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Displacement and disconnection: the impact of violence on migration networks and highway traffic in Mexico

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ABSTRACT

This paper examines how violence impacts migration flows and the strength of migration networks across Mexico's 2454 municipalities. Using a novel network algorithm and census data from 2005 to 2020, we detect structural changes in domestic and international migration beyond what net flows reveal. To identify causal effects, homicide rates are instrumented using variation in fuel prices and municipal distance to fuel pipelines, capturing exogenous shocks from large-scale fuel theft. Rising violence led to 1.12 million additional domestic emigrants, 50,200 fewer returnees from the United States, stronger emigration networks and reduced highway traffic linking violent areas to the rest of the country.

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1. Introduction

Violence, from armed conflict to organised crime, can influence migration in complex and sometimes contradictory ways (Korinek et al., 2025). While violence often drives people away by increasing the risk of victimisation (Blattman & Miguel, 2010; Zolberg et al., 1993), strong local ties and economic opportunities may anchor residents or attract newcomers (Adhikari, 2013). Mexico presents a compelling case study in understanding the multifaceted relationship between violence and migration. Since 2006, the government's militarised crackdown on drug cartels has fragmented criminal groups and pushed them into other illicit markets, such as large-scale fuel theft. Rather than reducing violence, this strategy intensified territorial competition and contributed to over 300,000 homicides and widespread insecurity (Calderón et al., 2015). Several studies have found that violence has harmed children's educational outcomes, increased unemployment, poverty, crime, fear of victimisation and hampered the growth of economic sectors such as manufacturing (Calderón et al., 2015; Dell, 2015; Gutiérrez-Romero & Oviedo, 2018; Michaelsen & Salardi, 2020). Thousands of people have fled their homes, at least temporarily, as a result of the violence, according to anecdotal and media reports (Pérez Vázquez et al., 2020). Nonetheless, conflicting reports indicate that many low-income families have been unable to afford the high costs of migration (Basu & Pearlman, 2017). Furthermore, violent areas, many of which are urban and prosperous, have continued to attract immigrants (Atuesta & Paredes, 2016). As a result, the overall effect of violence on migration in Mexico remains unknown.

So far, the literature has examined the causal effect of violence on migration flows in Mexico, mostly using population censuses and homicide data, focusing on the 2005–2012 period, yielding very different results. Some studies find that individuals moved to states (Atuesta & Paredes, 2016) and municipalities with lower levels of violence (Gutiérrez-Romero & Oviedo, 2018), while others find that homicides did not lead to increased domestic migration across municipalities or states (Basu & Pearlman, 2017). Another approach has instead examined emigration from Mexico to the US, using a variety of sources, including migration surveys and census data. Although total migration from Mexico to the US has decreased significantly since 2007, some studies show that municipalities with higher homicide rates increased migration to the US (Daniele et al., 2023; Orozco-Aleman & Gonzalez-Lozano, 2018; Rios Contreras, 2014). In contrast,

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others find that municipalities with higher violence sent fewer people to the US during the same period, possibly due to high migration costs (Basu & Pearlman, 2017).

This paper examines the impact of violence on domestic and international migration in Mexico from 2005 to 2020, addressing two long-standing questions in the literature: whether Mexico's surge in violence displaced population within the country and whether it affected international migration. Unlike earlier studies analysing Mexico, we study the impact of violence on migration networks rather than focusing solely on aggregate migration flows at the municipal level. Analysing changes in the number of migrants alone can obscure several important changes in the structure, strength and change of the migration networks. For instance, a municipality may exhibit stable migration volumes over time while experiencing pronounced shifts in where migrants originate and where they relocate. The intensity of these bi-directional migration flows is subject to change in relation to thousands of other municipalities both within and outside the country. Migration links may emerge between previously unconnected locations, while established corridors may weaken or disappear across domestic and international destinations.

We use network analysis to quantify topological changes in Mexico's domestic and international migration networks using individual-level census data from 2005 to 2020 and the noise-corrected (NC) algorithm proposed by Coscia and Neffke (2017). This algorithm enables us to analyse over six million potential bilateral migration links across Mexico's 2454 municipalities, including their international links. A key challenge is that migration flows are measured with noise, understood as uncertainty arising from measurement error in the data. In our case, all municipalities report migration flows in each census wave. Thus, the concern is not the absence of migration but whether flows are measured imprecisely and how such noise affects the identification of migration networks over time.

Traditional network analysis treats observed connections and their magnitudes as accurate, with potential measurement error arising only from unobserved connections (Newman, 2018; Serrano et al., 2009). The NC algorithm addresses this issue by instead modelling the expected strength of a migration link and comparing it to observed flows, allowing for the identification of statistically meaningful connections in a given period, even when flows are weak. In its original formulation, this comparison is used to delete links that are not statistically distinguishable from noise. While this is useful for describing network structure in one period, it is less suitable for analysing changes over time, since weak links are pruned even though they may grow or decline over time. Our first contribution is to modify the original NC backboning algorithm to track changes in migration network strength by retaining all observable origin–destination links between municipalities, without pruning weak links. This modification allows us to observe all migration links over time and quantify network expansion or contraction separately for domestic and international emigration and immigration at the municipal level through four distinct metrics.

The network literature that has analysed migration either models the mechanisms that drive network formation and change or focuses exclusively on describing the structure of migration networks without identifying their underlying drivers (Fagiolo & Mastrorillo, 2013; LeSage & Pace, 2008; Tranos et al., 2015). We follow the latter approach. The NC is descriptive by construction and does not model any socioeconomic factors or geographical distance that may affect migration. To address this limitation, in a second stage, we econometrically model the factors driving the identified changes in the migration network and, separately, the aggregate migration flows.

A key concern is endogeneity, as both violence and migration may respond to unobserved local conditions, biasing causal interpretation based solely on observed changes in migration patterns. Our identification strategy exploits the rise and spatial spread of homicide rates that accompanied Mexican drug cartels' expansion into large-scale gasoline and diesel theft. Due to underinvestment in the state-owned oil company PEMEX, the government increased reliance on fuel imports and reduced fuel subsidies. These policies raised gasoline and diesel prices, inadvertently making fuel theft highly profitable. Facing increased drug enforcement and these new profit opportunities, drug cartels expanded into large-scale fuel theft, fighting violently to control territory with access to pipelines, where they could extract fuel and resell it on the black market. This competition generated sharp increases in homicide rates along the fixed fuel pipeline infrastructure across the country, including regions that had not previously experienced high levels of violence (Franco-Vivanco et al., 2023).

We instrument for changes in municipal homicide rates using fuel price changes interacted with the inverse distance from each municipality to the nearest fuel pipeline. This interaction captures differential exposure to fuel theft and, thus, violence across municipalities, generating exogenous variation in both the level and the spatial distribution of violence. Fuel prices are determined by national energy policy and global markets, while pipeline infrastructure long predates the drug war and has no direct economic relevance for households beyond facilitating violence. First-stage estimates confirm that the instruments strongly predict changes in homicide rates and reveal substantial endogeneity in the violence-migration relationship.

