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ABSTRACT

The Impact of Violence during the Mexican Revolution on Migration to the United States*

The number of individuals forcibly displaced by conflicts has been rising in the past few decades. However, we know little about the dynamics—magnitude, timing, and persistence—of conflict-induced migration in the short run. We use novel high-frequency data to estimate the dynamic migration response to conflict for the case of the Mexican Revolution (1910-1917), one of the deadliest conflicts in world history. We find that, on average, insurgency events led to a large increase in migration rates of about 60 percent that lasted for a few months: after five months, migration rates reverted back to pre-violence levels. This finding masks substantial heterogeneity in treatment effects, as we find larger and more persistent effects for women and children. We show that violence was the main treatment channel, with variation in the intensity and nature of violence explaining the magnitude and persistence of the migration response. While migration costs, migrant networks, and land ownership moderated the migration response to conflict, we show that these factors affect different aspects of the response.

JEL Classification: F22, N31, N32, N36

Keywords: migration, refugees, Mexican Revolution

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"I worked as a servant in my youth, but I wanted to become independent. After years of hard work, I managed to open a small store in my town. I had to sell it to come to the United States because it was impossible to continue living with so many revolutions."

—Pablo Mares, Mexican refugee from Guadalajara, Jalisco. (Gamio, 1969, p. 85)

1. Introduction

The number of people forcibly displaced due to conflict has more than doubled from around forty million in 2011 to nearly ninety million in 2021 (UNHCR, 2022). These large-scale displacements have affected the economic and political sphere in destination and source countries. However, it is not clear whether these migration flows have reshaped migration patterns. Refugees, after settling in a new country, may encourage their friends and family to join them years later, and thus permanently alter migration flows between two countries. On the other hand, displacement may only be temporary, and migration may soon return to pre-conflict levels.

A significant conflict that provides an ideal setting for better understanding the relationship between conflict and subsequent migration patterns is the Mexican Revolution. The Mexican Revolution (1910-1917) was one of the deadliest civil wars in world history, resulting in the death of approximately 1.4 million individuals (about 10% of the population) and hundreds of thousands moving north across the border (Durand, 2016; McCaa, 2003; Moreno-Brid and Ros, 2009). Since there were virtually no legal restrictions on Mexican migration to the United States during that period, we can study the migration response to conflict in the absence of significant barriers to immigration. Conflict risk was also relatively homogeneous across population groups, as no ethnicity or social class was particularly targeted (Gonzales, 2002; Knight, 1986*a*), which favors the identification of migration responses for the entire population instead of for a persecuted group. We leverage these characteristics and use newly collected data on migration and conflict to estimate how insurgency events affected migration during the Mexican Revolution, systematically examining the magnitude, timing, and persistence of the response at the local level.

Previous historical research has shown that this time period marked a fundamental change in migration from Mexico to the United States. Before the 1910s, migration to the United States was relatively uncommon, but after the revolution, Mexico became one of the top countries sending migrants to the United States in the 1920s (Escamilla-Guerrero, 2020; Kosack and Ward, 2014). Since then, there has been a consistent geographic pattern of migration, where Mexican states that had high levels of migration in 1920s still have high migration rates today. This pattern suggests that the social networks that encouraged migration over the last 100 years may have been formed during the revolution (McKenzie and Rapoport, 2010; Woodruff and Zenteno, 2007). However, instead of the revolution being the major driver of migration during the 1910s, others point to growing labor market opportunities due to the labor shortages of World War I as the major force (Cardoso, 1980; Gamio, 1930; McCaa, 2003).[1] Therefore, the extent to which the revolution caused changes to migration patterns and shaped the path of migration after the conflict ended is a decidedly empirical question.

To estimate the effect of conflict on migration dynamics, we collect two novel data sources that are unique in terms of frequency and completeness. First, we collect daily data on migration flows during the 1910s from individual border crossings registered at 23 entry points along the US-Mexico border. Second, we digitize daily data on insurgency events from military reports compiled in the "Military History of the Mexican Revolution" (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983). These documents cover the most violent period (November 1910 to December 1915) only—characterized by generalized insurgency—as from 1916 the revolution was reduced to local guerrilla movements that lasted until 1917 and for which there is no comprehensive data available. The information contained in the reports allows us to assign insurgency events to a given day and a specific location (i.e., latitude and longitude), as well as to observe information about the intensity of violence such as the length of the event, the number of military casualties, and whether the event caused damages to civil infrastructure. The reports are classified into four military categories (shooting, combat, battle, or siege), which also capture the nature of conflict and intensity of violence. We link these two sources together at the municipality level (comparable to a county in the United States) and construct a month-by-municipality panel.

We employ a fully flexible event-study design to estimate the impact of conflict events on migration. More specifically, we compare monthly migration rates from municipalities that experienced a conflict event to municipalities that had not yet experienced or never experienced a conflict event. We find that conflict caused a significant short-run increase in migration rates of 60% relative to pre-event levels. This large effect only lasted for a few months after an event. After five months, migration rates returned to pre-event levels, and we do not find any permanent impact of conflict on migration rates beyond this time window. As part of our analysis, we show that our baseline estimates are unlikely to be

[1] McCaa (2003) speculates that about half of the 350,000 who moved northward during the 1910s moved due to pull factors rather than push factors.

biased by confounders, anticipatory behavior, interference between units in treatment assignment, or treatment effects heterogeneity, and therefore uncover the true effect of conflict on migration for treated municipalities. While we do not detect a permanent increase in migration overall, we find marginal evidence on larger and more persistent effects for women and children, which are likely to have changed the sex composition of the migration flow between 1910 and 1915.

We also show that violence was the main treatment channel. Following Bohra-Mishra and Massey (2011) and Kondylis (2010), we measure the intensity of violence with the number of casualties across events. We find that more violent events led to a large transitory increase in migration that lasted for about three months. In contrast, there was no migration response in municipalities that had a conflict with zero casualties. This result is in line with recent literature arguing that living in a violent context makes life shorter and less predictable (Aburto et al., 2023), which pushes individuals to move to places where the returns to human and physical capital are greater in the short- and long-run (Becker, Mukand and Yotzov, 2022; Orozco-Aleman and Gonzalez-Lozano, 2018). To provide further evidence on the treatment channel, we use alternative violence metrics, including the length of the event and information on whether the winner left or remained in the area where the event occurred. We find a large and permanent migration response to insurgency events that lasted more than three days and involved the occupation of territory by the victors.

In addition, we present new insights on how migration costs, migrant networks, and land ownership shape the decision to migrate in contexts of conflict. We use distance estimates by train from each municipality to the border to proxy for migration costs (Woodruff and Zenteno, 2007). We find large transitory effects in municipalities located 200 to 700 km from the border, with migration increasing by 60 to 80 percent relative to the baseline level in the month when the event occurs and during the following two months. We also find that migration increased more than 100 percent relative to the baseline level in municipalities located more than 700 and up to 1,200 km from the border. These results provide strong evidence that migration costs did not preclude refugee flows, but rather moderated the time of response to conflict.

To examine the role of migrant networks, we define municipalities as "networked" if we observe a history of migration before the occurrence of the first conflict.[2] When we divide the sample by networked

[2] The frequency of the data allows us to observe migration in the months prior to the event even in municipalities that were treated at the onset of the revolution. We can also observe migration between 1906 and 1908 based on data from Escamilla-Guerrero (2020).

and non-networked municipalities, we find a large and temporary increase in migration in networked municipalities, but no migration response to conflict in non-networked municipalities. This result suggests that contacts abroad were needed for being able to escape conflict, a result that aligns with research showing that networks make feasible the migration decision in contexts of armed conflict or persecution (Becker et al., 2021; Buggle et al., 2023; Spitzer, 2021). Our analysis suggests that these findings also apply to episodes characterized by general (applying to everyone) and temporary conflict, and not just to ethnicity-based violence that may reflect prolonged discrimination over time.

As for land ownership, pre-revolutionary Mexico was characterized by a high concentration of land (Chevalier, 1970; Florescano, 1987; Knight, 2016), with about 800 families owning more than 90% of the arable land. Moreover, landowners used coercive mechanisms of debt bondage to keep labor attached to haciendas (Alston, Mattiace and Nonnenmacher, 2009; Moreno-Brid and Ros, 2009). We use data on land concentration in 1900 from Sellars and Alix-Garcia (2018)—the share of the population living in large estates (haciendas or ranches) by municipality—to study how conflict affects migration in municipalities with different land ownership structures. In particular, we estimate the impact of conflict events for municipalities below and above the 75th percentile of the land concentration distribution. We find that conflict events had no effect on migration in municipalities with high land concentration, while in municipalities with lower land concentration, we observe a large increase in migration rates of 50 to 90% over the baseline. This finding supports the view that land ownership facilitates migration from areas affected by conflict, but also highlights that the structure of land ownership and labor institutions are likely to moderate the magnitude of the migration response.

We contribute to a long interdisciplinary literature on the relationship between conflict and migration.[3] One of our key contributions is the use of high-frequency within-country data, which allows us to more accurately pinpoint the dynamics (magnitude, timing, and persistence) of migration in response to conflict and violence. Prior studies in economics have often relied on cross-country and cross-year variation in conflict intensity (e.g., Schmeidl (1997); Melander and Öberg (2006); Davenport, Moore and Poe (2003)). Others have used within-country variation in conflict or violence but measured migration annually, potentially missing crucial dynamics surrounding the event (Spitzer, 2021; Orozco-Aleman and Gonzalez-Lozano, 2018).[4] To our knowledge, the only study that estimates how within-county variation in violence affects *monthly* migration is Williams et al. (2012), which estimates the effect of conflict

[3] See, for example, Bohra-Mishra and Massey (2011); Stanley (1987); Schmeidl (1997); Williams et al. (2012)

[4] There is a small literature on how shocks from natural disasters affect international migration (e.g., Halliday (2006); Mahajan and Yang (2020); Spitzer, Tortorici and Zimran (2020), but this literature also focuses on yearly migration rates.

during the Nepalese Civil War on the likelihood of migration between 2001 and 2006. Our paper differs by identifying the dynamics of migration, where we find a large short-run response that only lasts for about four months. Our setting is also unique in that there were limited migration restrictions between Mexico and the United States, allowing us to identify the effect free from policy constraints.

The second contribution we make is the identification of the effect of conflict on migration for the entire population. Most historical studies on forced migration or displaced people examine episodes characterized by the persecution of specific ethnic groups, whose migration response may not be representative of the whole population (Becker and Ferrara, 2019; Becker, 2022; Becker et al., 2020; Becker, 2022; Becker et al., 2021; Buggle et al., 2023; Spitzer, 2021). However, there are many instances where conflict changes the costs and benefits of migration for everyone, not for a specific group. The Mexican Revolution is one such conflict where no group was targeted for persecution, but where the violence was equally deadly for all who were exposed to it. We show that in the absence of persecution, violent conflicts can induce increases in migration rates and refugee flows.

The third contribution we make is to improve our historical understanding of the most important migration flow of the 20th century (Borjas, 2007). Many have argued for the importance of the Mexican Revolution in transforming Mexican migration and the American Southwest.[5] However, we are the first to provide direct, quantitative evidence on the impact that this conflict had on the scale and composition of flow at the local level. Our findings suggest that migration was influenced by the Mexican Revolution, but it did not significantly change the geography of migration or the relative importance of migrant-sending municipalities. We find a strong persistence of migration patterns: the correlation between the 1910 and 1920 percentile rank of migration rates is 0.83—that is, high-sending municipalities before the Mexican Revolution remained high-sending municipalities for years after the conflict ended. Unfortunately, with our data, we cannot observe who stayed within the United States or who returned, which makes it difficult to pinpoint the full demographic consequences of the revolution.[6]

[5] In one recent example, prominent historian Kelly Lytle Hernandez argued"[the] refugee population that arrived in the United States between 1910 and ... 1920 is the foundation of the growth of the Mexican American population today. So many families across the United States today can trace their origins north of the border to the Mexican Revolution." (NPR, 2022)

[6] We also cannot observe migration within Mexico. Previous literature agrees that the 1921 Mexican Census is notoriously unreliable (McCaa, 2003; González Navarro, 1974), and therefore it is difficult to infer how important internal displacement was during the revolution.

2. Historical Background

At the turn of twentieth century, Mexico was engulfed in an economic and political crisis, whose origin can be found in an outdated agrarian system and persistent social inequalities (Rosenzweig, 1965; Silva Herzog, 1965). Mexico's agricultural sector was based on large estates (haciendas) owned by a small group of elite landowners since colonial times (Chevalier, 1970; Florescano, 1987; Knight, 2002).[7] Although relatively modern machinery and irrigation systems were used in some haciendas, productivity in the vast majority of them was based on the exploitation of labor (Tannenmbaum, 1935). Landowners also used coercive mechanisms of debt bondage that restricted internal migration and forced the great majority of the population to work permanently in the haciendas (Moreno-Brid and Ros, 2009; Sellars and Alix-Garcia, 2018).

In addition, living standards had decreased due to the stagnation of wages and rising inflation. Commodity prices had soared by 25% since 1900, with some regions experiencing increases of up to 91% (Colegio de México, 1965). This led to widespread hardship and jointly with the high concentration of land, lack of democracy and freedom of speech, and a poor implementation of justice—characteristics of the Díaz dictatorship (1877 to 1911)—generated increasing discontent among the peasant population and the urban middle class. These conditions influenced the creation of the Anti-Reelectionist National Party led by Francisco Madero, with the objective of overthrowing Díaz in the 1910 elections. However, Madero was jailed before the elections and Díaz claimed to be re-elected for the eighth time (Dell, 2012; Garcíadiego, 2004).

As a response in November 1910, Madero called the population to rebel and fight against the regime. By March 1911, there were numerous organized insurgency movements across the country, primarily fighting for a better distribution of land, better living conditions, and a democratic electoral system (Knight, 1986*a*). These factions were popular social movements in which peasants, land owners, miners, railwaymen, and urban workers participated equally. In May 1911, Díaz was defeated, and Madero was elected president shortly after. However, he was assassinated in a counter-revolutionary coup in 1913, which led to insurgency against the federal government until mid-1914. Afterward, the revolution became a multi-sided conflict between different revolutionary factions that lasted until the end of 1915, when the Constitutionalist faction—led by Venustiano Carranza—defeated the *Division del Norte* army—led by Pancho Villa—and controlled most territories occupied by other revolutionary factions. Carranza's

[7] Using tax records, Sellars and Alix-Garcia (2018) document that the haciendas had an average size of 15,500-13,000 ha.

government was recognized by the United States in October 1915, and from 1916 the revolution was reduced to local guerrilla movements that mainly took place in the south of Mexico and lasted until 1917 (Garcíadiego, 2004; Knight, 1986*b*; Sánchez Lamego, 1983).

The revolution had a profound impact: the total demographic cost is estimated at 1.4-2.1 million people, two-thirds of which correspond to an excess of deaths (casualties and deaths caused by war-related factors such as famines and diseases) (McCaa, 2003; Moreno-Brid and Ros, 2009). This high death toll was influenced by the generalized use of modern warfare technologies such as the machine gun (Buchenau, 2011; Guzmán, 1965). The historical literature estimates that migration to the United States may account for one-fourth to one-third of the population loss (Gamio, 1930; McCaa, 2003; Ordorica and Lezama, 1993). However, some of these estimates are based on figures from the 1920 US Census, and therefore do not take into account return migration flows. Thus, we know little about the impact that violence during the Mexican Revolution had on Mexico-US migration patterns, especially since previous scholarship did not have access to fine-grained immigration data.

3. Data

3.1 Immigration: Border Crossing Records

To estimate the impact of violence on migration, we compile migration data from the Mexican Border Crossing Records between 1910 and 1920. These records are analogous to ship manifests used to research European migration to the United States; however, most Mexicans crossed into the United States via train or on foot. We use the universe of existing records available on Ancestry.com, recording 280,570 individual border crossings.[8] For each record, we observe information on first name, last name, age, place of birth, port of arrival, and date of crossing (day, month, and year).

To identify the migrant's location of origin, we classify places of birth into municipalities using the 1910 Census Catalogue of Localities (Mexico Secretary of Finance, 1918) and the Mexican Historical Archive of Localities (AHL), both maintained by Mexico's National Institute of Statistics and Geography

[8] The initial data collection was performed in 2015. We use data from National Archives and Records Administration publication A3365 (Brownsville and Others, Texas), A3370 (Columbus, New Mexico), A3372 (Naco, Arizona), A3377 (Ajo and Others, Arizona), A3379 (Laredo, Texas), A3395 (Del Rio, Texas), A3406 (El Paso, Texas), A3412 (El Paso, Texas), A3423 (Brownsville, Texas), A3431 (Laredo, Texas), A3437 (Laredo, Texas), A3466 (Presidio, Texas), A3467 (Calexico, CA), A3492 (Hidalgo, TX), M1502 (Brownsville, TX), M1754 (El Paso, TX), M1755 (El Paso, TX), M1759 (Douglas, AZ), M1760 (Douglas, AZ), M1767 (San Ysidro, CA), M1769 (Nogales, CA), M1770 (Rio Grande City, TX), M1850 (Sasabe, AZ), M2030 (Campo, CA)

(INEGI).[9] Although most records include information (state and town of birth) on the location of origin, 2.5% (7,094) of them do not report sufficient information to be accurately classified, which we exclude from the analysis. We exclude duplicate records across publications and records for non-Mexican individuals—Asians or Europeans who first migrated to Mexico before entering the United States. We also limit the data to individuals who crossed the border before 1916, which reflects the recording of insurgency events in military reports as discussed in the next section. Finally, we use the individuals' first name, if clearly reported, to infer sex. Following these refinements, our final sample contains 196,893 unique individuals who crossed the border between January 1910 and December 1915.

