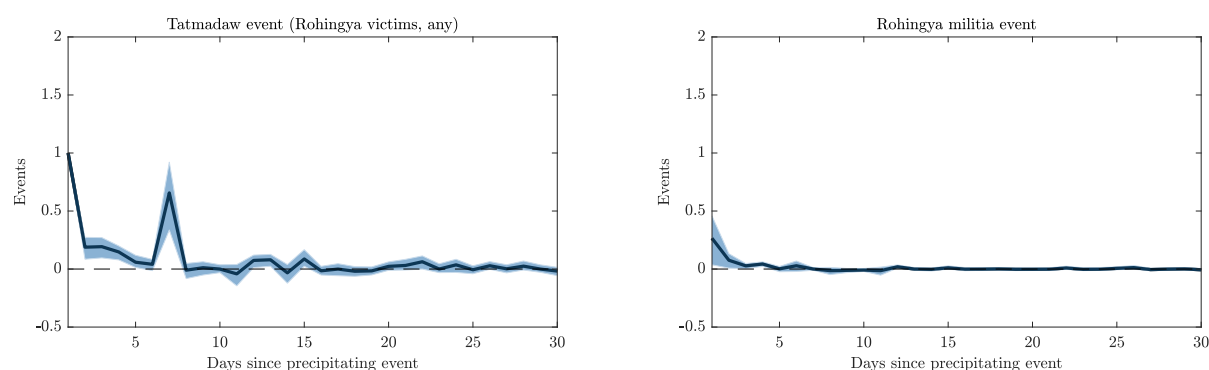
Notes: This table shows the number of incidents of each type during 2010-2020 period. Violence against civilians (VAC) includes incidents of sexual violence; attacks; abduction or forced disappearance. Non-VAC incidents involving civilians include: Shelling/artillery/missile attack; Arrests, Looting/property destruction; Remote explosive/landmine/IED; Excessive force against protesters.

C Additional analyses for section “The Persecution of Rohingya Muslims”

C.0.1 Reverse ordering of variables for Rohingya-Tatmadaw conflict events

Figure A1 shows that a conflict event involving the Tatmadaw (against Rohingya Muslims) is met with a small response (less than 0.25 conflict events on average) from the Rohingya militia.

Figure A1



C.0.2 Placebo Tests: Labor and Farmers’ Unrest Shocks

We investigate the potential effects of common types of social unrest shocks on conflict. For this, we compute the impulse response functions on interventions to labor and farmers’ shocks.

Labor shocks include protests organized by a group of workers (for instance, employees at a shoe factory) to demand improvements in their worker conditions. Common types of demands include wage increases and increased occupational health and safety rights. Participation in events varies widely and ranged between less than 20 workers to several thousand. We observe 441 labor unrest shocks across Myanmar, out of which only 4 were recorded in Rakhine state.

Farmers’ unrest shocks are, predominantly, demonstrations organized by farmers to protest cases of confiscated farmlands by the military and various state actors (for example, the Yangon Region Land Scrutinization Committee) and demand protections on the rights of farmers. In a few cases, we observe protests against private companies, after failing to compensate farmers for causing damage to their farmlands. These protests also vary in size, ranging from 20 to several thousands of participants. In our data, we observe 261 farmer unrest shocks across

Myanmar—only 19 of them took place in Rakhine state.

We find that, at the national level, these sources of unrest do not lead to an escalation of violence between the Tatmadaw and any of the ethnic militias, including Rohingya militias (Figures (Figure A2) and A3).

Figure A2: Impulse response functions of interventions by the Tatmadaw and the Rohingya militia to labor unrest shocks