We contribute to the migration literature focused on Mexico by analysing the causal impact of violence on both migration flows and migration networks, and over a long period (2005–2020), covering domestic, international and return migration of Mexicans from the US. Because common shocks (such as violence, enforcement operations and other regionally correlated disturbances) may affect migration in nearby municipalities simultaneously, we use Conley-type standard errors that allow for distance-based cross-municipality dependence, combined with heteroskedasticity- and autocorrelation-consistent corrections for serial correlation within municipalities over time (Colella et al., 2023).

Results show that violence expanded and reshaped migration patterns in ways net migration measures overlook, generating substantial internal displacement. Between 2005 and 2020, the rise in homicide rates led to 941,000 domestic immigrants and 1.12 million domestic emigrants, accounting for 5% of all domestic immigration, and 6% of all domestic emigration. Violence also expanded emigration networks both within the country and toward the US.

Our analysis of international migration focuses on the US, which accounts for over 90% of emigration and 95% of return migration of Mexicans from abroad. Although US and Mexican data indicate that more Mexicans returned than emigrated after 2008 (Gonzalez-Barrera, 2015; Pearson, 2021; Rosenblum et al., 2014), our findings show that violence partially reversed this trend. Between 2005 and 2020, violence led to 22,399 additional people emigrating to the US and discouraged 50,249 Mexican nationals from returning from the US This reflects a 5% rise in emigration to the US and a 3% drop in return migration from the US, despite stable return international network.

Another contribution is to provide causal evidence that, consistent with new economic geography mechanisms (Fujita et al., 1999; Krugman, 1991), violence undermines regional integration by increasing spatial frictions and weakening intermunicipal linkages. We analyse this using network analysis, comparing changes in daily vehicle traffic flows on Mexico's federal highways in 2005 and 2015, prior to the COVID-19 pandemic. Using changes in the number of drug trafficking organisations as an instrument for municipal homicide rates, we show that despite a 154% increase in national traffic, rising violence eliminated nearly 110 million daily intermunicipal trips, about 49% of highway traffic in 2015, primarily by reducing long-distance connectivity rather than local circulation.

Thus, for spatial economic analysis, this case study shows how local violence reshapes migration flows and networks, and with important implications for the spatial connectivity of affected municipalities. The paper proceeds as follows: Sections 2–4 describe hypotheses, the data used and trends observed using these data sources. Section 5 outlines the NC algorithm. Section 6 presents the econometric results. Section 7 discusses the highway robustness check. Section 8 concludes.

2. How does migration react to violence?

Extensive research has examined the micro- and macro- determinants of migration (Borjas, 1989; Munshi, 2016; van Meeteren & Pereira, 2018). This literature has shown that large-scale armed conflicts, such as civil wars, lead to forced displacement and emigration (Chetail, 2014). Yet, there is mixed evidence on whether drug-related violence causes displacement similar to that observed in large-scale armed conflicts (Basu & Pearlman, 2017). This ambiguity reflects that drug-related violence simultaneously generates migration push factors (insecurity and economic loss) and pull factors (local economic opportunities), unlike other conventional armed conflicts.

Territorial control by drug trafficking organisations exposes civilians to crossfire, kidnapping and extortion, with Latin America leading in stray-bullet injuries and deaths linked to gang and drug-related violence (UNLIREC, 2016). Fear of victimisation can be a powerful push factor to relocate despite significant economic or social costs (Atuesta & Paredes, 2016). Drug-related violence may also induce emigration through economic channels by raising firms' operating costs, helping explain why such violence in Colombia and Mexico has reduced output, profits, wages and employment (Dell, 2015; Rozo, 2018).

At the same time, these push pressures do not preclude the presence of countervailing forces that can sustain or even attract population inflows into violent areas. In long-term and geographically extensive conflicts, relocating to safer regions or abroad may be economically unfeasible for many households (Korinek et al., 2025). Moreover, while most drug trafficking profits flow to tax havens, some are redirected to the Global South and laundered through sectors such as construction, tourism and retail, sustaining local activity and employment even in violence-affected areas (Gutiérrez-Romero & Oviedo, 2018).

Previous research has examined the causal effect of violence on net migration in Mexico using census data for 2005–2012, with contrasting findings. One approach has been to analyse net flows of domestic immigration across states using coarsened exact matching, finding that people who moved to states with lower levels of drug-related homicide rates were willing to do so despite the potential wage penalty and higher cost of living (Atuesta & Paredes, 2016). Although this research suggests that violence may play a role, it ignores that, according to census data, most domestic immigration in Mexico occurs primarily within states. A second approach has been to look instead at the impact of violence on net emigration and immigration flows at the municipality level. The evidence is once again inconclusive. According to some studies using the difference-in-differences method with kernel matching, domestic immigration flows increased in municipalities with the lowest rates of drug-related homicide (Gutiérrez-Romero & Oviedo, 2018). Others, using municipality distance to highways and cocaine supply shocks from Colombia as an instrument for the annual overall homicide rate, find that drug-related violence did not increase domestic migration across municipalities or states (Basu & Pearlman, 2017).

Another open question is whether violence in Mexico discourages return migration from the US, a key issue given that Mexico is the largest source of unauthorised immigrants in the US (Passel & Cohn, 2014). According to official data from both countries, since 2008, more Mexican migrants have returned from the US than have migrated to it, reversing long-standing patterns (Gonzalez-Barrera, 2015). This shift is attributed to slow US economic recovery, tighter border enforcement and recent immigration laws, while survey evidence points to family reunification as the primary motive for return (Rosenblum et al., 2014).

The literature has instead extensively studied whether violence in Mexico increases non-transit emigration to the US, but the evidence remains mixed (Basu & Pearlman, 2017; Daniele et al., 2023; Orozco-Aleman & Gonzalez-Lozano, 2018). For instance, Basu and Pearlman (2017) use census data and instrument homicide rates with municipal proximity to highways and cocaine supply shocks, finding that higher violence reduces emigration to the US, consistent with rising costs from stricter border enforcement and border insecurity. In contrast, Orozco-Aleman and Gonzalez-Lozano (2018), using the Survey of Migration to the Northern Border of Mexico (EMIF North) and instrumenting for homicides using variation from electoral cycles, find that violence increased migration outflows to the US Similarly, Daniele et al. (2023) combine Mexican census data with US voluntary consular registration records and exploit an exogenous shock to US heroin demand in a difference-in-differences framework. They find that more people living in areas more suitable for opium production in Mexico and those near the northern border emigrated to the US.

These mixed findings reflect differences in data and methods used. Voluntary consular registries and the EMIF are better suited for identifying seasonal migration flows, whereas population census data are representative at the municipal level and better suited for studying the role of local violence and longer-term migration dynamics. For this reason, we rely on census data for both domestic and international migration. To address the limitations of studies focused solely on net migration flows, we complement them with network analysis, which is a well-established method in the spatial analysis of human mobility systems, including migration, air and vehicle traffic (see, Fagiolo & Mastrorillo, 2013; LeSage & Pace, 2008; Reggiani & Nijkamp, 2009; Tranos et al., 2015).