We collapse the individual-level data to estimate monthly migration rates by municipality: the main unit of analysis. We estimate migration rates (per 1,000 inhabitants) as the number of migrants over the municipality's population level according to the 1910 Census. To gauge whether violence had a larger impact on different demographic groups, we also estimate age-cohort- and sex-specific migration rates using the relevant denominator for the population at risk of migration. The age cohorts that we analyze are 1-15, 16-40, and over 40 years. Considering that in 1910 life expectancy at birth in Mexico was about 30 years (McCaa, 2003, p. 392), these demographic groups capture children, adults, and elderly, respectively.

3.2 Insurgency Events: Military Reports

Key to our analysis is linking the migration decision with insurgency events during the Revolution. To do so, we combine the migration data with newly digitized data on insurgency events during the Mexican Revolution. These data come from the universe of military reports of all the factions that participated in the conflict. These reports are compiled in eight volumes that constitute the "Military History of the Mexican Revolution" (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983). From these reports, we identify 2,411 unique insurgency events spanning from November 1910 to December 1915. Each report contains precise information on the place where the event occurred, which allows us to assign latitude and longitude coordinates to each location and classify them into municipalities. In addition, diverse features of the event are systematically reported, including the length of the conflict (days or hours), the number of military casualties, whether civil infrastructure was damaged during the event (railways, telegraph offices, bridges, and town halls are the most common cases), whether the winner remained in

[9] The AHL records changes in names of localities beginning in 1900 and counting at approximately 10-year intervals.

the conflict's location, and the winning faction.[10] The reports also include the event's military category: shooting, combat, battle, or siege (see Figure A.1).

In Table 1 we present the characteristics of insurgency events by military category. Shootings were the least violent of the events. They lasted about one day, caused 36 casualties on average, and less than 5% produced damages to civil infrastructure or the occupation of territory. Combats lasted slightly longer, but caused one hundred times more casualties and were more likely to induce the occupation of territory. Although sieges and battles represented only 2% of the events, they were significantly more violent, causing on average about 1,800 and 3,500 casualties, respectively. One in three sieges induced the occupation of territory by the victor and half of the battles caused damages to civil infrastructure. Hence, these military categories capture the variation in violence intensity across conflict events. Since the reports include the precise date of the events, we can also observe how the intensity of violence varied over time. Figure A.2 shows that the majority of events (about 70%) occurred during the third and fourth years of the revolution. However, those lasting more than three days and those associated with infrastructure damage—that is, events characterized by greater violence—took place throughout the entire period of analysis.

Figure 1 illustrates the variation in violence intensity at the local level. Almost all states experienced conflict events, but the intensity of violence, measured as days of conflict, varied substantially across municipalities. For instance, the Mexico City region was particularly affected by conflict events lasting more than 15 days. Within each state, we also observe municipalities where conflict events did not occur during the revolution (see zoomed-in region). Table 2 compares demographic and economic characteristics of two municipality groups: ever with conflict and never with conflict. In the last column, we present estimates of differences between these groups conditional on state fixed effects. We observe no statistically significant differences in pre-revolution migration rates, sex composition or illiteracy. We also find no differences in altitude, distance to the US border, distance to the nearest train station, or distance to the border by train. This evidence suggests that both municipality groups had a similar population composition and that their inhabitants faced equivalent migration costs. However, we find differences in population levels, number of large states, and access to communication and transportation technology, suggesting that conflict events were more likely to occur in municipalities with local relevance—that is, with higher economic and strategic value within states (Blattman and Miguel, 2010; Kalyvas, 2006).

[10] In some cases the number of civil casualties is also reported.

4. Identification Strategy

Our identification strategy exploits variation in conflict across municipalities and over time to identify the impact of violence on migration to the United States. Particularly, we are interested in the dynamics of migration—that is, how does migration change following a conflict event and when are these changes observed, if any. To identify these dynamics, we use a flexible event-study design:

$$y_{mt} = \mu_m + \lambda_t + \underbrace{\sum_{j=-5}^{-2} \beta_j D_m \cdot \mathbb{1}\{t - T_m^* = j\}}_{leads} + \underbrace{\sum_{j=0}^{5} \beta_j D_m \cdot \mathbb{1}\{t - T_m^* = j\}}_{lags} + \varepsilon_{mt}. \quad (1)$$

We regress the migration rate y_{mt} for municipality m in month-year t (January 1910, February 1910, ..., December 1915) on a binary indicator variable D_m for municipalities where a conflict event ever occurred. This variable is interacted with indicators for the five months before the first violent event (T_m^*) is observed (leads) and with indicators for the five months following the event (lags). The β_j coefficients are interpreted as the difference in migration rate between treated and control groups relative to the difference in migration rate in the omitted base period, the month prior to the event (lead -1). These leads and lags capture the migration dynamics surrounding the event—that is, the magnitude, timing, and persistence of the migration response in the months following a violent event. We assume staggered treatment adoption—once a unit is treated, it remains treated—as using variation in repeated events requires further assumptions on when outcomes (migration rates) return to baseline levels (Callaway and Sant'Anna, 2021). Observations that are more than five months before or after the event are included in lead -5 and lag 5, respectively. Thus, the estimated effect of the last lag $D_m \cdot \mathbb{1}(t - T_m^* \geq 5)$ provides information about the persistence of the response: whether conflict led to a transitory or "permanent" increase in migration (Clarke and Tapia-Schythe, 2021; Freyaldenhoven et al., 2021). Note that the estimate for the fifth lag is the weighted average of the effect in all future months, and thus reflects changes in migration that persisted until the end of the period examined (December 1915).

We include municipality fixed effects μ_m, which control for unobserved unit-specific factors that are constant over time and may influence migration, such as distance to the US border, pre-existing migration networks, or degree of urbanization. We also include time (month-year) fixed effects λ_t, which control for unobserved time-specific forces such as seasonal variation in migration. The month-year indicators also control for turning points in the direction of the revolution that may have led to migration in all municipalities, such as the assassination of Francisco Madero in February 1913 or the defeat of the Federal Army in July 1914. Since not all municipalities experienced conflict, we use not-yet- and never-treated units as control group, which allows us to avoid potential identification issues present in event study designs where treatment rolls out for the full sample (Sun and Abraham, 2021).[11] In our baseline specification, we cluster standard errors at the treatment level, i.e., by municipality (Bertrand, Duflo and Mullainathan, 2004).

For our empirical strategy to estimate average treatment effects on the treated (ATT), four identifying assumptions most hold: parallel trends in baseline outcomes, no anticipatory behavior prior to treatment, treatment effect homogeneity, and no interference between units in treatment assignment (Rosenbaum, 2007; Sun and Abraham, 2021). The parallel trend assumption states that conditional on municipality and time fixed effects, migration rates in treated municipalities would follow the same trend as the migration rates in untreated municipalities if the treated municipalities had never experienced conflict. No anticipatory behavior requires differences in migration rates between treated and control groups to be equal to the baseline difference in periods close to the event. No interference between units implies the absence of spillover effects across municipalities, and treatment effect homogeneity implies treatment effects to follow the same path among all municipalities first treated in the same time period. We will provide evidence supporting these assumptions as we present our results.

5. Results

Figure 2 shows our baseline estimates. We find a transitory surge in migration in the first four months after a conflict event. Relative to a baseline monthly migration rate (0.08 per thousand), migration increased by about 25 percent in the month when the event happened. This estimate, however, is not statistically significant at the 10 percent level and thus potentially captures the period during which individuals assess the opportunity cost of migration after experiencing conflict for the first time. We will later show that this delay in the response time is unlikely to reflect traveling times to the border. The magnitude of the effect grew larger for the first and second months after the event, with migration increasing by 60 percent relative to the baseline level. After this initial two-month surge, migration rates then slowly reverted back to baseline levels such that there was no permanent change in migration. In the third and fourth months migration rates fell to a 30 percent increase relative to the baseline and reverted back to pre-treatment levels from the fifth month onward. This transitory effect is clearer when we increase the time window of

[11] For never-treated units $T_m^* = \infty$ and we set $D_m = 0$ for all leads and lags.

analysis to ten lags. Figure A.3 shows a similar pattern: migration rates effectively returned to baseline levels in the sixth month and leveled off afterward. The p-value (0.32) of a Wald test for the last post-event coefficients (9 and +10) being equal confirms the transitory effect of violence on migration.

After estimating our event-study specification, we examine the presence of pretrends in migration rates before the event. There could be time-varying unobservables particular to treated municipalities such that those untreated do not provide an appropriate counterfactual trend in the absence of conflict. Clearly, this is a concern given that our specification does not include control variables due to the lack of historical data varying at the municipality-month level. Figure 2 shows that in the five months leading up to the event, there is no statistically different trend in migration rates between treated and untreated municipalities. We also present the p-value (0.63) of a Wald test for all pretreatment coefficients being equal to zero, which further supports the parallel trend assumption. Another concern is the presence of anticipatory behavior prior to treatment. For instance, locals may have known that a combat would occur beforehand, causing individuals to flee before the event. The point estimates on the pre-treatment coefficients and our test on the lack of pretrends provide evidence of no anticipation effects—that is, violence was sudden and unexpected for local residents.

It is also possible that migration had increased in municipalities that were eventually or never treated in response to events that occurred in neighboring locations. If the effects of conflict extend over control units that are close to treated units, these control units would fail to identify the counterfactual trend and thus our identification strategy would produce biased estimates (Aronow et al., 2021; Butts, 2021). To examine the presence of spillover effects, we assign treatment to all municipalities belonging to the same district based on the time period when the first municipality of the district first experienced conflict. This approach assumes that spillovers were local and therefore experienced within districts only. If spillover effects were significant, we would expect to observe migration dynamics similar to those captured by our baseline estimates, as municipalities affected by spillovers would experience changes in outcomes similar to those of treated municipalities. Figure A.4 shows that the coefficients of all leads and lags are relatively small and not statistically significant—that is, the ATT dissipates when units potentially affected by spillovers are considered as treated. This provides suggestive evidence that locations close to municipalities affected by conflict experienced non-significant spillover effects, if any.

A recent body of literature has shown that in settings where the timing of treatment varies across units, the coefficients on leads and lags can be contaminated by treatment effects in other periods (see, for example, Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). This bias—known as treatment effects heterogeneity—implies that the coefficients on leads are uninformative about the presence of pretrends or anticipatory behavior, and the coefficients on lags will not capture the migration dynamics caused by conflict. To correct for heterogeneity in treatment effects across adoption cohorts, we implement the interaction weighted estimator developed by Sun and Abraham (2021), which captures the cohort-specific average difference in outcomes relative to never being treated—that is, the cohort average treatment effects on the treated (CATT). Figure A.5 shows estimates robust to treatment effects heterogeneity. We find very similar migration dynamics: a large increase in migration during the first and second months that effectively reverted back to pre-treatment levels after the fifth month. The estimates also support the absence of pretrends and anticipatory behavior. Figure A.5 also shows that our findings hold when implementing the estimators of Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfoeuille (2020).

In sum, our analysis suggests that our baseline estimates are unlikely to be biased by confounders, anticipatory behavior, interference between units in treatment assignment, or treatment effects heterogeneity, and therefore uncover the true effect of conflict on migration for treated municipalities. The identification of these migration dynamics, however, is feasible due to the monthly frequency of our data. In Figure A.6, we present estimates using conflict and migration data varying at a lower frequency. Although less precisely estimated, we continue observing migration dynamics similar to the baseline estimates with data varying quarterly. However, no dynamics can be identified with data varying semiannually, highlighting the importance of using high-frequency data to more precisely identify the migration dynamics caused by conflict events. This exercise also suggests that results in previous literature using low-frequency data may capture the effects of unobserved confounds such as migrant networks.

5.1 Robustness Checks

We perform a series of robustness checks to address potential threats to identification. One concern is that municipalities that never experienced conflict may not be an appropriate control group, as these locations may be very different from municipalities that were eventually treated. Table 2 shows that there were some differences between ever- and never-treated locations. We follow Sun and Abraham (2021, p. 178) and Callaway and Sant'Anna (2021, p. 205) and drop all never-treated municipalities from the control group. In Figure A.7 we present estimates of Equation 1 using municipalities that had not yet experienced conflict as control group (Panel A). We observe a very similar migration response to conflict in terms of magnitude, timing, and persistence as in our baseline specification (Panel B). These migration dynamics are robust to clustering standard errors at different levels (municipality, district, municipality-time, or district-time) and remain virtually unchanged after we correct standard errors for temporal and spatial correlation (Colella et al., 2019; Conley, 1999) (see Table A.1).

A second concern is that a large number of never-treated municipalities concentrate in Oaxaca (see Figure A.19). Historically Oaxaca has been highly fragmented, with about 40% of the municipalities that existed in 1910 belonging to this state. The over-representation of Oaxaca municipalities in the control group may bias our estimates if the evolution of outcomes in these locations differs significantly from that in eventually-treated municipalities. Relatedly, Figure 1 shows that conflicts lasting more than 15 days concentrated in Mexico City and neighboring states (Mexico and Morelos). It is possible that the dynamics of the migration response in this region could be influencing our findings. To address these concerns, we first estimate Equation 1 excluding municipalities of Oaxaca and then excluding municipalities of the Mexico City region. Figure A.8 shows that our findings hold after performing these robustness tests, confirming that the migration dynamics previously described are not driven by the composition of our control group or specific regional responses.

A third concern is that our findings may be driven by how we assign treatment. Recall that we assign treatment based on the first event and assume staggered treatment adoption. Our finding showing a transitory migration response could only be capturing the impact of less violent conflicts if these are overrepresented among events that happened first. To rule out this possibility, we assign treatment based on events other than shootings, which were the least violent conflict events by any metric (see Table 1). Similarly, our result showing that migration rates returned to baseline levels after five months implies that events other than the first one did not significantly affect migration. As a robustness test, we estimate Equation 1 for units that experienced three events assigning treatment based on the second occurrence. Figure A.9 shows that the migration response is virtually the same when shootings are not considered for treatment assignment. It also shows that events experienced after the first occurrence were unlikely to induce migration; therefore the characteristics of events experienced for the first time played a major role in shaping the migration response to conflict.

5.2 Heterogeneous Effects of Conflict

Our finding of a large transitory increase in migration may mask heterogeneous responses to conflict, as the ability to cope with a conflict experience is likely to vary across individuals (Ibáñez, 2014; Morrison and May, 1994). To examine the heterogeneity in the migration response to conflict events, we estimate Equation 1 for men, women, and three age cohorts: children (1-15 years old), adults (16-40 years old), and elderly (over 41 years old).[12]

Table 3 shows that the temporary surge in migration was similar for men and women in the first two months after the event. However, there is one major difference: while migration among men returned to pre-treatment levels in the third month (column 2), our estimates suggest that the migration response of women was more persistent, probably lasting throughout the period of analysis (column 3). In Figure A.10, we present suggestive evidence that, at least during the period of analysis, conflict events significantly changed the sex composition of the migration flow. The female-to-male ratio constantly increased until the end of 1915, when widespread insurgency ceased and the revolution transitioned to local guerrilla movements. Figure A.10 also shows that the sex ratio did not return to pre-revolution levels until the beginning of 1917, when the First Bracero Program was implemented.[13]

Table 3 also shows that there was a stronger and more persistent migration response among children. While we find a similar increase in migration rate for children (column 4) and adults (column 5) in the first month after the event, this effect is not statistically significant for the latter in subsequent months. The point estimates also suggest a return to baseline migration levels among adults in the third month after the event. In contrast, the migration of children increased by 0.07 per thousand inhabitants in the second month after the event—an increase of about 20 to 50 percentage points greater than for prime-age adults and elderly—which persists up to the fourth month. Like for women, the coefficient on the +5 period suggests a permanent increase in migration among children. Based on these results, it appears that conflict events led to a stronger and more persistent migration of mothers and their children. This evidence is in line with previous literature arguing that during the revolution the migration of families increased (Durand, 2016). The shorter response and null permanent effect for men may be due to an increase in enrollment rates into the federal army (revolutionary factions) and consequently a higher mortality among men. Although historical literature has argued that all factions, revolutionary and federal, used compulsory service to fill their ranks (Chávez Leyva, 1998; Knight, 1986*a*,*b*), to our knowledge there are no military records on deaths (or for recruitment) to test whether this mechanism explains the aforementioned differences in migration responses across population groups.

[12]We define the age cohorts based on life expectancy at the time (McCaa, 2003).

[13]This guest-worker program was created to satisfy the demand for agricultural labor force in the American Southwest caused by the participation of the United States in the First World War.