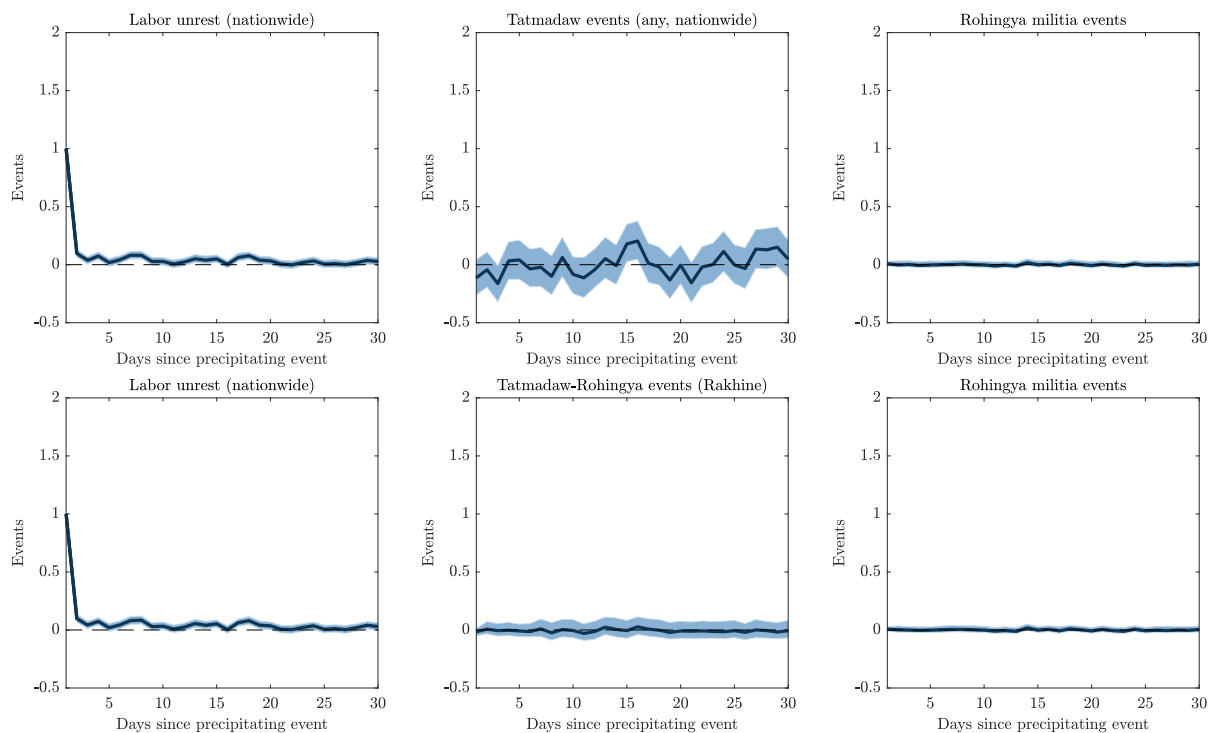
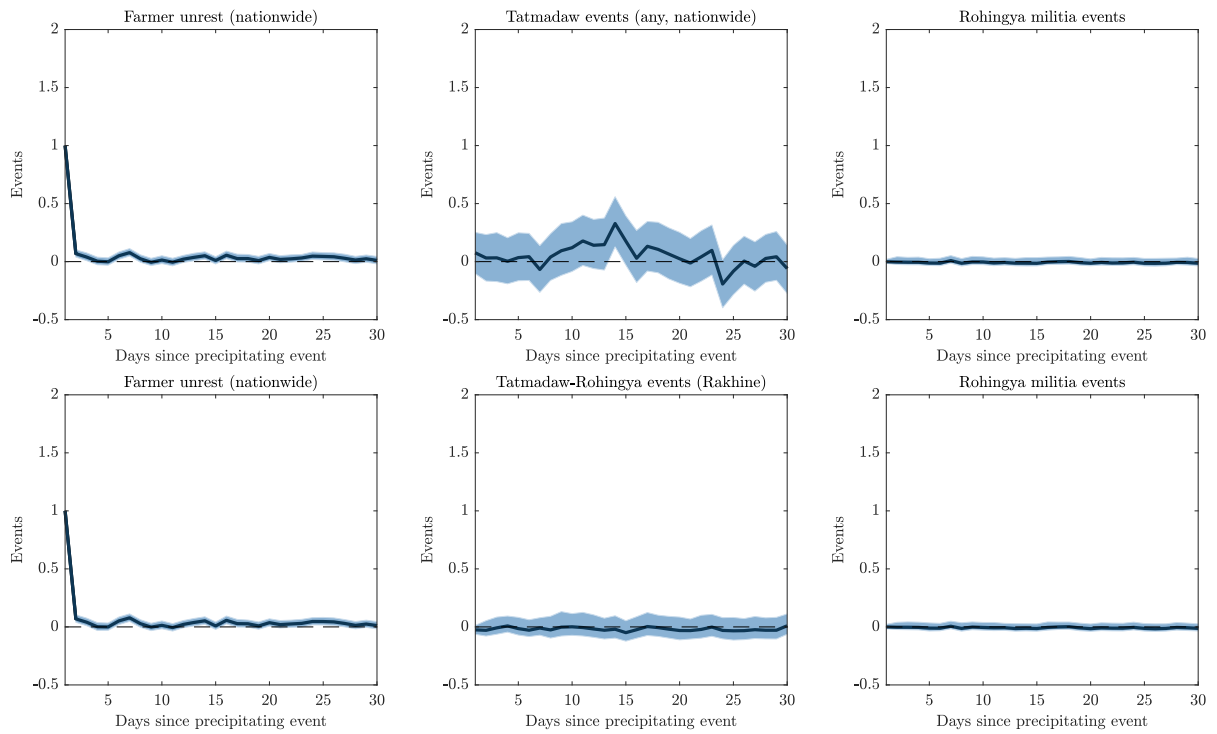