Network analysis does not replace aggregate migration flows; it addresses a different question. Network analysis captures changes in the structure of migration networks, specifically whether an area, even with stable net migration, is gaining or losing links with multiple origins or destinations, domestically or internationally. These structural changes are not visible in aggregate flows but are central to understanding whether areas are becoming more integrated with, or increasingly disconnected from, the rest of the country or abroad. We hypothesise that violence increases emigration, while its net effects on migration flows and implications for migration networks remain an empirical question.

3. Data

3.1. Migration data

To examine changes in municipalities' migration networks and flows, we use the 2010 and 2020 population censuses and the 2015 intercensal survey, all collected by Mexico's National Institute of Statistics and Geography (INEGI), which makes them publicly available as microdata samples on its website. These include sampling weights to ensure representativeness at both the municipal and national levels (INEGI, 2020). Each source captures migration that occurred during the five years preceding the interview date. That is, the 2010 census covers migration moves from 2005 to 2010, the 2015 survey from 2010 to 2015 and the 2020 census from 2015 to 2020. Notably, the 2020 census was not affected by the COVID-19 pandemic, as fieldwork concluded shortly before lockdowns.

The census captures migration history by asking respondents where they lived five years before the interview. Those who lived in Mexico report their municipality and state; those who lived abroad report their former country of residency. Unfortunately, the census does not record migration within the same municipality, the exact timing of moves across municipalities, or whether a person moved multiple times. However, by comparing respondents' current residence and their reported residence five years earlier, we can identify whether they moved during the five years preceding the interview. Because the census also records each person's nationality, we can identify Mexican nationals who returned from abroad. Among these return migrants, we distinguish those who came back from the US.

3.2. Identifying changes in migration

For each municipality and census, we count how many individuals moved in and how many moved out, distinguishing domestic moves from those to the US. We then compare these counts across consecutive censuses to measure changes in immigration and emigration for each municipality, which are used in the change in migration flow analysis and as inputs to the separate change in migration network analysis (described in Section 5).

We compare the 2010 census to the 2015 intercensal survey, and the 2015 survey to the 2020 census. Because each census captures migration flows over the preceding five years, this yields two periods of analysis: 2005–2010 and 2010–2015 for the first comparison, and 2010–2015 and 2015–2020 for the second.

During our period of analysis, only the 2010 and 2020 censuses asked respondents to indicate whether any household member had emigrated abroad. The dates of international emigration are recorded with greater precision than those of domestic migration. To maintain consistency with the periods we analyse for domestic migration, we capture international emigration that occurred five years before the interview, as well as in the year 2010. This allows us to compare changes in international emigration networks for the years 2005 and 2010, and 2010 and 2015.

3.3. Homicide rate and additional variables

We use the overall municipal homicide rate as a proxy for local violence. Although some earlier studies relied on drug-related specific homicides (Dell, 2015; Gutiérrez-Romero & Oviedo, 2018), this database is available only for 2006–2012, since the government discontinued its publication due to political sensitivity. Currently, overall homicide records can be obtained either from the judicial system (subject to uneven investigative capacity and political incentives) or from death certificates made public by INEGI. The death certificates are issued by forensic professionals prior to any judicial investigation and are compulsory for burials, making them the most reliable and consistently available source of homicide data. Accordingly, and in line with the existing literature, we construct municipal homicide rates per 100,000 inhabitants using INEGI death certificates and total population estimates from the Consejo Nacional de Población (CONAPO). To assess how fluctuations in homicide rates influence changes in migration, we estimate the changes in these rates between the years 2005 and 2010, and between 2010 and 2015.

We include three additional municipal-level controls. First, changes in poverty are measured as the difference in municipal poverty rates across consecutive five-year periods from 2005 to 2015, using census data and defining poverty as per capita income below five dollars per day. The 2005 poverty rate is proxied using the 2000 census, as household income is not reported in the 2005 intercensal survey. Second, changes in inequality are measured as the difference in the municipal Gini coefficient over the same five-year periods, based on estimates from the Consejo Nacional de Evaluación de la Política de Desarrollo Social (CONEVAL). Third, as a proxy for wealth, we include the changes in annual nighttime light per capita. These are calculated as the differences between consecutive five-year averages in 2001–2005, 2006–2010 and 2011–2015. Nighttime light data are from the Earth Observation Group at the Payne Institute for Public Policy (Elvidge et al., 2017).

In the robustness section, we examine how violence affects highway vehicle traffic using data from the Ministry of Communications and Transport.

4. Homicide trends

When Felipe Calderón became President of Mexico in 2006, the country had one of the lowest homicide rates in Latin America, at 10 per 100,000 inhabitants (Wainwright, 2017). Nonetheless, drug-related violence was already rising in northern states such as Tamaulipas, Chihuahua, Sinaloa and Michoacán. To reassert authority after winning a closely contested election, Calderón launched a militarised campaign against drug cartels, departing from long-standing strategies focused on drug interdiction (Dell, 2015). This marked the beginning of Mexico's 'war on drugs', which led to a sharp increase in violence. The homicide rate doubled by 2011, fell slightly toward the end of his term, and rose again under President Peña Nieto, peaking in 2018. Although violence has stabilised under President Andrés Manuel López Obrador (AMLO), it remains at historically high levels (Figure 1).

Drug trafficking groups adapted to intensified state crackdowns by diversifying into alternative illegal markets, notably large-scale gasoline and diesel theft (Franco-Vivanco et al., 2023). By 2010, the Zetas and the Gulf Cartel were systematically stealing fuel from pipelines, relying on the cooperation or coercion of local and federal authorities. Soon after, emerging criminal organisations, including the now-transnational Cártel Jalisco Nueva Generación (CJNG) led by Nemesio Oseguera Cervantes ('El Mencho') and the Santa Rosa de Lima Cartel, also entered the fuel theft business, triggering violent disputes over pipelines. Stolen fuel is primarily distributed through ostensibly legitimate gasoline stations, but it is also smuggled into the US and Guatemala, while mayors, as heads of local police, face pressure to tolerate these operations, and those who resist risk assassination (Gutiérrez-Romero, 2026).