Variation in conflict risk across individuals can also explain heterogeneous migration responses, as armed groups can persecute specific population groups with the aim of expropriating land and assets, separating rebel groups from their support base, or generating fear among the population (Azam and Hoeffler, 2002; Balcells and Steele, 2016; Engel and Ibáñez, 2007; Ibáñez, 2014; Stanley, 1987). In the case of the Mexican Revolution, however, no ethnic group or social class was particularly targeted. The indigenous and non-indigenous populations participated equally in the conflict as well as peasants, industrial workers, middle-class politicians, and land owners (Gonzales, 2002; Knight, 1986*a*). In addition, the Mexican Revolution was not led by a single faction or political party, but rather it was a multi-sided civil war in which various factions united against the federal government, broke apart, and fought against each other throughout the conflict (Dell, 2012; Garcíadiego, 2004). As Table 2 suggests, differences in the economic and strategic value of territory were likely to influence conflict risk rather than individual or household characteristics (Blattman and Miguel, 2010; Kalyvas, 2006).

6. Treatment Channel and Moderating Factors

Episodes of conflict may not necessarily induce migration due to (in)voluntary immobility (Becker, 2022; Lubkemann, 2008)—about 19 percent of the countries that experienced conflict events from 1990 to 2007 did not report forced displacement (Ibáñez, 2014). While previous literature tends to agree that violence is the main channel through which conflict induces migration (see, for example, Apodaca, 1998; Balcells and Steele, 2016; Davenport, Moore and Poe, 2003; Moore and Shellman, 2004; Schmeidl, 1997; Schultz, 1971), it does not provide causal evidence about this relationship. Furthermore, factors such as local economic conditions, immigration restrictions, and social networks can moderate the migration response to conflict and violence (Alvarado and Massey, 2010; Boustan, 2007; Engel and Ibáñez, 2007; Ibáñez, 2014; Zolberg, Suhrke and Aguayo, 1989). In this section, we exploit variation in the intensity and type of violence across conflict events to identify the effect of violence on migration and study the role of migration costs, migrant networks, and land ownership in shaping the migration response.

Violence

Lifetime uncertainty—the perception of uncertainty of survival—influences decision making in fundamental aspects such as investing in education and health, or engaging in childbearing (Becker et al., 2020; Chiovelli et al., 2021; Barro and Friedman, 1977; Nettle, 2010; Padilla-Romo and Peluffo, 2023; Sasson, 2016). Recent literature shows that exposure to violence plays a major role in shaping lifetime uncertainty, as violence increases the risk of premature death and long-lasting health and psychological problems (Aburto et al., 2023). Hence, living in violent contexts makes life shorter and less predictable, which pushes individuals to migrate to places where the returns to human and physical capital are greater in the short- and long-run (Becker, Mukand and Yotzov, 2022; Buggle et al., 2023; Orozco-Aleman and Gonzalez-Lozano, 2018).

To examine whether violence affected the migration response to conflict, we first use war casualties to proxy for violence intensity and focus on municipalities that were first treated with a combat, which represent about 85% of the units that ever experienced conflict.[14] Crucial for our analysis is that 40% of combats did not cause civil or military casualties, allowing us to compare migration responses between units that experienced the same kind of event but with varying degrees of violence. Since combats may have occurred in locations with specific unobservable characteristics, we use units that were eventually first treated with a combat as the control group. Figure 3 shows that violence significantly induced migration, with municipalities where combats caused casualties experiencing a large transitory increase in migration that lasted for about three months. In contrast, the leads and lags for units where combats did not cause casualties are mostly negative and statistically insignificant, confirming that conflicts with low levels of violence are unlikely to induce migration (Morrison and May, 1994; Bohra-Mishra and Massey, 2011). Aburto et al. (2023) also argue that the effects of violence are magnified for populations that experience structural inequalities, such as children and women. The estimates in Figure A.11 confirm that the migration response to violent combats was not only larger but permanent among females.

To provide further evidence on violence as a treatment channel, we use conflict length as an alternative violence metric. Figure A.12 shows that conflict length is positively correlated with the number of casualties, implying that the migration response should be stronger in locations affected by long-lasting conflicts. To test this hypothesis, we classify events into three categories that roughly split ever-treated units into thirds: conflicts that lasted less than one day (33%), 1 to 3 days (32%), and more than three days (35%). Figure 4 shows that in municipalities that experienced conflicts lasting more than three days, migration increases drastically in the month when the event occurs and grows larger over the next two months. Afterward, migration decreases but stabilizes at a level representing a 50% increase over the baseline (0.08)—that is, the migration response to long-lasting conflicts was permanent. Note, however, that the estimated coefficient on the -5+ lag is statistically different from zero, which suggests

[14] Previous literature shows that war casualties reflect violence in contexts of conflict (Aburto et al., 2023; Bohra-Mishra and Massey, 2011; Kondylis, 2010).

the presence of trending confounds in pre-event periods. Following Dobkin et al. (2018), we fit a linear trend based on pre-event period information and then estimate the effect relative to the linear extrapolation. In Figure A.13 we show that our finding holds after controlling for these confound dynamics. In contrast, conflicts that lasted less than one day or between 1 to 3 days had *zero* impact on migration.[15]

While previous descriptive studies have documented the association between violence and migration—see Ibáñez (2014) for a review—our results provide causal evidence that violence impacted the migration response to conflict events during the revolution. Our analysis also shows that different types of violence are likely to have differentiated impacts on the magnitude and persistence of migration.

Migration costs

While refugee migration is influenced by factors outside the control of migrants, in many contexts individuals have some agency in the decision to migrate—that is, they weigh the benefits and costs of leaving and evaluate when to leave (Becker, 2022; Becker, Mukand and Yotzov, 2022). Hence, migration costs may moderate the response to conflict, as individuals facing high costs may choose to delay migration or stay (Engel and Ibáñez, 2007; Ibáñez and Vélez, 2008). Previous research has documented that in contexts of conflict a greater distance between origin and destination reflects larger travel, psychological, and information costs (Morrison and May, 1994; Lozano-Gracia et al., 2010). We follow this literature and estimate Equation 1 for municipalities located at different distances from the US border. We perform this analysis using distance estimates from Woodruff and Zenteno (2007), who compute the distance by train from each municipality to the border according to the railway network that existed in the early 1900s. Note that for municipalities without direct access to railways, the estimates consider the distance to the nearest train station.

Table 4 presents the results of our analysis. We find inconclusive evidence about the impact of conflict in municipalities located less than 200 km from the border. Although the point estimates are large, most coefficients are statistically insignificant (column 2). This finding suggests that conflict may not necessarily induce migration in contexts where the costs of leaving and returning home are low. Similar to our baseline result, we find statistically significant transitory effects in municipalities located 200 to 700 km from the border, with migration increasing by 60 to 80 percent relative to the baseline level in the month when the event occurs and during the following two months (column 3). We also find that

[15] In the Appendix, we present evidence suggesting that in situations where the winner remained, there was a stronger and more permanent migration response, whereas in places where the winner left, there was only a transitory response (see Figure A.14 and Figure A.15).

migration increased more than 100 percent relative to the baseline level in municipalities located more than 700 and up to 1,200 km from the border (column 4). Although migration rates in these municipalities were considerably smaller than in locations closer to the border, our results suggest that the increase in migration—observed from the third month after the event—persisted throughout the period of analysis and potentially after the end of the revolution. For very distant places, municipalities located more than 1,200 km from the border, we find close to zero and statistically insignificant effects.[16]

The migration dynamics described above provide strong evidence that migration costs did not preclude refugee flows, but rather moderated the time of response to conflict. Since we observe refugee flows at the border, one possibility is that our results may reflect traveling times. This is unlikely to be the case, as a journey by train from central Mexico to the border took 45 to 60 hours depending on the route (De Cardona, 1892).[17] Another possibility is that the response time was longer in locations far from the border due to conflict dynamics. For example, conflict events may have affected railway infrastructure in these areas. To test this hypothesis, we estimate Equation 1 for municipalities where, according to the military reports, no infrastructure was damaged or destroyed as a result of conflict events. The results show very similar migration dynamics: the migration response occurs with a three-month delay in municipalities located far from the border (columns 5 to 8). We also find similar results when using the linear distance from the municipality centroid to the nearest border point as an alternative proxy for migration costs (Table A.2).

Our analysis provides causal evidence that the time of response to conflict increases with migration costs. At the time, a third-class ticket from central Mexico to El Paso, Texas cost 30 pesos—roughly equivalent to three months' wage for a laborer—which confirms that migrating from distant municipalities was expensive (De Cardona, 1892). Since our results are unlikely to be driven by travel times or destroyed infrastructure, it is plausible that individuals from municipalities located far from the border had to sell assets to finance the migration process, which could have taken time considering that conflict disrupts markets.

Migrant Networks

A growing body of literature has documented that the presence of migrant networks is fundamental to explaining the migration response to conflict (see Munroe et al. (2023) for a review). Indeed, one

[16]Note that these municipalities are located in the Southeast region, where few insurgency events took place and migration to the United States was almost non-existent before the revolution.
[17]Traveling times are for the routes Mexico City-Brownsville and Mexico City-El Paso, respectively.

possibility is that conflict events only induced migration in locations where the population had a network connection in the United States. To test this hypothesis, we identify the presence of historical and recent networks at the municipality level. We consider that a municipality had access to historical networks if someone had migrated to the United States between 1906 and 1908—that is, two years before the beginning of the revolution.[18] Similarly, we consider that a municipality had access to recent networks if someone had migrated either one or six months before the first conflict was experienced. Figure 5 shows the effect of conflict events across networked and non-networked municipalities. We find that migration rates increased by 0.1-0.12 per thousand inhabitants in the first and second months after the event in municipalities with networks, an effect two times greater than our baseline result (0.05) and about ten times greater than that for non-networked units. This finding holds when we limit the analysis to violent events—that is, conflicts with military or civil casualties.

In Figure A.16 we present estimates across all network categories. Panel A shows that conflict induced similar migration responses in terms of magnitude, timing, and persistence in municipalities with historical or recent migration flows prior to the event. Panel B shows estimates for places with no prior migration. Although the effects are much smaller and less precise (note that the scale of the vertical axis is different in Panel A and Panel B), there is some evidence that locations without recent migration before the conflict experienced a permanent increase in migration: the coefficient on the 5+ period suggests a permanent increase of 0.007-0.010 per thousand inhabitants relative to pre-event levels.

In general, our findings are in line with previous research showing that networks facilitate migration from conflict-affected areas (see, for example, Becker et al., 2021; Buggle et al., 2023; Davenport, Moore and Poe, 2003; Schmeidl, 1997; Spitzer, 2021). Our analysis of high-frequency data, however, provides a key insight into how networks shape migration dynamics in the short run. Our results show that networks were *necessary* to migrate and their presence did not affect the timing or persistence of migration but only its magnitude. This implies that the spatial distribution of pre-existing networks is likely to moderate conflict effects. Figure 6 shows the geographic composition of the flow before the revolution (Panel A) and after the end of the most violent period (Panel B). While there are clear changes in the intensity of migration captured by local migration rates, the migrants' sources remain largely the same. For example, migration from the Center was negligible before the revolution and remained so despite the intense conflict that took place in this region. This finding provides strong evidence of the validity of the diffusion hypothesis of migration in contexts of conflict and violence. This hypothesis states that regardless of the

[18] Pre-revolution immigration data come from Escamilla-Guerrero (2020).

strength of the incentive to migrate, individuals generally will not do so unless one of their close contacts has already migrated (Spitzer and Zimran, 2023, p. 1). We will later present a formal assessment of the impact of the revolution on migration patterns to the United States.

Land Ownership

Another factor that can moderate the migration response to conflict is land ownership. While land can be sold or used as collateral to finance migration, markets are unlikely to fully operate in contexts of conflict, which affects the price and liquidity of land (Ibáñez, 2014; Ibáñez and Moya, 2016). Empirical work has also shown that land owners may face higher opportunity costs from migration, as recovering abandoned land is difficult for returning refugees (Engel and Ibáñez, 2007). Conversely, the incentives to migrate may increase for land owners if their assets are expropriated as a result of conflict (Ibáñez and Vélez, 2008). Conflict may also induce migration among the landless population if land ownership is highly concentrated, particularly in agrarian societies (Boberg-Fazlić, Lampe and Sharp, 2023). Hence, the extent to which land tenure affects the migration response is ambiguous.

One characteristic of pre-revolutionary Mexico was the high concentration of land by a small elite (Chevalier, 1970; Florescano, 1987; Knight, 2016). It is documented that at the time haciendas could have an extension of up to 500 thousand hectares, with some families controlling nearly three million hectares across several estates. As a result of this concentration, 835 families owned more than 90% of the arable land and haciendas became practically the only source of employment in many regions (Manzanilla Schaffer, 1963). In the states of Zacatecas and San Luis Potosí, for example, 76 and 82 percent of the population lived or worked in haciendas, respectively (Easterling, 2012, p. 18-20). Moreover, landowners used coercive mechanisms of debt bondage to keep labor attached to haciendas (Alston, Mattiace and Nonnenmacher, 2009; Moreno-Brid and Ros, 2009).

To examine the role of land ownership in moderating the migration response to conflict, we use data on land concentration in 1900 from Sellars and Alix-Garcia (2018): the share of the population living in large estates by municipality. Figure A.17 confirms that land concentration was particularly high in the West-Central region, with several municipalities having more than 60% of their population living in haciendas or ranches. These data allow us to examine the impact of conflict events in municipalities with different levels of land concentration. In particular, we estimate Equation 1 for municipalities below and above the 75th percentile of the land concentration distribution. In municipalities below the 75th percentile, on average, 10% of the population lived in large estates and private holdings had an extension of 50 hectares, while in municipalities above this threshold, 60% of the population lived in estates of 13-15.5 thousand hectares (Sellars and Alix-Garcia, 2018).

Figure 7 shows that conflict events had no effect on migration in municipalities with high land concentration. In contrast, in municipalities with low land concentration, we observe a large increase in migration rates of 0.05-0.09 per thousand inhabitants, representing an increase of 50 to 90% over the baseline (0.099). While our finding supports the argument that land ownership facilitates migration from areas affected by conflict (Bohra-Mishra and Massey, 2011), it also highlights that the structure of land ownership is likely to moderate the magnitude of the migration response. A more equal land ownership structure reflects stronger property rights and more efficient rule of law, which increases the liquidity of land and reduces opportunity costs from migration for relatively small landowners (Chernina, Dower and Markevich, 2014; Ibáñez, 2014).

Information and Transportation

Local conditions can also influence the migration response to conflict (Becker, 2022; Becker, Mukand and Yotzov, 2022; Munroe et al., 2023). For example, the decision to leave may critically depend on the access to information and the availability of transportation infrastructure. To examine the role of information and transportation in moderating the migration response to conflict, we collect information on the telegraph (Mendoza Vargas, 2014) and railway network (Woodruff and Zenteno, 2007) by the eve of the Mexican Revolution. The access to telegraphs may have provided information about the direction of the revolution and facilitated communication with migrant networks in the United States. Similarly, the access to railroads may have enabled them to flee more promptly. Figure A.18 shows that although the telegraph and railway networks reached most regions of the country, many municipalities did not have direct access to communication and/or transportation infrastructure. This allows us to identify the municipalities that had either a telegraph office or train station and those that did not.

In Figure 8 we present estimates for the above-mentioned groups of municipalities. Note that we limit the analysis to municipalities where conflict events did not cause damage to civil infrastructure. We find significant increases in migration rates for both groups, which suggests that access to information or transportation was not a necessary condition for individuals to flee northward. Further, we do not detect any pretrends to migration in places with infrastructure, suggesting that there was no anticipatory response. However, there is one difference between the two groups: the migration response is slightly delayed for places with no infrastructure. While our baseline results find an increase in migration one month after the event, for municipalities with no infrastructure, we first detect an increase in migration two months after the event.

7. Did the revolution change migration patterns?

Our finding that insurgency events only caused a short-run increase in migration is at odds with recent literature arguing that the Mexican Revolution is key to understanding Mexico-US migration, as it kick-started mass migration and shaped the patterns that persisted throughout the twentieth century (Hernández, 2022). Indeed, migration networks may have formed during the revolution and then encouraged migration decades later (Woodruff and Zenteno, 2007; McKenzie and Rapoport, 2010).

It is possible that our approach misses the "true" impact of the revolution on migration due to measurement error in our immigration data, for instance. To test whether the Mexican Revolution altered migration to the United States in the long run, we compare pre- and post-revolution migration rates. If the revolution changed the geography of migration or the relative importance of migrant sources, then there should be a low correlation in migration rates across time. To measure this correlation, we calculate migration rates by municipality between January and October 1910—before the revolution started—and compare them to migration rates a decade later between January and October 1920—years after insurgency ceased. To keep outcomes consistent over time, we percentile rank the migration rate in each decade, after keeping municipalities with non-zero migration.

Figure 9 shows a strong persistence of migration between 1910 and 1920. The correlation between the 1910 percentile rank and the 1920 percentile rank is 0.83, implying that high-sending municipalities before the Mexican Revolution remained high-sending municipalities for years after the conflict ended. In sum, despite the short-term exodus out of the country caused by the revolution, our analysis suggests that the geography of migration or the relative importance of migrant-sending municipalities did not substantially change.