Figure A3: Impulse response functions of interventions by the Tatmadaw and the Rohingya militia to farmers' unrest shocks



C.0.3 Robustness check: Tatmadaw-Rohingya conflict events do not precede monk-led protests

Figure A4

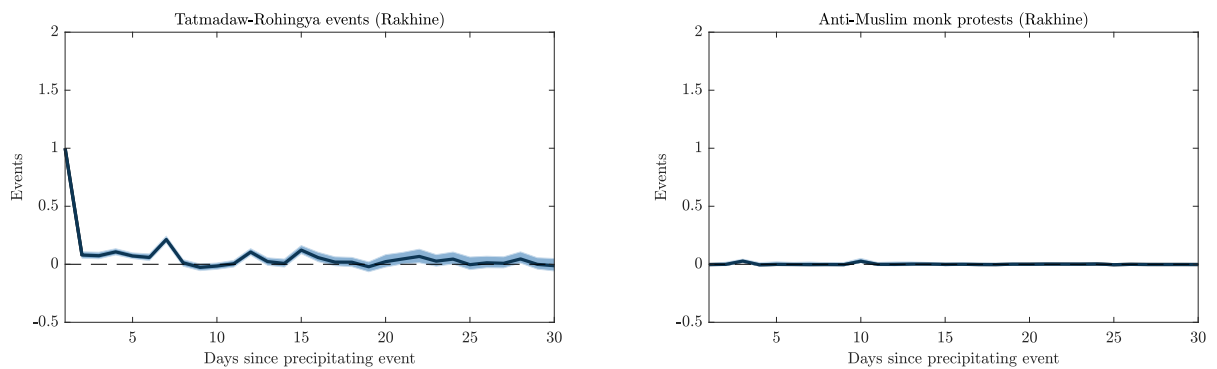
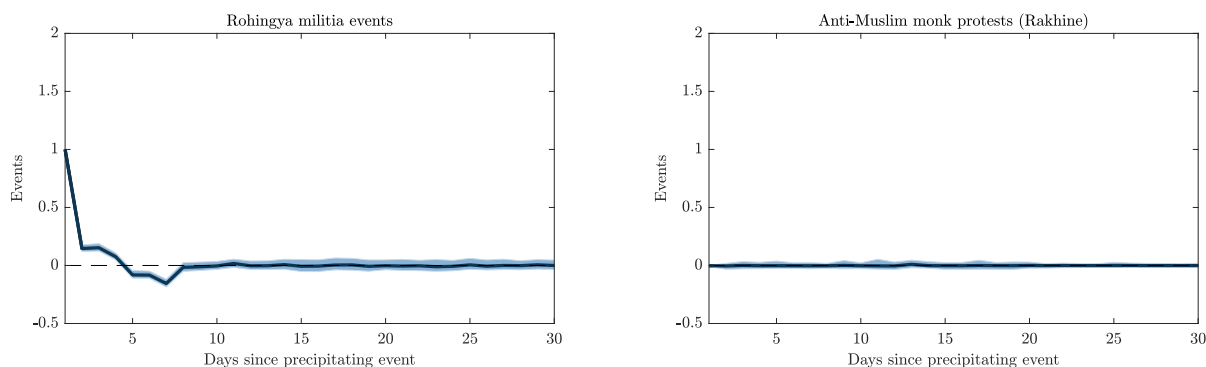


Figure A5



D Cox's Bazar Panel Survey: Implementation Details

These surveys took place in the Cox's Bazar and Bandarban districts between March and August 2019. The study had a sample size of 5,020 households, divided between refugee camps ($n=2,493$) and host communities ($n=2,527$) across Cox's Bazar.

We used a two-stage procedure to create a representative sample of camp residents. First, we selected 192 primary sampling units (PSUs) from the 1,954 camp blocks. Second, we conducted a household listing in each selected PSU and selected 13 households per block through simple random sampling.

To create a representative sample of host region households, we used mauzas as PSUs. Mauzas are the lowest administrative unit in Bangladesh. We stratified mauzas into high-spillover areas and low-spillover areas, defined as those within three hours of walking distance from the refugee camp and those further away than that. We then selected a random sample of PSUs via probability proportional to population size. Each mauza was then divided into segments of roughly 100-150 households based on the latest population census. We randomly selected three segments from each mauza, conducted a listing in each, and sampled 13 households per segment via simple random sampling.