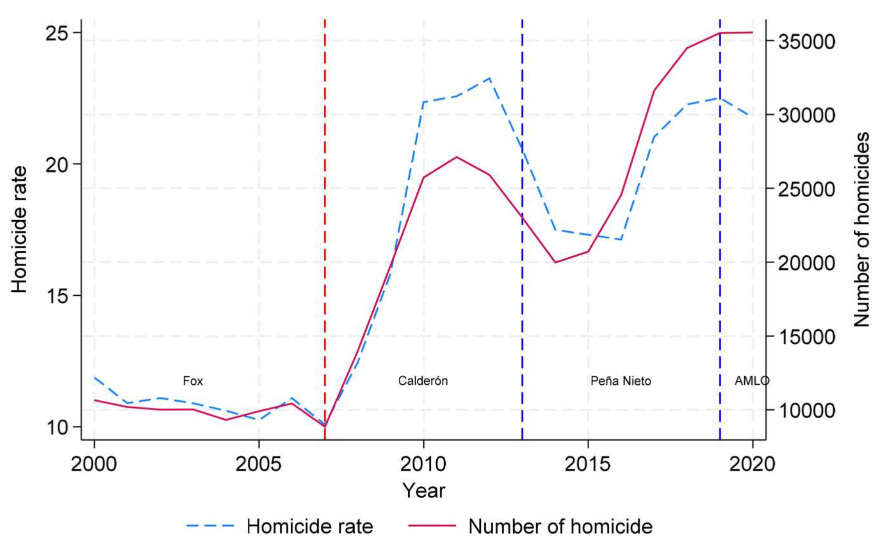

Figure 1. Number of homicides and homicide rates per 100,000 inhabitants in Mexico.
Source: Authors' own calculations using INEGI (homicides) and CONAPO (population).

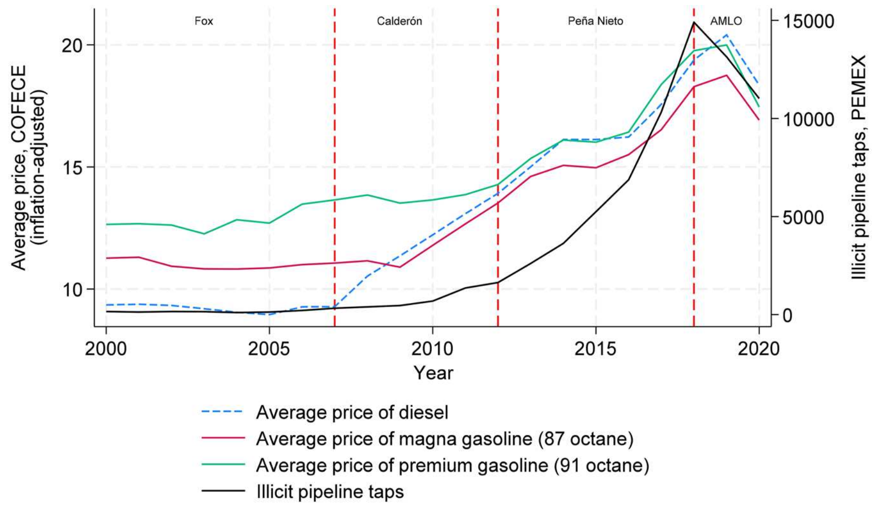

Figure 2. Average price of gasoline in pesos and number of illicit pipeline taps.
Source: Authors' own calculations using PEMEX (pipeline taps) and COFECE (prices).

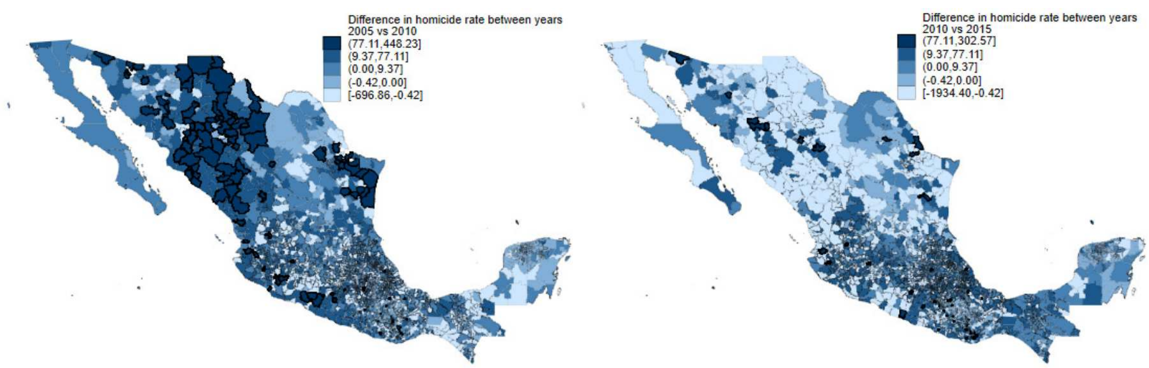

Figure 3. Changes in homicide rates.
Source: Authors' own calculations using INEGI (homicides) and CONAPO (population).
Note: Internal thresholds are identical across panels; only the minimum and maximum bounds differ to reflect changes in the overall range.

The deregulation of gasoline prices further increased the appeal of fuel theft. As PEMEX's infrastructure aged and production declined, Mexico became more dependent on fuel imports. The federal government responded with sudden price hikes to reduce subsidies and align with global markets, including a 20% increase in 2017 and full deregulation in 2018, with prices remaining high since then (Gutiérrez-Romero, 2026). In 2018, the federal government temporarily reduced fuel theft by distributing gasoline via tanker trucks instead of pipelines. However, this measure was short-lived, and fuel theft quickly resurged.

Figure 2 shows the rise in illicit pipeline taps and fuel prices. Figure 3 illustrates how violence initially concentrated in northern and coastal states, then spread to central Mexico. Figure 4 shows the pipeline network, revealing a spatial overlap with areas experiencing rise in violence.

5. Change in migration networks

We use network analysis to track changes in domestic and international migration across each of Mexico's 2454 municipalities. The network-based approach considers the origin and destination of migration flows for each area. This allows us to measure whether a municipality's connections with other areas, both domestically and internationally, are becoming stronger or weaker over time.

Popular methods in network analysis, such as the disparity filter (DF), often assume that if no migration connection is recorded between two areas, it indicates a complete absence of migration (Serrano

Figure 4. Gasoline and diesel pipeline network.
Source: PEMEX.

et al., 2009). This approach fails to account for potential measurement errors or data noise, which can lead to biased network representations. To overcome this limitation, we apply the noise-corrected (NC) backbone algorithm developed by Coscia and Neffke (2017). The NC algorithm addresses these issues by accounting for noise in the data and providing a more accurate calculation of changes in both the structure and strength of migration networks using a probabilistic approach. Importantly, this approach solely focuses on identifying the network structure itself and its changes, without incorporating or needing socio-economic factors that may drive these changes.

The NC algorithm reveals the migration network and changes in such a network over time in three steps. First, it compares the observed migration flows between municipalities, known as edge weights, to what would be expected under a null model. The null model assumes that the likelihood of migration flows between any two areas is proportional to the total number of migrants leaving one area and entering another. The expected number of interactions between two areas, i.e., node pair (i, j) is expressed as in Equation (1). This formula provides the baseline expectation for each migration connection, the expected edge weight, under the null model.

$$E[N_{ij}] = \hat{N}_i \frac{\hat{N}_j}{\hat{N}_T} \tag{1}$$

where $\hat{N}_T$ represents the total number of migrants across the entire network (we analyse domestic and international migration separately). $\hat{N}_i$ is the total number of emigrants leaving the municipality i and $\hat{N}_j$ is the total number of immigrants arriving at location j within the same period.