8. Conclusion

In this paper, we use high-frequency data to estimate whether conflict events during the Mexican Revolution affected migration to the United States. We find that the impact of conflict was large but temporary: monthly migration rates at the municipality level increased by 60 percent in the first few months after the event. However, migration rates reverted back to pre-conflict levels after five months. At the same time, this result masks heterogeneity in the impact of conflict on migration. We find larger and more persistent effects for women and children than for men. We also show that violence was the main treatment channel, with variation in the intensity and nature of violence explaining the magnitude and persistence of the migration response. Our results confirm that more violent events lead to larger changes in migration rates, which persisted throughout the period of analysis. In addition, we provide new insights on how migration costs, migrant networks, and land ownership shape the decision to migrate in contexts of conflict. We show that these factors affect different dimensions of the migration response: migration costs moderate the timing while networks and land ownership moderate the magnitude of the response.

We know from anecdotal and indirect evidence that migration flows from Mexico to the United States increased during the Mexican Revolution. However, we are the first to provide direct, quantitative evidence on the impact that this conflict had on the scale, composition, and persistence of the migration response at the local level. Most studies on forced migration use data varying annually (or at a lower frequency) to examine conflicts characterized by the persecution of a specific ethnicity, religious group, or social class. The Mexican Revolution provides an ideal setting for extending our understanding of the relationship between conflict and migration. In particular, our use of high-frequency data allows us to more precisely identify the migration dynamics caused by conflict events in a context where conflict risk was relatively homogeneous across population groups, while also improving our historical understanding of the Mexican Revolution and the Mexico-US migration history.

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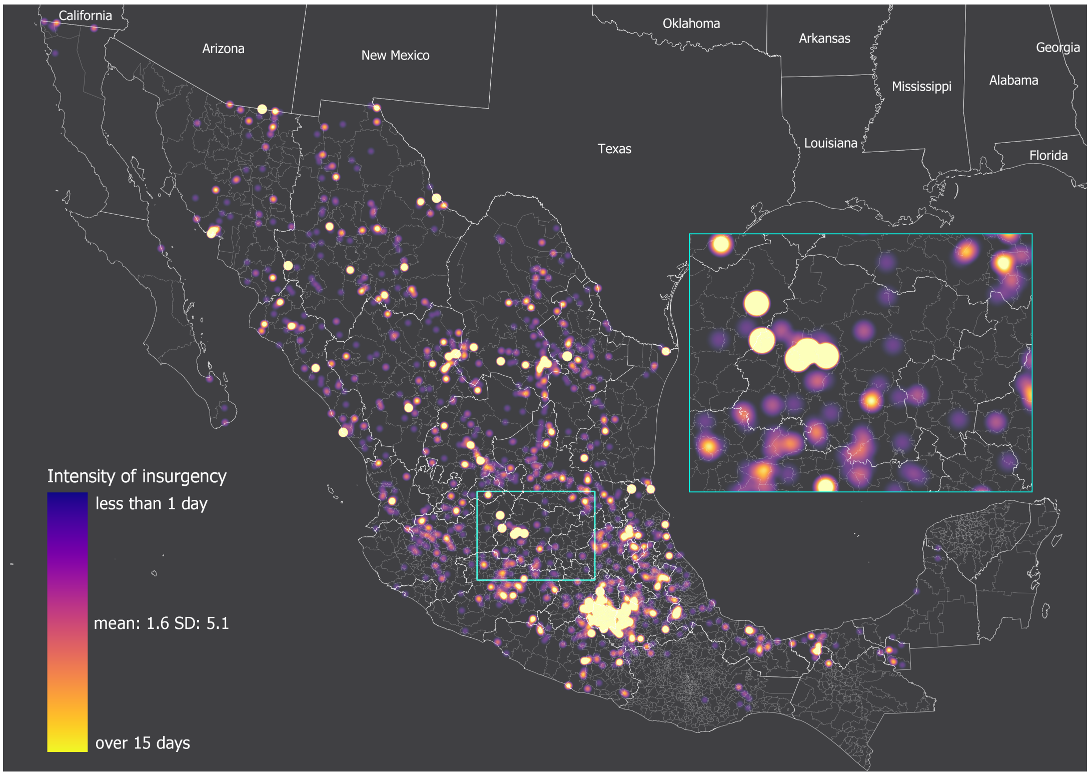
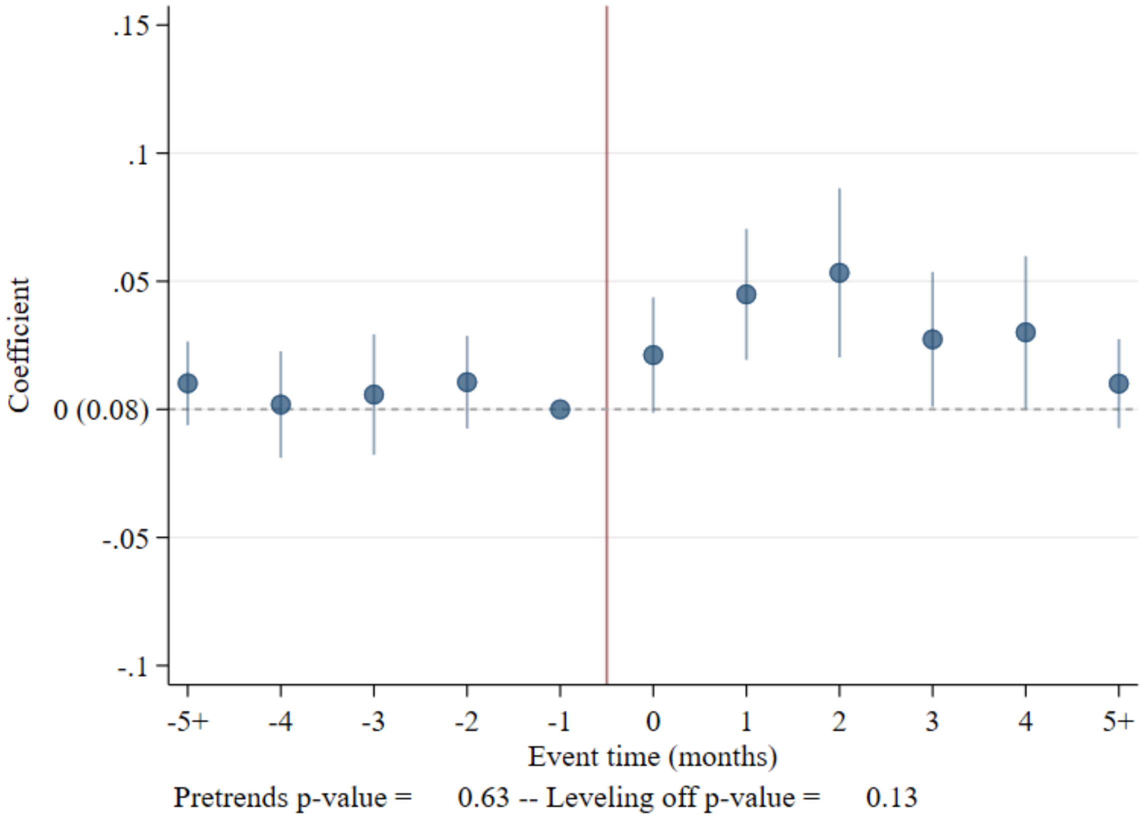
Figures and Tables

Figure 1: Spatial Distribution of Insurgency Events



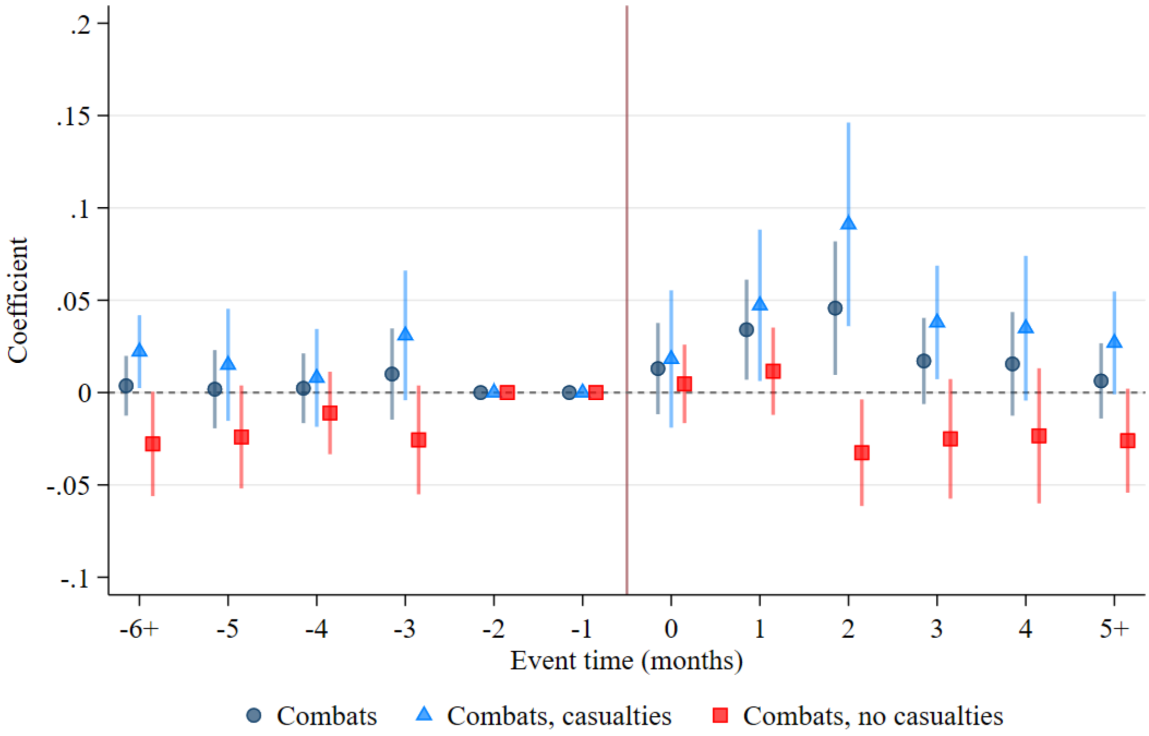
Source: Military History of the Mexican Revolution (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983).
Notes: The map shows the spatial distribution and intensity (days of conflict) of 2,411 insurgency events that occurred from November 1910 to December 1915. We assign latitude and longitude coordinates to each event and use a 15 km radius buffer. Bright colors denote longer events. Armed conflicts occurred across the country with the exception of the Yucatan Peninsula and some states in Southern Mexico. Although intense conflicts occurred in all regions, events lasting more than 15 days concentrated in Mexico City—the seat of the federal government—and nearby states.

Figure 2: The Effect of Conflict on Migration



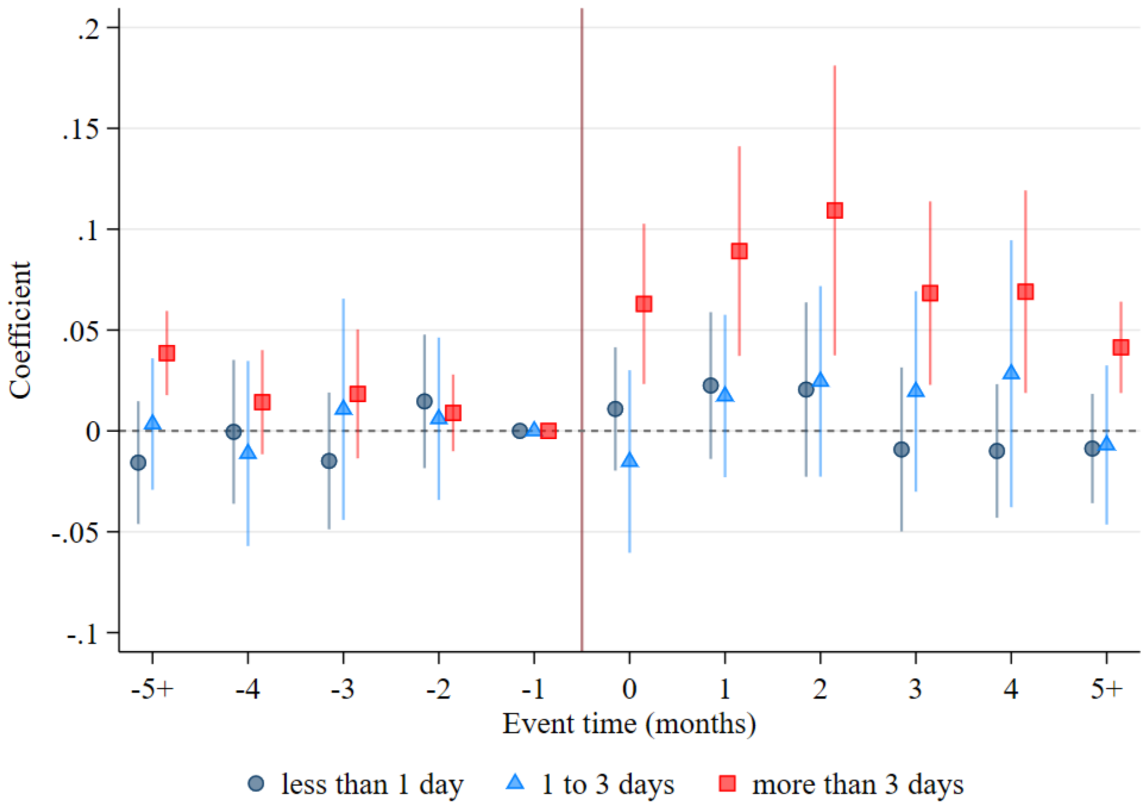
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that violence induced emigration in the first three months after the event. During this period monthly emigration rates increased more than 50 percent. The average value of the outcome of interest in t-1 (baseline omitted period) is 0.08. The control group consists of not-yet and never-treated units. Standard errors are clustered at the municipality level. pretrends: Wald test for all the pre-event coefficients being equal to 0. Leveling off: Wald test for the last post-event coefficients (4 and 5+) being equal. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure 3: Conflict Mortality and Migration



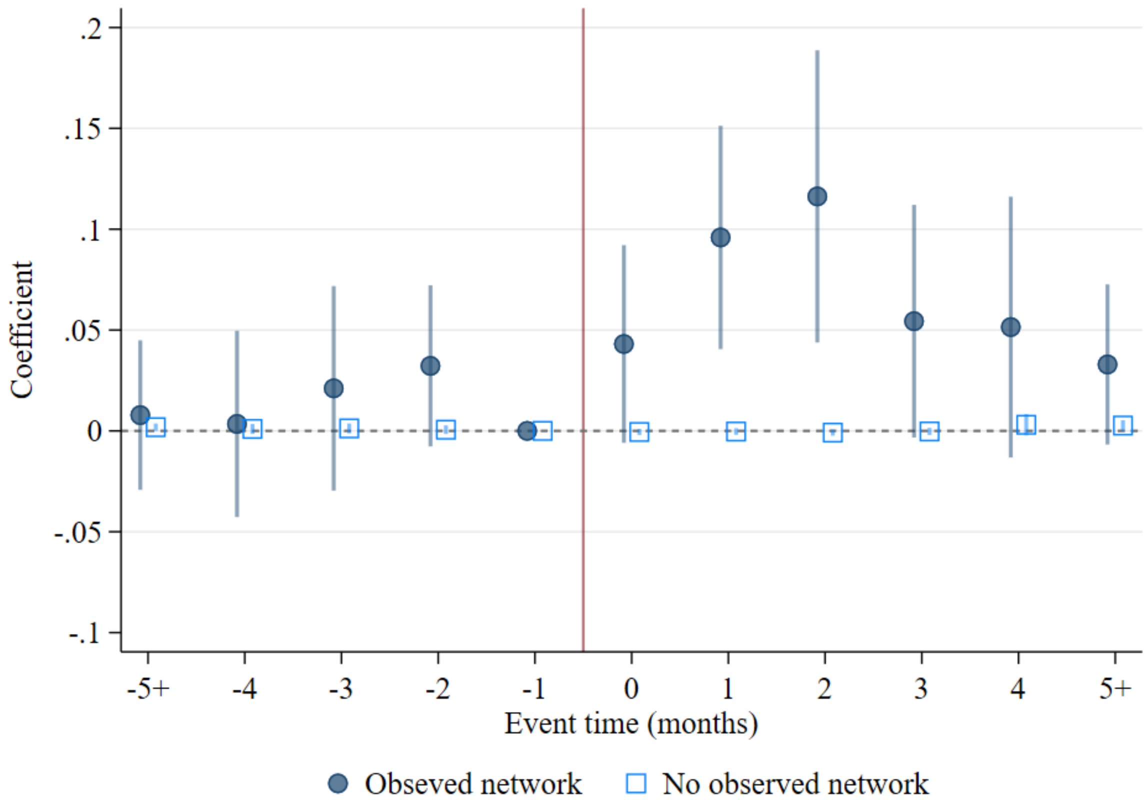
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that violence, proxied by conflict mortality, significantly induced migration, with municipalities where combats caused casualties experiencing a large transitory increase in migration that lasted for about three months. In contrast, the leads and lags for units where combats did not cause casualties are mostly negative and statistically insignificant. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure 4: Conflict Length and Migration



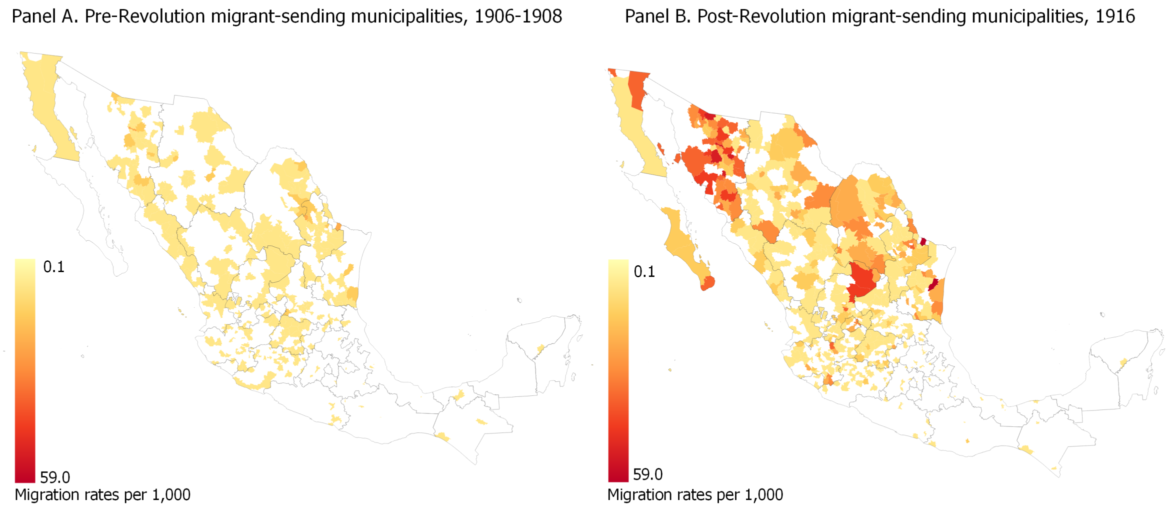
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that monthly migration rates increased permanently only in municipalities that experience more than 3 days of conflict during the period of analysis—on average, treated units experienced two days of conflict. Note that estimated coefficient on time period (-5+) is statistically different from zero, suggesting the presence of confound dynamics in pre-event periods. In Figure A.13 we show that our finding holds after controlling for confound dynamics. The control group consists of not-yet and never-treated units. Standard errors are clustered at the municipality level. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure 5: Networks and Migration



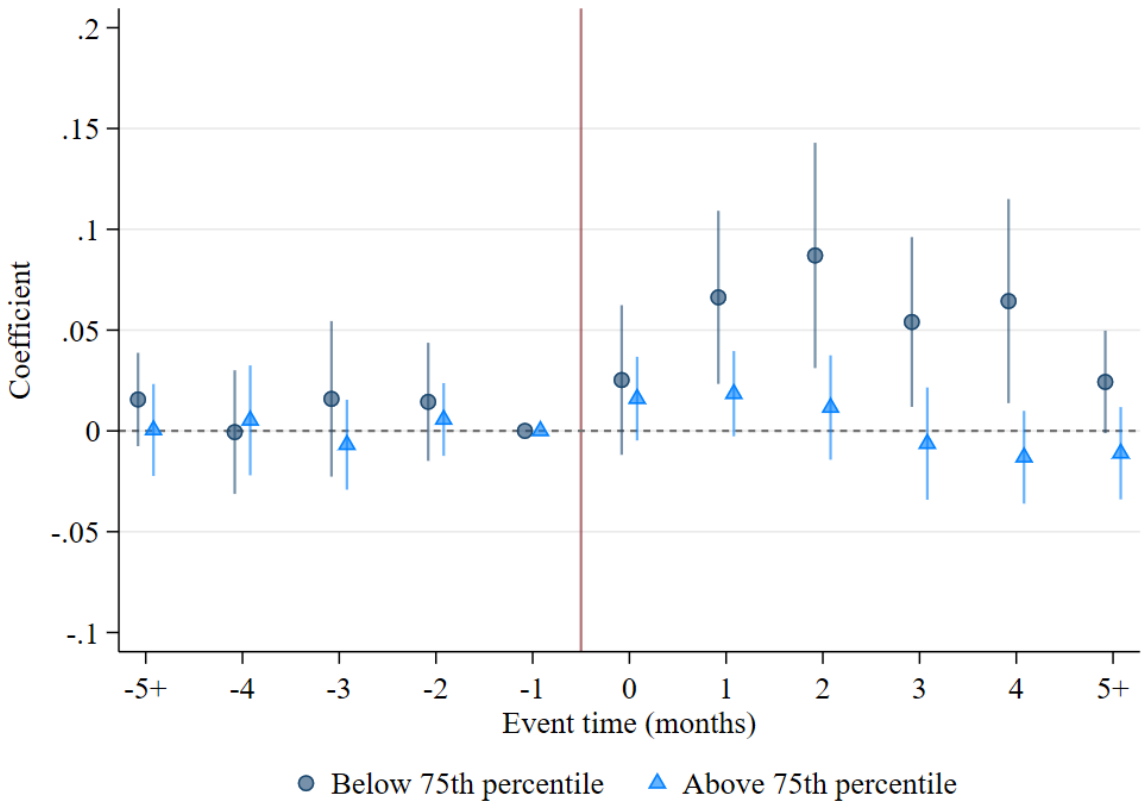
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that the access to networks mediated the migration response to insurgency events. Changes in monthly migration rates are about 10 times greater in municipalities with access to migrant networks. We identify municipalities with access to networks as those with migration flows during between January and November 1910, or between 1906 and 1908 Escamilla-Guerrero (2020). The control group consists of not-yet and never-treated units. Standard errors are clustered at the municipality level. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure 6: Mexican Migration to the United States



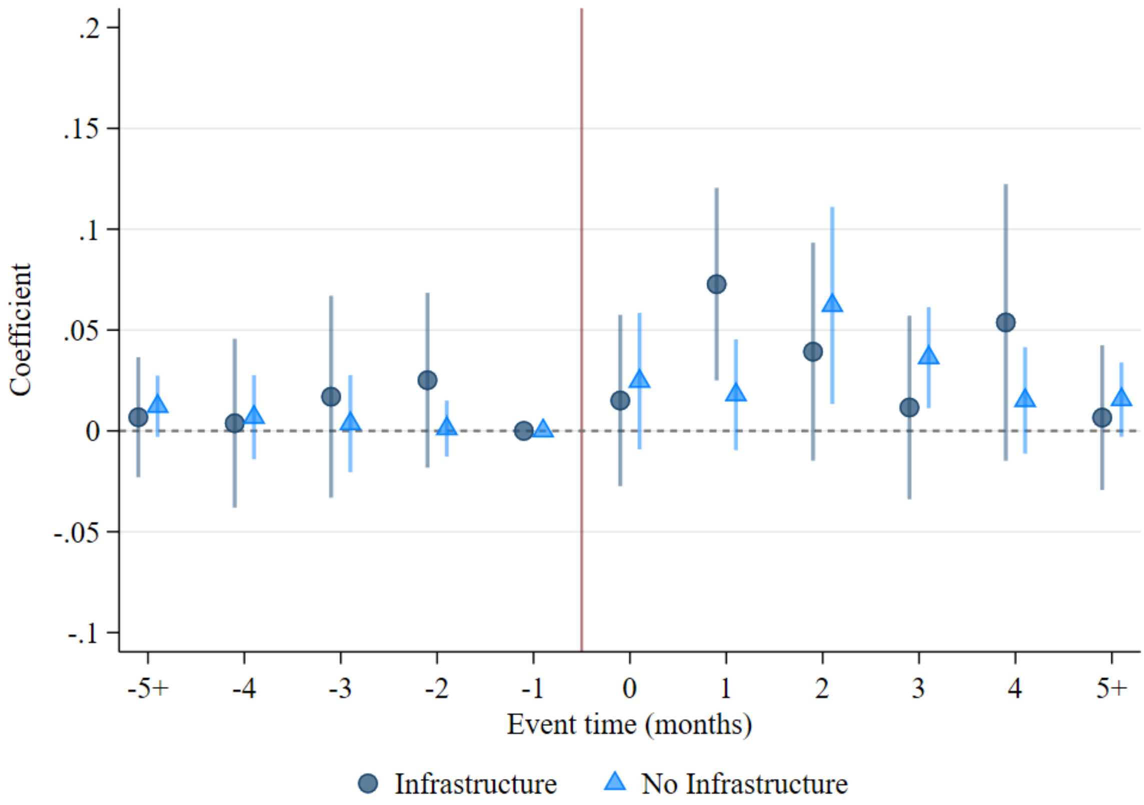
Source: Mexican Border Crossing Records.
Notes: The map shows (average)annual migration rates per 1,000 inhabitants by municipality.

Figure 7: Land Concentration and Migration



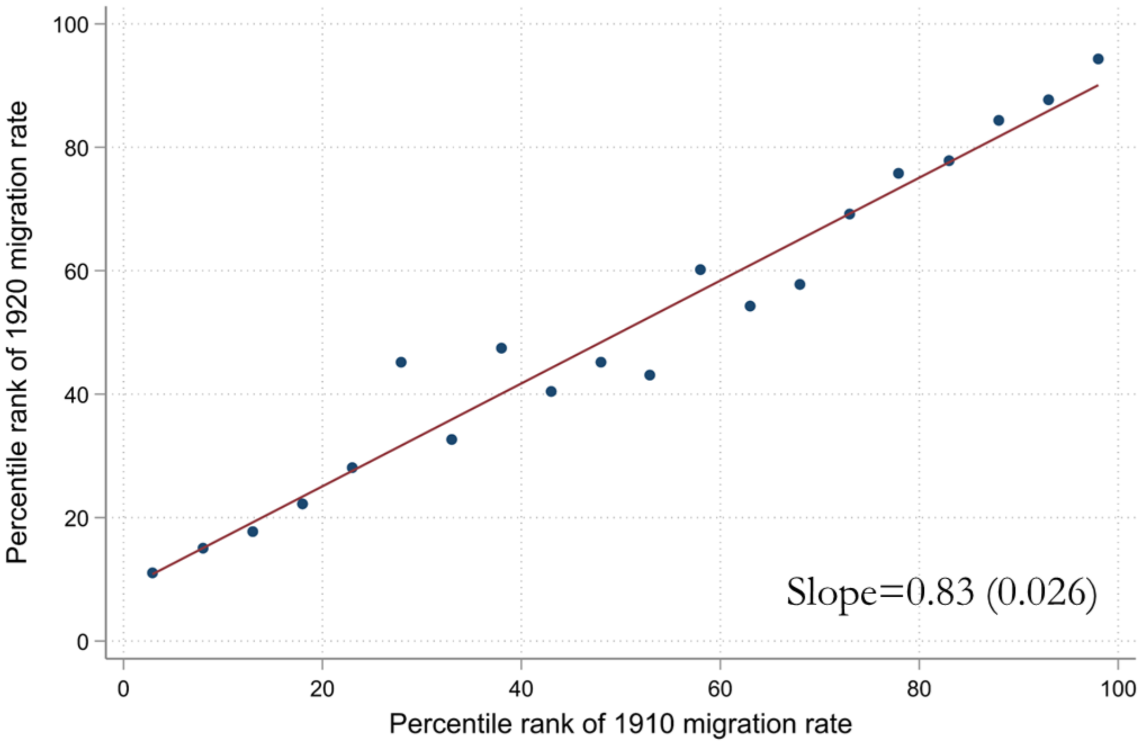
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that land concentration mediated the migration response to insurgency events, with monthly migration rates only increasing in municipalities with an intermediate land concentration. Land concentration is measured as the share of population living in large estates (haciendas or ranchos), which proxies for the share of population without land. The average land extension of haciendas and ranchos was 15,500 ha and 13,500 ha, respectively, while the average private property holding was about 50 ha. Data on land concentration comes from Sellars and Alix-Garcia (2018). The control group consists of not-yet and never-treated units. Standard errors are clustered at the municipality level. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure 8: Information/Transportation and Migration



Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that the access to communication (telegraphs) and/or transportation (trains) infrastructure moderated the timing of the migration response. In municipalities with no train stations or telegraph offices, the migration response was delayed by one time period relative to municipalities with communication and/or transportation infrastructure. The estimates are conditional on no damage to civil infrastructure reported. We do not observe statistically significant changes on migration rates in municipalities where insurgency damaged civil infrastructure (estimates available upon request). The control group consists of not-yet and never-treated units. Standard errors are clustered at the municipality level. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure 9: The stability of migration between 1910 and 1920



Source: Mexican Border Crossing Records between January and October 1910, and January and October 1920.
Notes: The figure shows the correlation of the percentile rank of migration rates by municipality between 1910 and 1920 for municipalities that had non-zero migration. Migration rates are calculated by municipality in both 1910 and 1920, and then percentile ranked within their respective year. The slope reflects the correlation.

Table 1: Characteristics of Insurgency Events

	Full Sample	Shooting	Combat	Siege	Battle
Mean length (days)	1.61	1.08	1.30	16.58	19.88
Mean military casualties	355.15	36.32	357.02	1,806.04	3,472.92
Damage to infrastructure (share)	0.09	0.05	0.09	0.21	0.52
Occupation of territory (share)	0.14	0.04	0.15	0.29	0.24
Observations	2,411	361	2,001	24	25

Source: Military History of the Mexican Revolution (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983).
Note: The table presents characteristics of insurgency events during the Mexican Revolution by military category. The average event lasted 1.6 days and had 355 military casualties. These figures vary across military categories, which capture differences in the intensity of violence.

Table 2: Municipality Characteristics by Treatment Status

	1	2	3	4	5
	All units	Ever with Conflict	Never with Conflict	Difference	Conditional Difference
Pre-revolution migration rate	0.04	0.09	0.02	0.07***	0.02
Population (thousands)	19.48	10.55	22.13	−11.59***	3.24***
Sex ratio	1.02	1.01	1.03	−0.02***	−0.00
Share of illiterate population	0.54	0.50	0.55	−0.05***	−0.01*
Number of large estates	16.14	23.70	13.89	9.81***	6.48***
Share of population in large estates	0.21	0.33	0.17	0.16***	0.00
Altitude (m)	1,327.75	1,328.15	1,327.63	0.52	−44.53
Distance to the US border (km)	796.83	606.99	853.22	−246.23***	−4.79
Distance to nearest train station (km)	150.24	84.95	169.63	−84.68***	−1.06
Distance to the US border by train (km)	891.79	684.48	953.37	−268.89***	−3.38
Share with train station	0.23	0.43	0.17	0.26***	0.18***
Share with telegraph office	0.15	0.27	0.11	0.16***	0.15***
Observations	2,787	638	2,149	2,787	2,787

Source: Conflict data are from the Military History of the Mexican Revolution (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983). Average monthly migration rates are for the period January 1910 to June 1910. Estates (haciendas and ranchos) and population data are for 1900 (Sellars and Alix-Garcia, 2018). Distance by train and railways data are for the early 1900s (Woodruff and Zenteno, 2007). Telegraphs data are for ca. 1895 (Mendoza Vargas, 2014).
Note: The table presents means of municipality characteristics. We estimate differences (column 4) and differences conditional on state fixed effects (column 5) between ever with conflict and never with conflict municipalities. * = Significant at 10% level; ** = Significant at 5% level; *** = Significant at 1% level.

Table 3: The Impact of Conflict on Migration by Population Group

	1	2	3	4	5	6
	Baseline	Males	Females	Children	Prime Age	Elderly
Leads						
-5+	0.010	0.016	0.004	0.007	0.014	0.004
	(0.010)	(0.012)	(0.011)	(0.014)	(0.013)	(0.014)
-4	0.002	−0.003	0.006	0.003	0.004	−0.016
	(0.013)	(0.015)	(0.014)	(0.019)	(0.016)	(0.018)
-3	0.006	0.004	0.007	−0.007	0.014	0.011
	(0.014)	(0.015)	(0.017)	(0.016)	(0.024)	(0.015)
-2	0.011	0.017	0.004	0.016	0.006	0.003
	(0.011)	(0.014)	(0.014)	(0.021)	(0.013)	(0.019)
Lags						
0	0.021	0.025	0.016	0.021	0.018	0.034
	(0.014)	(0.017)	(0.013)	(0.021)	(0.015)	(0.027)
1	0.045***	0.042**	0.047**	0.055**	0.047**	0.017
	(0.016)	(0.018)	(0.019)	(0.026)	(0.020)	(0.019)
2	0.053***	0.044**	0.064***	0.074***	0.040	0.049*
	(0.020)	(0.019)	(0.024)	(0.028)	(0.025)	(0.027)
3	0.027*	0.022	0.034*	0.059**	0.001	0.030
	(0.016)	(0.015)	(0.019)	(0.026)	(0.016)	(0.025)
4	0.030*	0.019	0.040*	0.052*	0.015	0.012
	(0.018)	(0.018)	(0.020)	(0.028)	(0.021)	(0.019)
5+	0.010	0.001	0.020*	0.024*	0.002	−0.005
	(0.011)	(0.012)	(0.012)	(0.014)	(0.014)	(0.015)
Observations	199,944	199,800	199,800	199,800	199,800	199,728
R-squared	0.409	0.356	0.326	0.274	0.367	0.221
Outcome t-1	0.08	0.09	0.08	0.08	0.10	0.07
Pretrends	0.59	0.40	0.99	0.60	0.86	0.27
Leveling off	0.11	0.14	0.17	0.19	0.37	0.21

Source: 1910 Population Census of Mexico, Mexican Border Crossing Records, and Military History of the Mexican Revolution (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983).