In the second step, the NC algorithm calculates the difference between the observed edge, the observed migration flow, $\hat{N}_{ij}$ and the expected edge weights $E[N_{ij}]$ to estimate the so-called edge weights' lift. As shown in Equation (2), the lift measures how unexpectedly high an edge weight is given the weights of i and j.

$$L_{ij} = \frac{\hat{N}_{ij}}{E[N_{ij}]} \tag{2}$$

Since the lift L_{ij} is not symmetric around 1, a transformation to centre it around zero, L'_{ij} is applied for easier interpretability.

In the third step, the NC algorithm calculates the variance of the edge weights, which is the variation of the migration flows $V[\hat{N}_{ij}]$. The variance, given by Equation (3), is derived from a binomial distribution, as migration flows are treated as a series of independent interactions (i.e., migrating from area i to j).

$$V[N_{ij}] = N_T P_{ij}(1 - P_{ij}) \tag{3}$$

where P_{ij} is the probability of having a migration flow going from location i to location j, relative to all migration flows in the network. P_{ij} is unknown but can be estimated as the observed frequency with which interactions occur, as shown in Equation (4).

$$\hat{P}_{ij} = \frac{\hat{N}_{ij}}{\hat{N}_T} \tag{4}$$

A common problem in network analysis, especially with sparse networks, is that for certain node pairs i and j, the observed migration flow may be zero $\hat{N}_{ij} = 0$. In such cases, its variance will also be zero, $V[N_{ij}] = N_T P_{ij}(1 - P_{ij}) = 0$. In other words, when no observed migration occurs between two locations, it becomes difficult to estimate the likelihood of migration flows between them with any precision. The NC algorithm addresses this uncertainty by estimating P_{ij} using a Bayesian framework.

This Bayesian framework uses a Beta distribution to model the probability of migration between two municipalities P_{ij}, incorporating prior information about migration patterns. The prior is based on the average number of migrants leaving and arriving at each municipality across the entire migration network, with the parameters α and β derived from the mean and variance of those flows. As actual migration data become available, the algorithm updates the prior to form a posterior distribution. This posterior, also modelled by a Beta distribution, refines the initial estimates by incorporating the observed migration data for each municipality $\hat{P}_{ij}$. Thus, the posterior is just a refined version of the initial guess (the prior) after considering all the available data. The posterior distribution also follows a Beta distribution $\hat{P}_{ij}$ ~Beta $[n_{ij} + \alpha, n_T - n_{ij} + \beta]$. Where $n_{ij} = 0$ (since there is no observed migration for that node), and the prior parameters α and β, and n_T refer to the same total number of migration flows in the network based on the prior information when dealing with uncertainty.

5.1. Our modification of the NC algorithm to calculate changes in the migration network

Once the posterior distribution for $\hat{P}_{ij}$ is calculated, the original NC algorithm evaluates the expected variance of edge weights to determine which migration flows are statistically significant at time t. This is where backboning comes in. Backboning is a strategy used to simplify complex, noisy networks by retaining only the most meaningful connections. In this case, migration flows between two municipalities at time t. The algorithm sets a threshold, controlled by the parameter δ, and removes edges that do not meet this level of statistical significance. This helps to highlight the underlying structure of the network by pruning weaker, less reliable connections, leaving only the most relevant flows for further analysis. Our approach deviates from the original NC algorithm in two ways, as explained next.

Our goal is to analyse changes in the migration network over time, comparing two points, t and $t + 1$ (e.g., the 2010 census and the 2015 intercensal survey). Pruning weak edges at a single time point would present a challenge, as it might falsely suggest that certain edges emerge or disappear in the following period, when they may have simply been present but weak. To avoid this, we adjust the NC algorithm by retaining *all* edges in the network, derived from the posterior distribution, instead of filtering based on statistical significance.

Second, we evaluate whether migration flows between two municipalities have increased or decreased over time. This is done by analysing the edge weight variance to assess the probability that the edge's weight is effectively zero. We derive a posterior distribution for the same edge at different moments in time, t and $t + 1$, and use the inferred variances to quantify whether the edge weight has significantly changed between these two periods using t-tests. To account for uncertainty, we apply bootstrapping.

We do not use these t-tests to prune statistically significant changes. Here, the t-tests indicate directional trends, whether migration flows are increasing or decreasing, without removing any edges. A positive t-score reflects an increase in migration flows, while a negative t-score indicates a decrease. We refer to the values of these t-tests as z-scores because they provide a standardised way to measure changes in migration flows, even though they are not bounded between 0 and 1. Like traditional z-scores, they represent deviations from expected values, allowing for comparisons across different networks and time periods.

By retaining all edges, we avoid the problem of artificially emerging edges caused by pruning at one time point t but not at another $t + 1$, or vice versa. This approach ensures that we track changes in the migration network, including its weak connections. The standardised z-scores aggregate changes at the municipal level into four distinct metrics (domestic emigration, domestic immigration, international emigration and international immigration), capturing shifts across six million of potential bilateral connections among municipalities over time.

5.2. Migration flows and networks

Table 1 reports the change in the network z-scores and the average municipal-level changes in migration, rather than national totals, between the 2010 census and the 2015 intercensal survey, and between that 2015 survey and the 2020 census. Each of these captures migration that occurred during the five years preceding the respective interview.

Domestic emigration declined between 2005–2010 and 2010–2015, while network z-scores increased, indicating a strengthening of the domestic migration network despite lower overall out-migration. Between 2010–2015 and 2015–2020, domestic emigration rebounded, with increases in both migrant numbers and network z-scores.

Domestic immigration displays a clearer pattern: both migrant numbers and network z-scores declined in the first period and increased in the second. Return migration from the US declined along both dimensions in the first period. The subsequent period tells a more mixed story: from 2010–2015 to 2015–2020, the number of return migrants rose (by 99.89), even as their network z-score continued to fall (−60.48).

Emigration to the US followed a cyclical path, contracting between 2005 and 2010, and rebounding between 2010 and 2015 in both migrant numbers and network z-scores.

The changes in Mexico's domestic migration network can also be visualised. For instance, Figure 5 compares migration flows between the 2015 intercensal survey and the 2020 census. The nodes represent municipalities, and the edges reflect migration connections, estimated using the NC backboning algorithm.

Table 1. Descriptives per municipality.