Note: The table shows that conflict induced migration in the first three months after the event (lag 0). The age cohorts analyzed are 1-15 (children), 16-40 (prime age adults), and over 40 years (elderly). Life expectancy at birth in Mexico was about 30 years (McCaa, 2003, p. 392). Population data by age cohort and sex is not reported for some municipalities. The control group consists of not-yet- and never-treated units. Outcome t-1 is the average value of the outcome of interest in the omitted period. Pretrends: Wald test p-value for all the pre-event coefficients being equal to 0. Leveling off: Wald test p-value for the last post-event coefficients (4 and 5+) being equal. * = Significant at 10% level; ** = Significant at 5% level; *** = Significant at 1% level. Standard errors clustered at the municipality level in parentheses.

Table 4: The Impact of Conflict on Migration by Distance to the Border

	1	2	3	4	5	6	7	8
	Distance by Train (km)				**Distance by train (km) - No Damage**			
	0 - 200	201 - 700	701 - 1200	Over 1200	0 - 200	201 - 700	701 - 1200	Over 1200
Leads								
-5+	−0.010	0.021	0.003	−0.005	0.124	0.015	0.003	−0.005
	(0.160)	(0.018)	(0.003)	(0.003)	(0.113)	(0.024)	(0.003)	(0.003)
-4	−0.056	0.006	0.002	0.000	0.058	0.006	0.002	0.000
	(0.198)	(0.023)	(0.002)	(0.004)	(0.198)	(0.031)	(0.003)	(0.004)
-3	−0.013	0.013	0.000	0.004	0.206	0.003	−0.000	0.004
	(0.256)	(0.023)	(0.001)	(0.004)	(0.307)	(0.030)	(0.002)	(0.004)
-2	0.083	0.012	0.002	0.004	0.164	0.008	0.002	0.004
	(0.124)	(0.024)	(0.002)	(0.004)	(0.150)	(0.032)	(0.003)	(0.004)
Lags								
0	−0.063	0.063**	−0.001	−0.003	−0.126	0.076**	−0.001	−0.003
	(0.181)	(0.029)	(0.001)	(0.003)	(0.230)	(0.039)	(0.001)	(0.003)
1	0.412*	0.067**	0.001	−0.006	0.362**	0.081**	0.002	−0.006
	(0.214)	(0.031)	(0.002)	(0.004)	(0.178)	(0.041)	(0.002)	(0.004)
2	0.368	0.093**	0.003	−0.007	0.377*	0.105*	0.003	−0.007
	(0.231)	(0.044)	(0.004)	(0.004)	(0.204)	(0.058)	(0.004)	(0.004)
3	0.214	0.040	0.005**	−0.001	0.352	0.029	0.004*	−0.001
	(0.243)	(0.031)	(0.002)	(0.004)	(0.226)	(0.034)	(0.002)	(0.004)
4	0.382	0.031	0.003	−0.002	0.664	0.020	0.004	−0.002
	(0.324)	(0.029)	(0.003)	(0.005)	(0.412)	(0.038)	(0.004)	(0.005)
5+	0.015	0.014	0.006***	0.001	0.055	0.016	0.006**	0.001
	(0.157)	(0.020)	(0.002)	(0.004)	(0.161)	(0.027)	(0.003)	(0.004)
Observations	156,024	171,648	179,712	154,584	155,448	166,608	175,968	154,584
R-squared	0.398	0.381	0.363	0.358	0.390	0.378	0.363	0.358
Outcome t-1	0.78	0.11	0.004	0.000	0.65	0.11	0.005	−0.005
Pretrends	0.95	0.59	0.52	0.02	0.75	0.93	0.52	0.03
Leveling off	0.13	0.37	0.19	0.45	0.07	0.85	0.27	0.43

Source: 1910 Population Census of Mexico, Mexican Border Crossing Records, and Military History of the Mexican Revolution (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983).

Note: The table shows that migration costs, measured as distance to the border by train, moderated the timing of the response to conflict. We use distance estimates are from Woodruff and Zenteno (2007). The control group consists of not-yet- and never-treated units. Outcome t-1 is the average value of the outcome of interest in the omitted period. Pretrends: Wald test p-value for all the pre-event coefficients being equal to 0. Leveling off: Wald test p-value for the last post-event coefficients (4 and 5+) being equal. * = Significant at 10% level; ** = Significant at 5% level; *** = Significant at 1% level. Standard errors clustered at the municipality level in parentheses.

Appendix

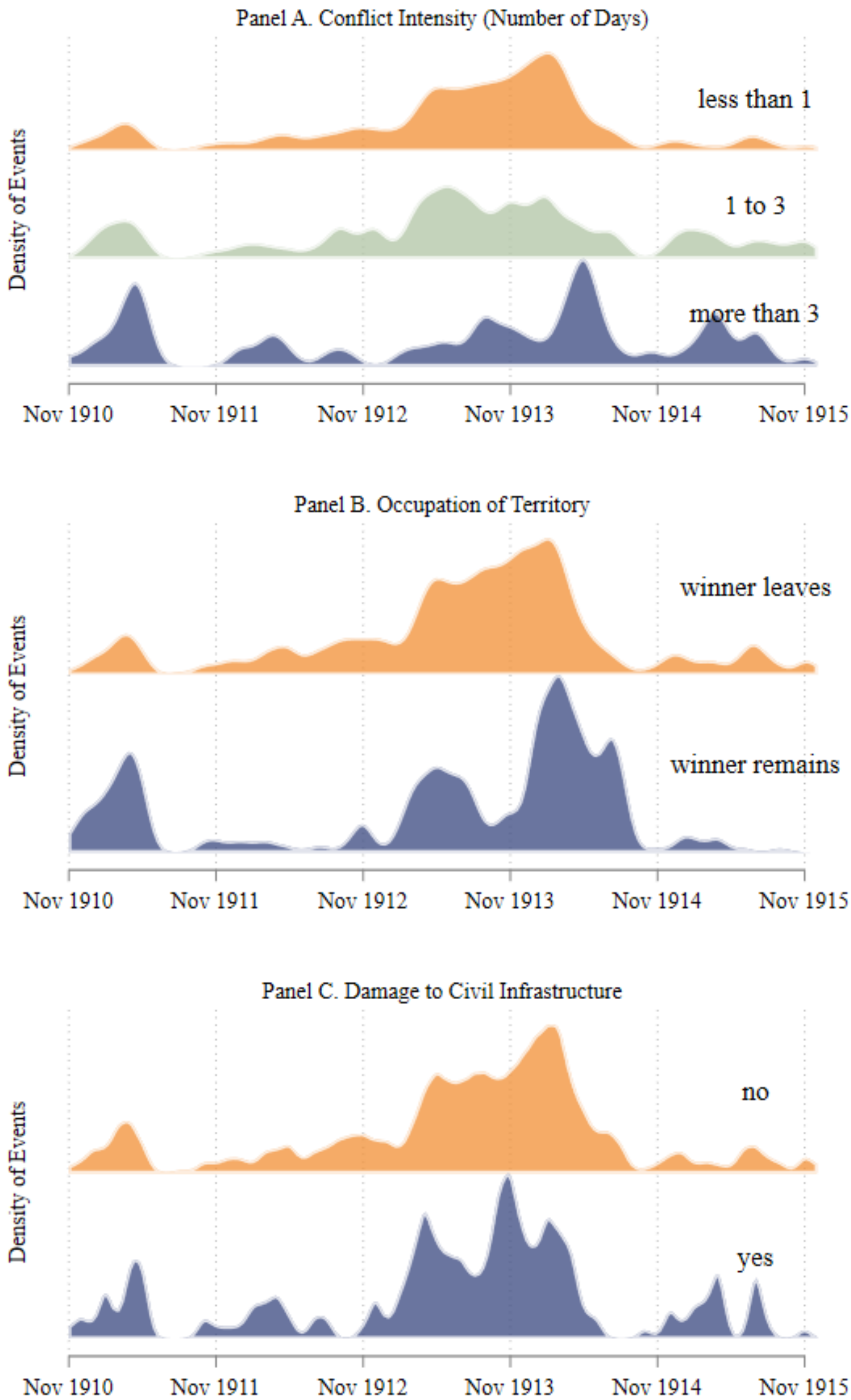
Figure A.1: Abstract of military report

Combat in Santa Barbara (29-30 March 1911)

Knowing that the revolutionaries had occupied Santa Barbara (27 km southwest of Parral, Chihuahua), the Lieutenant Colonel Arizmendi sent 5 officers and 65 soldiers of the 7th Regiment and 18 members of the Rural Army to this place on March 29. Upon its arrival, this force fought against the revolutionaries who were in the mountains that surrounded the town. After fighting all day, the revolutionaries moved into the town. The fighting continued on March 30, when the revolutionaries were defeated and ran away, leaving in the field 7 dead and 27 horses. The federal army had 4 death and 8 wounded (F. 1146, Exp. 62, AHSDN).

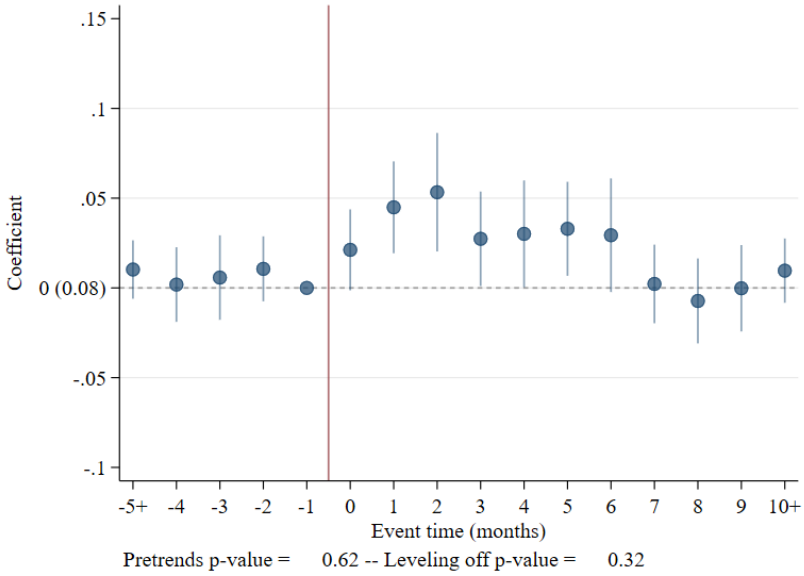
Source: Military History of the Mexican Revolution (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows an example of the digitized military reports. Each report contains precise information on the place where the event occurred, which allows us to assign latitude and longitude coordinates to each location and classify them into municipalities. Diverse features of the event are also systematically reported, including the length of the conflict (days or hours), the number of military casualties, whether civil infrastructure was damaged during the event (railways, telegraph offices, bridges, and town halls are the most common cases), whether the winner remained in the conflict's location, and the winning faction.

Figure A.2: Distribution of Insurgency Events Over Time



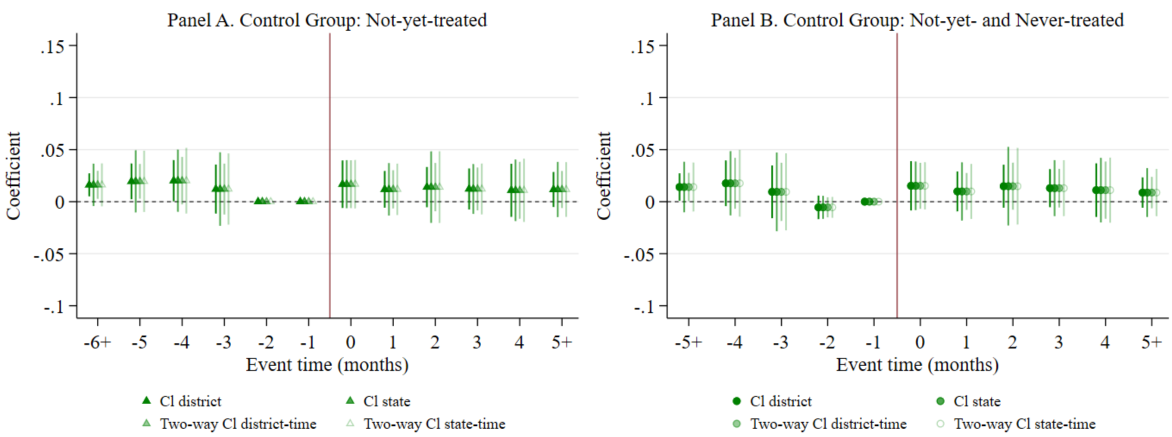
Source: Military History of the Mexican Revolution (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows the density distribution of insurgency events during the period of analysis. Although the bulk of the events occurred during the second and third year of the revolution, the nature of the events varied significantly in terms of length (number of days), occupation of territory (whether the winner leaves or remains in the conflict's location), and damage to civil infrastructure (whether infrastructure such as bridges, telegraph offices, town halls, or train stations were damaged or destroyed during the event).

Figure A.3: The Effect of Insurgency on Migration. Alternative time window



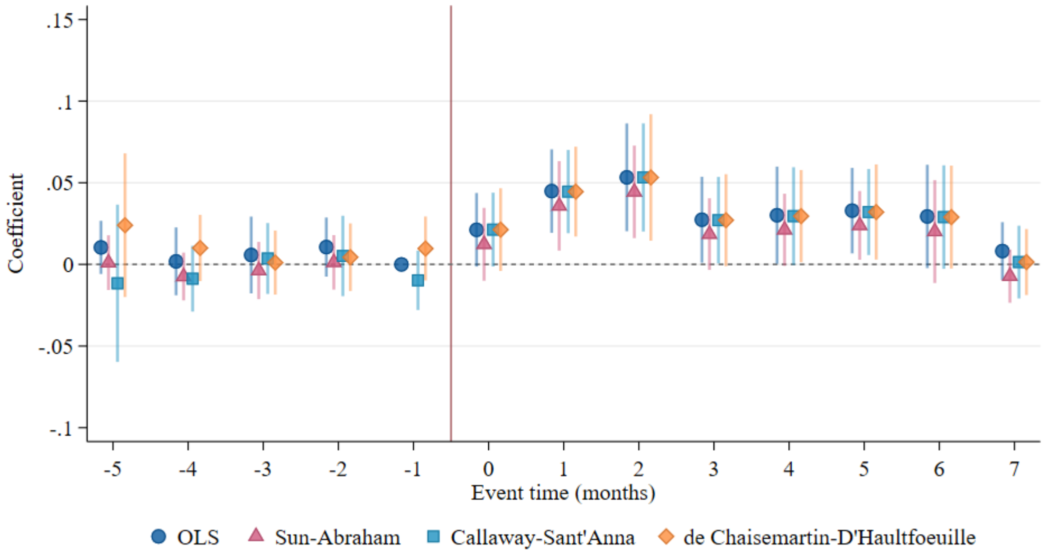
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that the main result holds when the time window is expanded. Standard errors are clustered at the municipality level. pretrends: Wald test for all the pre-event coefficients being equal to 0. Leveling off: Wald test for the last post-event coefficients (5+) being equal. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.4: Spillover Effects



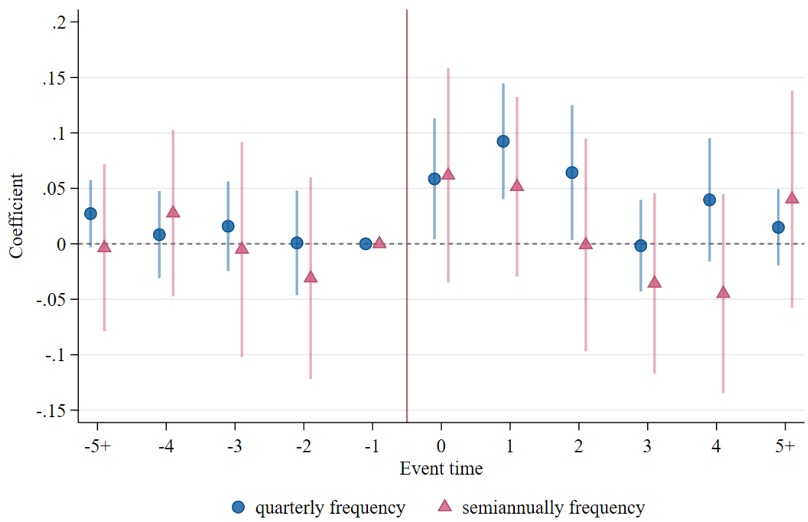
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows the absence of spillover effects. We assign treatment to all municipalities belonging to the same district using as reference the time period when the first municipality was first treated. This approach assumes that spillovers were local and therefore experienced within districts only. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.5: The Effect of Insurgency on Migration. Estimates robust to treatment effects heterogeneity



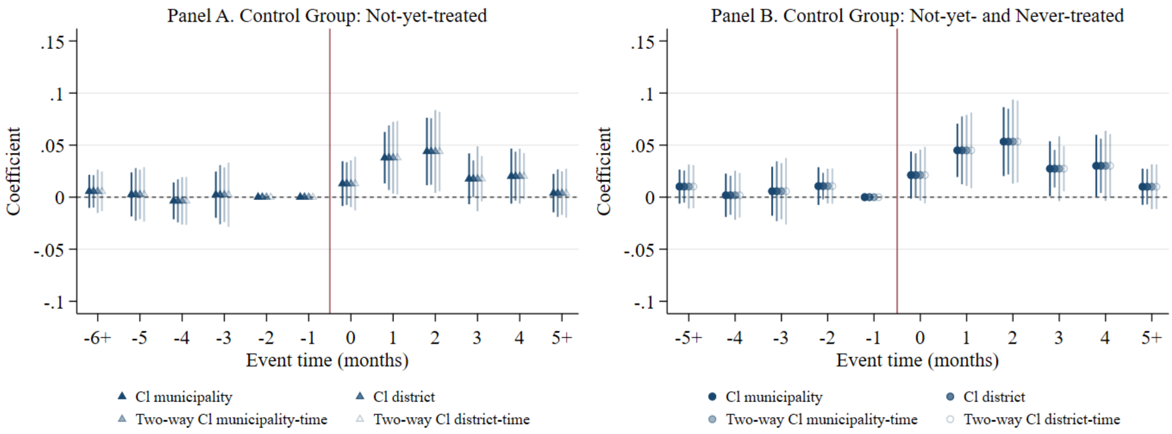
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that our baseline results (OLS) hold after correcting for treatment effects heterogeneity. We find very similar results when implementing the estimators proposed by Sun and Abraham (2021), Callaway and Sant'Anna (2021), and de Chaisemartin and D'Haultfoeuille (2020). Markers represent weighted average of cohort-specific dynamic treatment effect estimates (CATT), with weights corresponding to the cohort share estimates. For Sun-Abraham estimates, lines indicate 90% pointwise confidence intervals. For Callaway-Sant'Anna and de Chaisemartin-D'Haultfoeuille estimates, lines indicate 90% confidence intervals that are valid for the entire path of dynamic effects.

Figure A.6: The Effect of Insurgency on Migration. Alternative data frequency



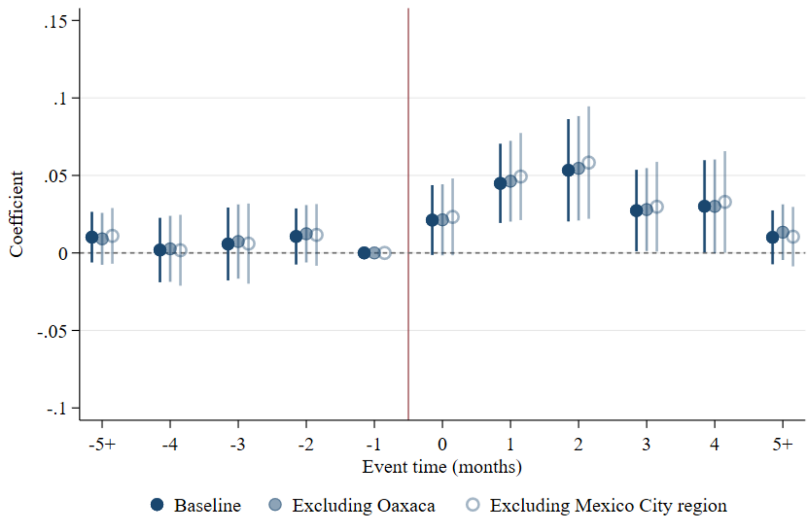
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure highlights the importance of using high-frequency data to more precisely identify the migration dynamics caused by conflict events. Although less precisely estimated, we continue observing similar migration dynamics with data varying quarterly (every 3 months). However, no dynamics are observed with data varying semiannually (every six months). Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.7: Alternative Control Group



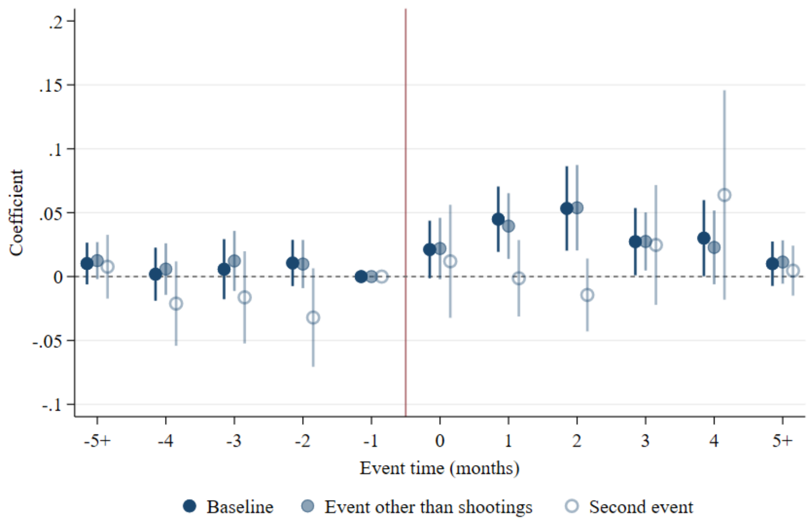
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that insurgency induced emigration in the first two months after the event. This finding is robust to using different control groups and clustering standard errors at different levels. We omit leads 1 and 2 to avoid underidentification when there are no never-treated units in the control (see Borusyak, Jaravel and Spiess, 2021). Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.8: Control Group Composition and Regional Migration Responses



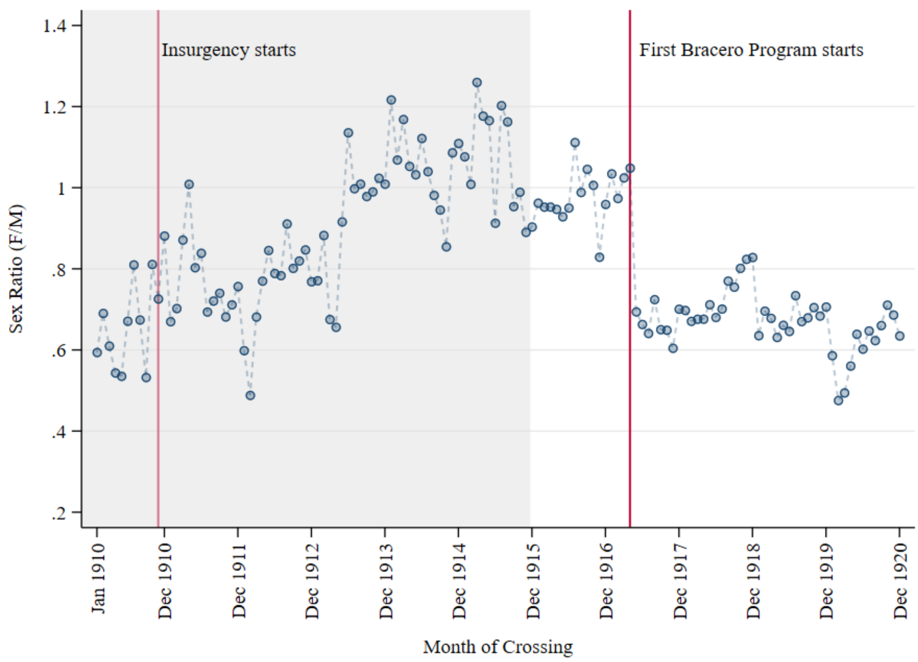
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: We perform two robustness checks to our main results. First, we exclude the state of Oaxaca from the analysis. In 1910, about 40% of the municipalities belonged to this state (see Figure A.19), with the great majority not experiencing violence (see Figure 1). This implies that municipalities in Oaxaca will be over-represented in the control group, which could be a source of bias. Second, we exclude the Mexico City region. Figure 1 also shows that conflicts lasting more than 15 days concentrated in Mexico City and neighboring states (Mexico and Morelos). It is possible that our findings could be importantly influenced by the characteristics of the migration response in this region. The control group consists of not-yet and never-treated units. Standard errors are clustered at the municipality level. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.9: Alternative Treatment Assignment



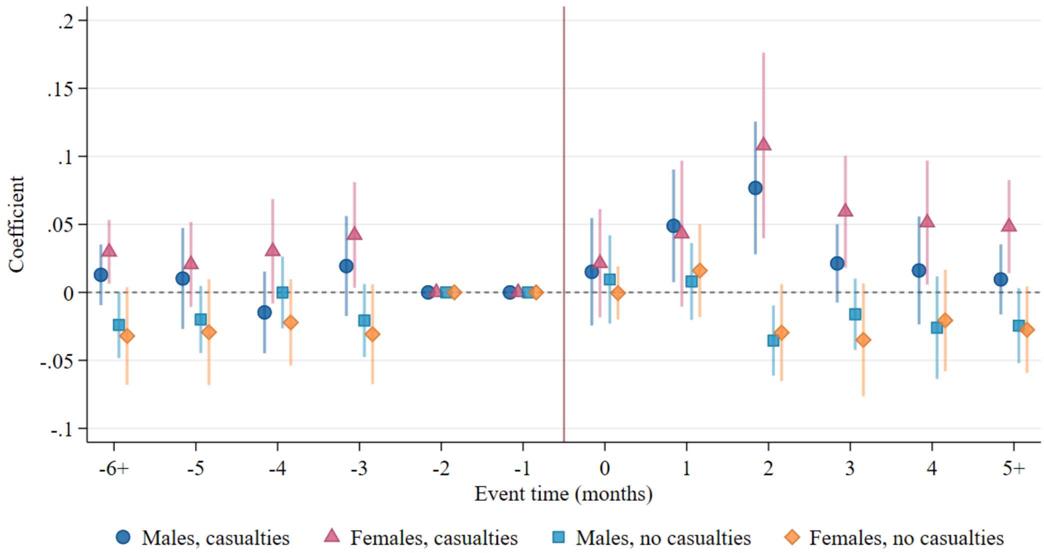
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that our findings are robust to assigning treatment based on events other than shootings, which were the least violent conflicts. It also shows that conflict events experienced after the first occurrence were unlikely to induce migration. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.10: Sex Composition of Migration Flows



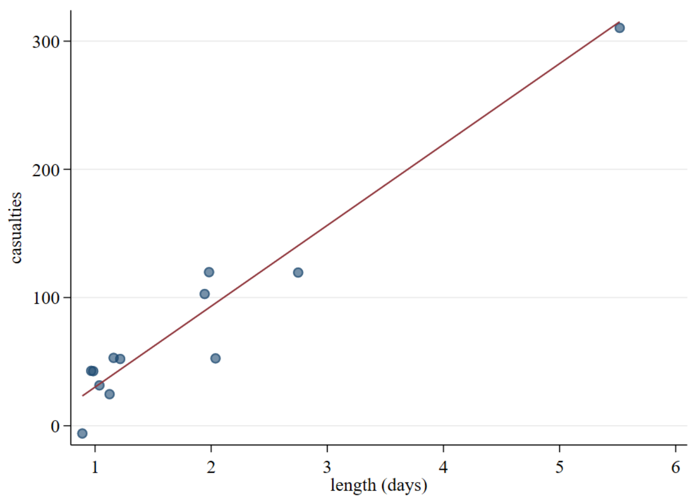
Source: Mexican Border Crossing Records
Notes: The figure shows that before the Mexican Revolution, men were over-represented in the migration flow. As the conflict developed, the sex composition of the flow changed, with women accounting for the majority of the border crossings. The shaded area covers the period of analysis.

Figure A.11: Conflict Mortality and Migration by Sex



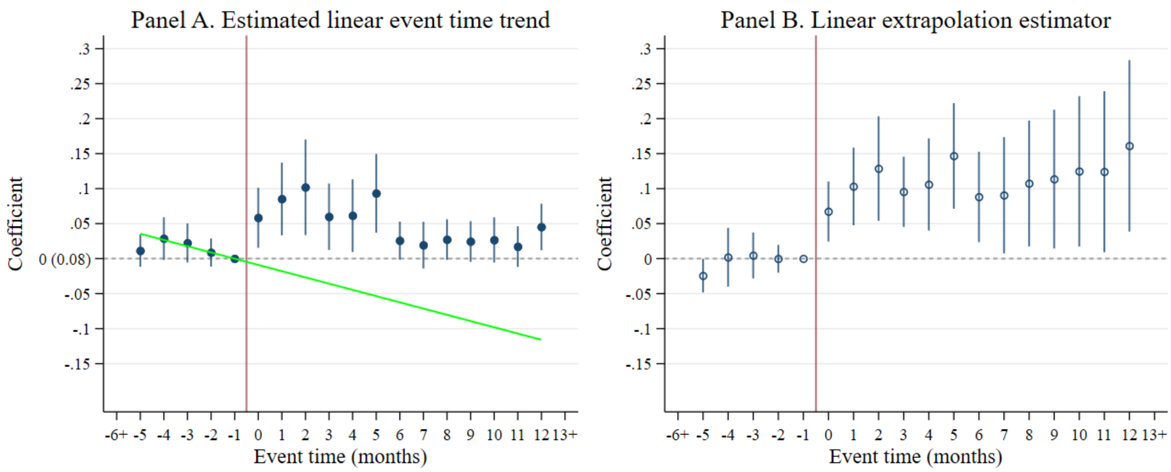
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that the effects of violence—proxied by the number of casualties—are magnified for populations that experience structural inequalities, such as children and women. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.12: Binned Scatter Plot of Conflict Length and Casualties



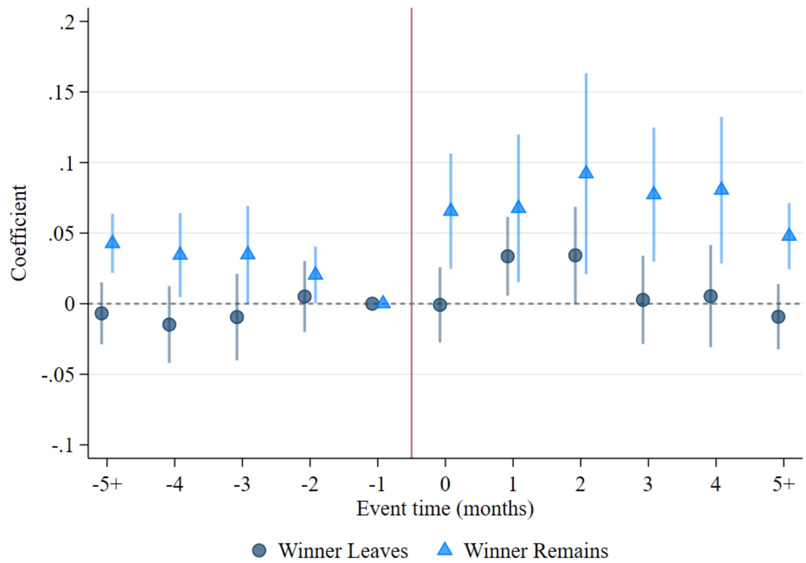
Source: Military History of the Mexican Revolution Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The underlying regression controls for type of event (shooting, combat, siege, or battle) and region fixed effects.