	Mean	Standard deviation	Observations	Mean	Standard deviation	Observations
	Change in period 2005–2010 vs 2010–2015			Change in period 2010–2015 vs 2015–2020		
Change in the number of domestic emigrants leaving municipalities	−233.999	3555.338	2456	205.619	2507.785	2456
Change in the number of domestic immigrants moving into municipalities	−234.676	3120.915	2456	199.223	3451.752	2456
Change in the number of Mexicans that returned from the US	−453.003	620.070	2456	99.889	304.276	2456
Z-score of change in migration network of domestic emigrants	3.667	8354.301	2456	331.580	7972.509	2456
Z-score of change in migration network of domestic immigrants	−8.247	5592.579	2456	276.566	7346.668	2456
Z-score of change in migration network of return immigration of Mexicans from the US	−155.474	367.819	2456	−60.477	201.625	2456
Change in the number of people who left Mexico for the US	−24.789	107.296	2456	4.234	75.349	2433
Z-score of change in migration network of emigration to the US	−98.030	181.428	2456	91.466	170.718	2433
Change in the number of homicides	6.612	81.470	2394	−2.099	74.575	2394
Change in the homicide rate	12.108	77.131	2393	−5.049	72.859	2394
Change in the Gini index	0.017	0.050	2453	−0.041	0.043	2445
Change in the poverty rate	−0.153	0.100	2442	−0.016	0.055	2456
Change in night-time light per capita	0.003	0.022	2455	−0.044	0.048	2456
Change in the average price of diesel and gasoline divided by the municipality's average distance to the nearest oil pipeline	0.149	2.637	2456	0.552	9.631	2456
Change in the price of gasoline in real pesos	0.951	0.012	2456	2.364	0.013	2456

Source: Authors' own calculations.

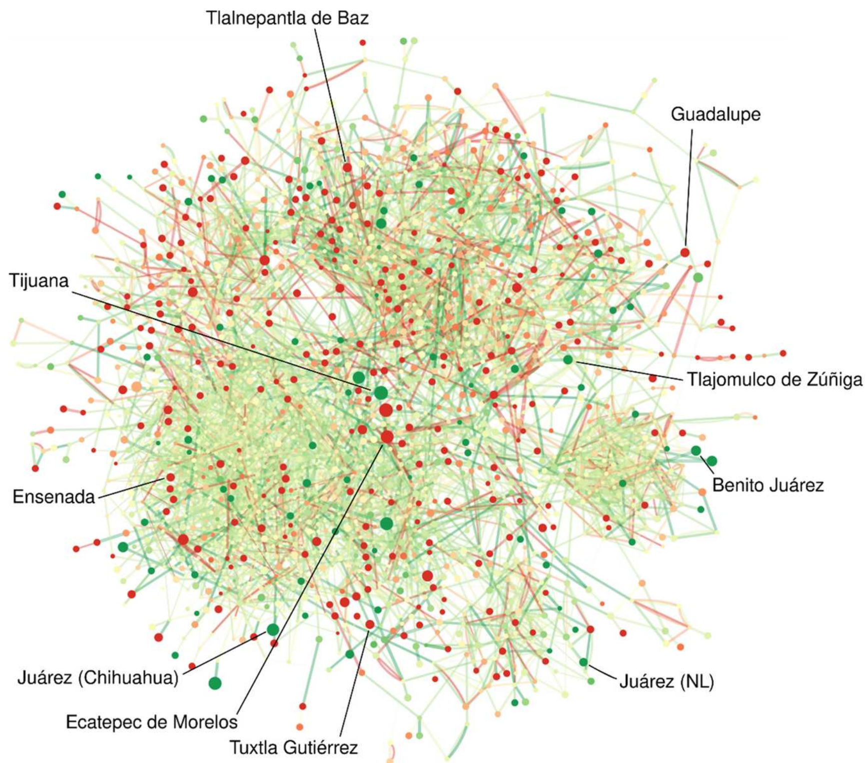

Figure 5. Change in domestic immigration comparing migration flows between the periods 2010–2015 and 2015–2020. Source: Authors' own calculations.
Note: Nodes represent municipalities, and links represent migration flows. Node colour indicates changes in migration flows between periods (green: increase, yellow: no change, orange: moderate decline, red: strong decline). The layout is topological rather than georeferenced to visualise changes in connectivity rather than geographic proximity.

Municipalities are colour-coded to indicate increased or decreased migration connectivity. The network is not spatially constrained, as significant migration flows occur between distant regions. The network depicts several municipalities in green, indicating that immigration is not concentrated in a few major urban centres but is spread across a wide range of areas (Figure 5). To improve readability and avoid clutter, we omit the names of most municipalities from the figure, highlighting only a few notable cases. For instance, municipalities like Tijuana and Juárez (in Chihuahua) are expanding their domestic immigration network by receiving flows from wider areas. Other areas near Mexico City such as Tlalnepantla and Ecatepec (both in Estado de México), have reduced their immigration network. In the following section, we examine the potential impact of rising homicide rates on the observed changes in the emigration and immigration networks.

6. Econometric analysis and results

After quantifying the changes in migration networks, we evaluate the extent to which violence drove those changes. We begin by estimating a panel fixed effects regression, as in Equation (5).

$$\Delta m_{it} = \beta_1 DHomicides_{it} + \beta_2 DX_{it} + \beta_3 T_t + m_i + e_{it} \tag{5}$$

where $\Delta \mathrm{m}_{it}$ denotes the standardised z-scores, which measure changes in the migration network for municipality i during time t (i.e., change in z-scores when comparing census data during 2005–2010 vs. 2010–2015 and 2010–2015 vs. 2015–2020).

We use four dependent variables separately. These are the z-scores for domestic emigration, domestic immigration, international emigration to the US, and return migration of Mexicans from the US. Separately, we also analyse the change in the number of migrants, whether emigrants or immigrants, domestic or international.

$\Delta Homicides_{it}$ denotes the change in the homicide rate during the quinquennial periods compared (i.e., 2005 vs. 2010 and 2010 vs. 2015). The vector ΔX_{it} denotes the change in the poverty rate, and the Gini coefficient during the periods compared (i.e., 2005 vs. 2010 and 2010 vs. 2015). We also add the changes in annual nighttime light-per-capita, over the periods 2001–2005, 2006–2010 and 2011–2015, as a proxy for wealth. The specification includes a dummy variable comparing the quinquennial terms being compared T_t (equal to 1 for the first quinquennial comparison 2005–2010 vs. 2010–2015 and zero otherwise) and municipality fixed effects μ_i. The residual is denoted by ε_{it}.

Standard errors are adjusted for spatial and temporal dependence using a Conley-type distance-based clustering structure with a 190 km radius and are heteroskedasticity- and autocorrelation-consistent with two lags, the maximum feasible given that the analysis compares two census-to-census periods. The distance cutoff is chosen to capture spatial correlation arising from shared labour markets, violence and policy exposure among nearby municipalities. Results are robust to alternative thresholds and arbitrary clustering correction as described by Colella et al. (2023).

6.1. Instrumental variables

While the panel fixed effects regression controls for time-invariant differences across municipalities, it is unlikely to resolve endogeneity arising from time-varying factors that jointly affect homicide rates and migration. Battle locations among drug trafficking organisations are non-random, as groups violently contest control over smuggling routes and areas suitable for large-scale fuel theft. These areas may also attract or deter domestic and international migrants for reasons unrelated to violence, such as changes in institutional conditions or economic opportunities. Consequently, unobserved local shocks may simultaneously affect violence and migration, biasing the fixed-effects estimates.

If unobserved economic shocks drive both homicides and migration, the estimates may be upward biased, overstating the likely deterrent effect of violence on migration. Conversely, if violence depresses local economic activity and discourages migration simultaneously, the estimates may be downward biased, understating the true effect of the rise in homicides. To address this concern, we implement an instrumental variables (IV) strategy, combining it with municipality and year fixed effects to isolate plausibly exogenous variation in violence.

Our identification relies on two instruments: (i) the national changes in fuel prices, which have been driven by exogenous national policy shifts and the gradual elimination of fuel subsidies following the financial collapse of the national oil company, PEMEX and (ii) the differences among municipalities in their exposure to fuel theft, measured by the interaction between these price changes and the inverse distance from each municipality's centroid to the nearest fuel pipeline.

These instruments capture exogenous variation in the likelihood of territorial conflicts among criminal organisations, thereby affecting changes in local homicide rates. Large-scale fuel theft occurs directly from these pipelines carrying gasoline and diesel. That is why areas located close to this infrastructure have been heavily contested by criminal groups due to the high value of fuel theft revenues.

The exclusion restriction requires that distance to the nearest pipeline and changes in fuel prices affect homicides but do not directly influence migration networks or flows. This assumption is plausible for three reasons. First, pipelines are underground, were built mainly in the 1950s–1960s and serve only to transport gasoline and diesel across the country, so proximity does not generate local economic conditions relevant for migration decisions. Second, retail fuel prices have long been set by the federal government and vary with international markets and national subsidies, which have remained largely uniform nationwide. Only minor zone-level price adjustments have been permitted in a small number of remote or high-cost areas, affecting only two states. However, these adjustments are so minor that they are unlikely to generate meaningful variation in local economic conditions relevant for migration (Table 1). Third, although some stolen fuel is sold locally, most rents are captured by criminal organisations through regulated distribution channels rather than accruing to local populations (Gutiérrez-Romero, 2026).

To implement our instrumental variables strategy, we first estimate the relationship between the instruments and changes in homicide rates, relevant for our period of analysis. This first-stage specification is presented in Equation (6):

$$\Delta Homicides_{it} = \mu_{1i}Z_{it} + \mu_{2i}\Delta X_{it} + \mu_{3i}T_t + \mu_i + u_{it} \tag{6}$$

Our likely endogenous variable, the change in homicide rates, is measured as the difference between 2005 and 2010, and between 2010 and 2015. Variation in violence over these intervals is used to explain likely changes in our main outcome: the change in migration networks, derived from comparing censuses. The vector Z includes two instruments. The first is the five-year change in the price of premium gasoline, the fuel most closely tied to international market prices, measured as the difference between 2005 and 2010, and between 2010 and 2015. The second is the interaction between the change in the average price of fuel (including premium, Magna and diesel) and the inverse distance from each municipality's centroid to the nearest fuel pipeline. For this instrument, fuel price changes are measured between 2004 and 2009, and between 2009 and 2014.

We use slightly different periods for capturing changes in fuel prices for two reasons: (i) fuel price increases may have led to a rise in homicide rates with a lag, particularly in municipalities near pipelines, where rising prices increase the value of fuel theft; and (ii) repeated (and unannounced) fuel price increases may have strengthened incentives for large-scale fuel theft.

All fuel prices are adjusted to real terms using the national consumer price index from INEGI. Gasoline and diesel prices are sourced from the Comisión Federal de Competencia Económica (COFECE). The distances to gasoline and diesel pipelines are our estimates based on geospatial data from PEMEX.

In the first stage, we estimate whether proximity to pipelines is associated with larger increases in homicide rates, particularly during periods of rising fuel prices. As shown in the next subsection, our results suggest that the instruments are reasonably strong. The standard endogeneity tests reveal that the simple panel fixed specifications are downward biased as they do not capture the true impact of the rise in homicide rates, and we should therefore prefer the IV specification.

The second-stage IV panel fixed effects specification, denoted by Equation (7), estimates the impact of the instrumented change in the homicide rate on the z-score (which denotes the change in the migration network).

$$\Delta m_{it} = \kappa_1 \Delta \widehat{Homicides}_{it} + \kappa_2 \Delta X_{it} + \kappa_3 T_t + \kappa_i + e_{it} \tag{7}$$

where κ_1 is the regression coefficient of the instrumented change in the homicide rate. The municipality fixed effects and residuals are denoted by κ_i and e_{it}. Again, the standard errors are adjusted using a Conley-type distance-based clustering correction and heteroskedasticity- and autocorrelation-consistent standard errors with two lags, implemented using the acreg Stata package by Colella et al. (2023).

6.2. Impact of violence on migration networks

We begin by examining the impact of homicide rates on domestic and international migration networks. Our analysis of international migration focuses on the US, as it accounts for over 90% of emigration and 95% of return migration of Mexicans from abroad.

Table 2, columns 1–4, show the panel fixed effects estimates. The results indicate that rising homicide rates are associated with declines in both the network of domestic migration and emigration to the US, as measured by the respective z-scores. However, none of these associations is statistically significant and may reflect bias due to endogeneity.

To address concerns about endogeneity, we next focus on the instrumental variables (IV) panel fixed effects estimations. Table 3 presents the first-stage results corresponding to the second-stage IV regressions shown in columns 5–8 of Table 2. The instruments consistently explain variation in homicide rates. Both instruments – the five-year change in premium gasoline prices and the interaction between average fuel prices and inverse distance to the nearest fuel pipeline – are strong predictors of changes in homicide rates and are statistically significant across specifications.

The interaction term captures the idea that municipalities closer to pipelines are more exposed to fuel theft-driven violence, particularly when fuel prices increase. The higher this interaction (i.e., higher fuel prices and closer proximity to pipelines), the more homicide rates rise. The other instrument, the five-year change in gasoline prices, also reveals that rises in gasoline prices are linked to higher homicide rates.