Figure A.13: Controlling for Confound Dynamics
Municipalities with Three or More Days of Conflict



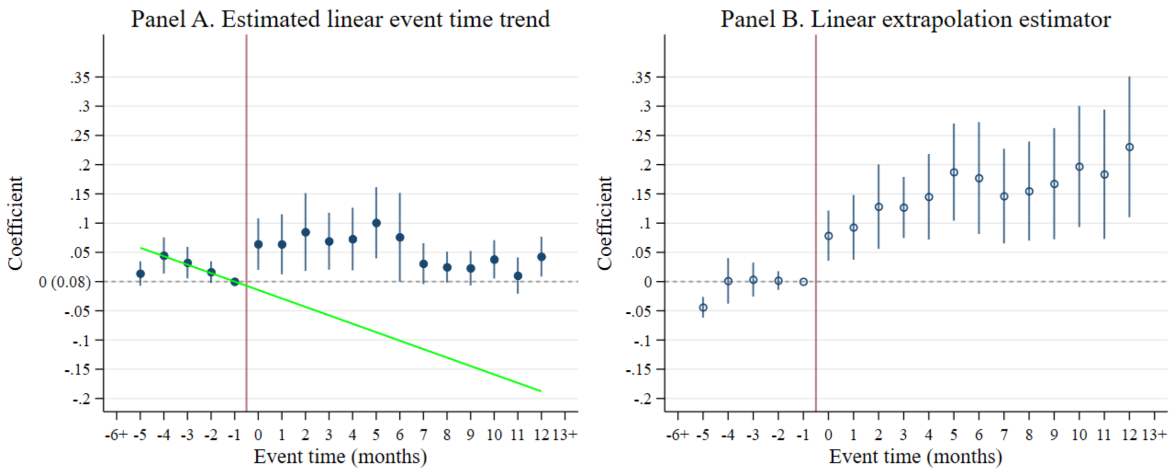
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: We follow Dobkin et al. (2018) and Freyaldenhoven et al. (2021) to control for confound dynamics. We extrapolate a linear event-time trend from the five immediate pre-event periods (Panel A). We then overlay the event-time coefficients for the trajectory of the dependent variable and the extrapolated linear trend. The estimated treatment effect is the deviation from the extrapolated linear trend (Panel B). Circles represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.14: Occupation of Territory and Migration



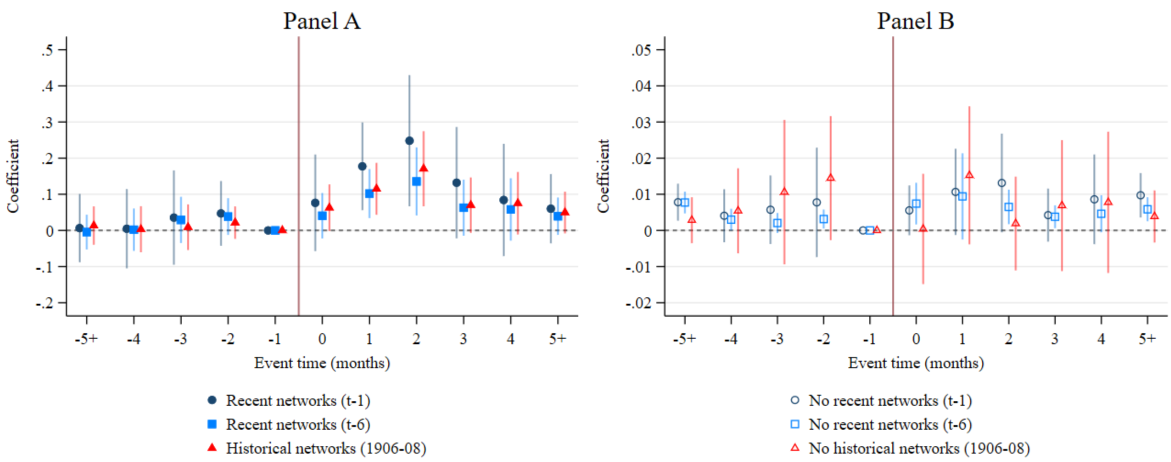
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: Previous research shows that armed forces remaining in the conflict's location disrupts economic and social interactions, as the occupiers may expropriate resources and/or increase mistrust among the population (Cassar, Grosjean and Whitt, 2013; Grosjean, 2014; Rohner, Thoenig and Zilibotti, 2013). Military presence may also increase violent behavior among the population and the likelihood of future conflict (Aburto et al., 2023; Fontana, Nannicini and Tabellini, 2018; Vishwasrao, Schneider and Chiang, 2019). The occupation of territory by armed forces is thus a manifestation of violence that can increase lifetime uncertainty and migration (Engel and Ibáñez, 2007; Ibáñez and Vélez, 2008). The figure shows that monthly migration rates increased permanently only in municipalities where the winner remained after the conflict. Note that coefficients on the leads are statistically different from zero, suggesting the presence of confound dynamics in pre-event periods. In Figure A.15 we show that our finding holds after controlling for confound dynamics. The control group consists of not-yet and never-treated units. Standard errors are clustered at the municipality level. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.15: Controlling for Confound Dynamics
Municipalities where the Winner Remained After the Conflict



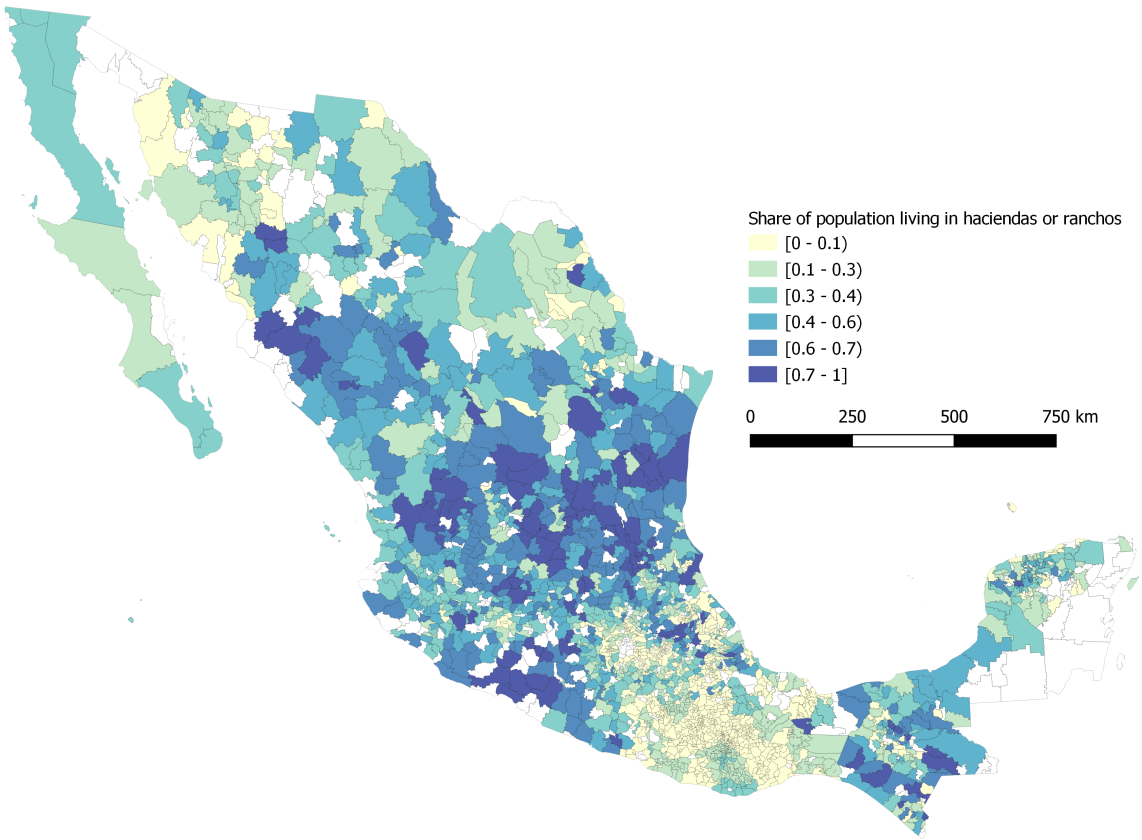
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: We follow Dobkin et al. (2018) and Freyaldenhoven et al. (2021) to control for confound dynamics. We extrapolate a linear event-time trend from the five immediate pre-event periods (Panel A). We then overlay the event-time coefficients for the trajectory of the dependent variable and the extrapolated linear trend. The estimated treatment effect is the deviation from the extrapolated linear trend (Panel B). Circles represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.16: Historical/Recent Networks and Migration



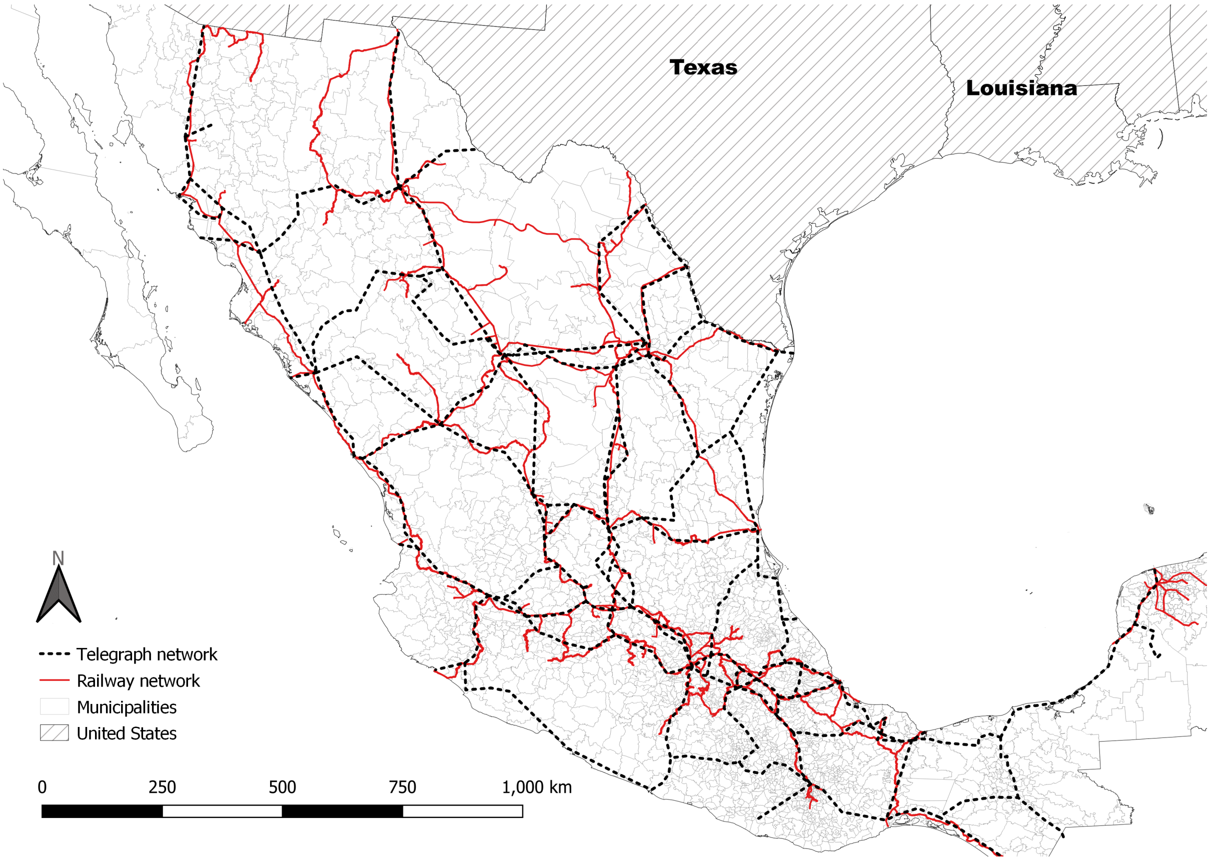
Source: Mexican Border Crossing Records and Sánchez Lamego (1956, 1957, 1960, 1976, 1979, 1983).
Notes: The figure shows that the access to recent and historical networks mediated the migration response to insurgency events. Changes in monthly migration rates are about 10 times greater in municipalities with access to migrant networks (note the difference in the scale of Panel A and B). We identify municipalities with access to recent networks as those with migration flows during the first or six months immediately preceding the event. We identify municipalities with access to historical networks as those with migration flows before the Mexican Revolution (1906-1908). The data on Mexican migration before the revolution comes from Escamilla-Guerrero (2020). The control group consists of not-yet and never-treated units. Standard errors are clustered at the municipality level. Markers represent point estimates. Lines indicate 90% pointwise confidence intervals.

Figure A.17: Land Concentration in 1910



Source: Data from Sellars and Alix-Garcia (2018).
Notes: The map shows the share of population living in large states (haciendas or ranchos), which proxies for land ownership at the local level. Land concentration was particularly high in the West-Central region.

Figure A.18: Communication and Transportation Network Before the Mexican Revolution



Source: Telegraphs data are for ca. 1895 (Mendoza Vargas, 2014). Railways data are for ca. 1910 (Ferrocarriles Nacionales de México, 1914)

Notes: The map shows that the telegraph and railway networks were quite developed before the Mexican Revolution. However, many municipalities did not have direct access to communication and/or transportation infrastructure.

Figure A.19: Map for Guidance

Source: Adapted from López-Alonso (2015).

Notes: The map displays the geographic regions, states, and municipalities of Mexico. It also displays the border crossing points and ports of entry covered by the immigration data.

Table A.1: Error Correction for Temporal and Spatial Correlation

	1	2	3	4	5	6	7
	Baseline	SC 100 km	SC 200 km	SC 300 km	SC 100 km TC 6 mo.	SC 100 km TC 12 mo.	SC 100 km TC 12 mo. H
Leads							
-5+	0.010						
	(0.010)	(0.010)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)
-4	0.002						
	(0.013)	(0.013)	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)
-3	0.006						
	(0.014)	(0.015)	(0.016)	(0.016)	(0.015)	(0.015)	(0.015)
-2	0.011						
	(0.011)	(0.012)	(0.014)	(0.013)	(0.009)	(0.009)	(0.009)
Lags							
0	0.021						
	(0.014)	(0.014)	(0.016)	(0.016)	(0.011)*	(0.011)*	(0.012)*
1	0.045***						
	(0.016)	(0.020)**	(0.023)**	(0.023)*	(0.017)***	(0.017)***	(0.018)**
2	0.053***						
	(0.020)	(0.020)***	(0.023)**	(0.023)**	(0.021)**	(0.021)**	(0.021)**
3	0.027*						
	(0.016)	(0.016)*	(0.018)	(0.018)	(0.016)*	(0.016)*	(0.016)*
4	0.030*						
	(0.018)	(0.017)*	(0.018)*	(0.018)*	(0.019)	(0.019)	(0.018)*
5+	0.010						
	(0.011)	(0.010)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)
Observations	199,944	199,944	199,944	199,944	199,944	199,944	199,944
R-squared	0.409	0.409	0.409	0.409	0.409	0.409	0.409

Source: 1910 Population Census of Mexico, Mexican Border Crossing Records, and Military History of the Mexican Revolution (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983).

Note: The table presents estimates with standard errors corrected for temporal and spatial correlation (Colella et al., 2019; Conley, 1999). Standard errors are clustered at the treatment level (municipality) in the baseline (column 1). We use different distance cutoffs (kilometers) beyond which the correlation of the error term between municipalities is assumed to be zero (columns 2-4). We also use different temporal cutoffs (months) beyond which the temporal correlation among observations of the same municipality is assumed to be zero (columns 5-6). Column 7 reports hetroskedasticity-robust standard errors corrected for temporal and spatial correlation. * = Significant at 10% level; ** = Significant at 5% level; *** = Significant at 1% level.

Table A.2: The Impact of Conflict on Migration by Distance to the Border

	1	2	3	4	5	6	7	8
	Linear Distance (km)				**Linear Distance (km) - No Damage**			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
Leads								
-5+	0.024	0.006	0.002	0.004	0.043	0.002	0.002	0.004
	(0.039)	(0.007)	(0.002)	(0.005)	(0.043)	(0.007)	(0.002)	(0.006)
-4	−0.004	0.004	0.003	0.002	0.020	0.000	0.003	0.002
	(0.049)	(0.008)	(0.002)	(0.005)	(0.061)	(0.009)	(0.003)	(0.005)
-3	0.015	0.001	0.004	−0.002	0.043	−0.004	0.005	−0.004
	(0.056)	(0.008)	(0.002)	(0.003)	(0.073)	(0.009)	(0.003)	(0.003)
-2	0.025	0.008	0.002	0.003	0.045	−0.002	0.001	0.003
	(0.042)	(0.011)	(0.002)	(0.004)	(0.057)	(0.009)	(0.003)	(0.005)
Lags								
0	0.079	0.007	0.001	−0.002	0.093	0.004	0.001	−0.003
	(0.054)	(0.009)	(0.001)	(0.002)	(0.074)	(0.010)	(0.002)	(0.003)
1	0.183^{***}	−0.009	0.001	0.002	0.206^{***}	−0.010	0.002	0.002
	(0.060)	(0.008)	(0.001)	(0.003)	(0.072)	(0.008)	(0.002)	(0.004)
2	0.193^{**}	0.012	0.002	0.005	0.231^{**}	0.001	0.002	0.006
	(0.077)	(0.012)	(0.001)	(0.008)	(0.098)	(0.012)	(0.002)	(0.010)
3	0.103	−0.004	0.006^{**}	0.003	0.124^{*}	−0.013	0.006^{**}	0.003
	(0.063)	(0.008)	(0.002)	(0.004)	(0.068)	(0.008)	(0.002)	(0.004)
4	0.109	0.002	0.006	0.002	0.151	−0.004	0.007	0.002
	(0.071)	(0.010)	(0.004)	(0.006)	(0.096)	(0.010)	(0.005)	(0.007)
5+	0.026	−0.000	0.006^{***}	0.008^{*}	0.038	−0.001	0.006^{***}	0.008
	(0.040)	(0.008)	(0.002)	(0.004)	(0.050)	(0.008)	(0.002)	(0.005)
Observations	165,528	165,456	165,528	165,456	161,928	163,224	163,584	163,872
R-squared	0.407	0.358	0.357	0.363	0.400	0.357	0.357	0.363
Outcome t-1	0.28	0.03	0.002	0.007	0.28	0.02	0.002	0.009
Pretrends	0.68	0.95	0.51	0.65	0.82	0.82	0.55	0.16
Leveling off	0.10	0.73	0.96	0.19	0.12	0.63	0.75	0.16

Source: 1910 Population Census of Mexico, Mexican Border Crossing Records, and Military History of the Mexican Revolution (Sánchez Lamego, 1956, 1957, 1960, 1976, 1979, 1983).

Note: The table shows that migration costs, measured as the linear distance to the border, moderated the timing of the response to conflict. Distance estimates are from the municipality centroid to the nearest border point. The control group consists of not-yet- and never-treated units. Outcome t-1 is the average value of the outcome of interest in the omitted period. Pretrends: Wald test p-value for all the pre-event coefficients being equal to 0. Leveling off: Wald test p-value for the last post-event coefficients (4 and 5+) being equal. * = Significant at 10% level; ** = Significant at 5% level; *** = Significant at 1% level. Standard errors clustered at the municipality level in parentheses.