Table 2. Change in homicide rate and z-standardised migration scores.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel fixed effects				Second-stage IV panel fixed effects			
	Change in domestic emigration z-scores	Change in domestic immigration z-scores	Change in international emigration (leaving for US) z-scores	Change in international immigration (Mexicans return from US) z-scores	Change in domestic emigration z-scores	Change in domestic immigration z-scores	Change in international emigration (leaving for US) z-scores	Change in international immigration (Mexicans return from US) z-scores
Change in the homicide rate	−0.146	−0.398	−0.065	−0.121	83.47**	49.91***	2.032**	3.229
	(0.642)	(0.708)	(0.053)	(0.125)	(38.28)	(18.41)	(0.807)	(2.853)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipalities fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4736	4736	4700	4736	4736	4736	4700	4736
Number of municipalities	2368	2368	2350	2368	2368	2368	2350	2368
F test of excluded instruments:					15.16	15.16	15.53	15.16
Prob > F					0.00	0.00	0.00	0.00
Stock-Yogo weak ID test critical values: 15% maximal IV size					11.59	11.59	11.59	11.59
Hansen J statistic (overidentification test of all instruments)					2.60	2.26	1.77	1.16
Chi-sq(1) *P*-val					0.11	0.13	0.18	0.28
Endogeneity test of endogenous regressors:					5.82	7.43	6.30	2.41
Chi-sq(1) *P*-val					0.02	0.01	0.01	0.12

Note: Other controls include changes in the poverty rate, annual nighttime light-per-capita, and the Gini coefficient. Conley-type distance-based clustered standard errors, robust to heteroskedasticity and serial correlation, are shown in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3. First-stage regression for Table 2 and Table 4.

Second-stage IV regression's dependent variable:	(1) Change in domestic emigration	(2) Change in domestic immigration	(3) Change in international emigration, leaving for US	(4) Change in international immigration, Mexicans returning from US
First-stage regression's dependent variable:	Change in the homicide rate	Change in the homicide rate	Change in the homicide rate	Change in the homicide rate
Change in the average price of diesel and gasoline divided by the municipality's average distance to the nearest oil pipeline	0.103**	0.103**	0.108**	0.103**
	(2.27)	(2.27)	(2.60)	(2.27)
Change in the price of gasoline in real pesos	370.5***	370.5***	372.7**	370.5***
	(5.33)	(5.33)	(2.27)	(5.33)
Change in the Gini index	−37.22	−37.22	−50.07	−37.22
	(−0.57)	(−0.57)	(−0.64)	(−0.57)
Change in the poverty rate	−42.52	−42.52	−51.93	−42.52
	(−1.30)	(−1.30)	(−1.46)	(−1.30)
Change in the nighttime light-per-capita	605.4***	605.4***	590.4**	605.4***
	(3.35)	(3.35)	(2.80)	(3.35)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Number of municipalities	4736	4736	4700	4736

Note: Conley-type distance-based clustered standard errors, robust to heteroskedasticity and serial correlation, are shown in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The first-stage F-statistics exceed the conventional threshold of 15, indicating instrument relevance (Table 2, columns 5–8). The Cragg-Donald Wald F-statistics and Hansen J overidentification tests, reported at the bottom of Table 2, suggest that the instruments are both relevant and valid. Endogeneity tests, also shown in Table 2, reject the null hypothesis of exogeneity for domestic emigration, domestic immigration and international emigration to the US, indicating that IV estimates should be preferred. In any case, both the fixed effects and IV estimates suggest that increases in homicide rates did not significantly affect the return migration network.

For return migration from the US, the endogeneity test yields a p-value of 0.12, providing no evidence of significant endogeneity in this case. The instruments remain relevant, as they explain substantial variation in homicide rates, drawing on fuel price changes that reflect national and international market dynamics, including those in the US driving international fuel prices. While unobserved shocks may bias estimates in other migration outcomes by simultaneously affecting violence and migration decisions, the data do not indicate a strong correlation between such unobserved shocks and return migration. As a result, although the IV strategy remains valid and is supported by relevant instruments, the panel fixed effects estimator is likely sufficient for identifying the effect of homicides on return migration in this specification.

The second-stage IV panel fixed effects estimation, Table 2, shows how rising homicide rates have reshaped migration networks, particularly within Mexico. A one-unit increase in homicide rates significantly increased both domestic emigration (83.47) and domestic immigration (49.91) networks, proxied by changes in their z-scores. There is a distinction, though: the domestic emigration network is becoming stronger than the immigration network, meaning that more people are leaving than arriving, and from a wider variety of areas. Importantly, the impact of rising homicide rates on the domestic emigration network is much stronger than on the international emigration network to the US, where the effect is positive but smaller (2.03), as seen in Table 2, column 7. These findings suggest that, while higher homicide rates are driving some people to leave Mexico altogether, reflected in a strengthened emigration network to the US, the overwhelming impact is felt within the country. The weak and statistically insignificant effect on the return migration network from the US (3.23) suggests that rising homicide rates are not impacting the strength of this network (Table 2, column 8).

As noted earlier, our network algorithm does not model any of the drivers that may affect network changes, including distance, a key component of gravity models used, for example, to study migration between Mexico and the US (Flores et al., 2013). This is suitable when mobility is not influenced by proximity, as seen in the spread of COVID-19 through flight networks. In such cases, distance-agnostic models can perform as well as or better than gravity approaches (Simini et al., 2012). As a robustness check, we incorporated distance into the NC algorithm and re-estimated the IV regressions. The results are unchanged (and not reported for brevity), indicating that the NC algorithm captures network topology equally well with or without distance.

6.3. Impact of violence on the number of migrants

All previous studies on the impact of violence on migration in Mexico have focused on changes in net flows, tracking the number of people moving into or out of areas. While informative, these net flows overlook the complexity of how violence reshapes the intricate domestic and international migration networks across all municipalities. This is why we chose the NC algorithm, which allows for a more detailed analysis of these bipartite networks. But what if, as in previous research, we had analysed only net migrant flows? Would we have arrived at the same mixed results as earlier research?

To evaluate the impact on migrant flows, we re-estimate our analysis using the change in the number of people migrating into and out of each municipality, both domestically and to the US, as the dependent variables. Each census records flows from the preceding five years, resulting in two periods of analysis: 2005–2010 and 2010–2015 for the first census comparison, and 2010–2015 and 2015–2020 for the second.

The main results are presented in Table 4. The panel fixed effects results, columns 1–4, suggest that both emigration and immigration are negatively associated with homicide rates, but these results are not statistically significant. For international migration, we see a statistically significant, albeit small, negative effect on emigration to the US The effect of homicide rates on return migration from the US is also negative, but not significant.

To address concerns about endogeneity between homicide rates and migration, Table 4 (columns 5–8) reports second-stage IV panel fixed-effects regressions. For consistency, we employ the same set of instruments as in the network analysis.

Table 3 reports the first-stage regressions for the IV specifications used to estimate the effects of homicide rates on changes in the number of migrants. These first-stage results are identical to those corresponding to the specifications using migration network z-scores as outcomes. This is because the endogenous regressor (change in homicide rates), instruments, controls, fixed effects and sample size are all the same across specifications.

The second-stage IV panel fixed effects estimator reveals that homicide rates significantly impact migration at the municipal level (Table 4). For domestic migration, a one-unit increase in homicide rates leads to a statistically significant increase in the number of domestic emigrants (39.58) and domestic immigrants (33.33). This suggests that rising violence does not deter people from arriving, potentially due to the economic opportunities that these municipalities may offer, albeit in net terms, more people are leaving. These IV findings indicate that during the period analysed from 2005 to 2020, the number of domestic emigrants and immigrants has increased, but emigration has increased further.

We now turn to migration to the US. A one-unit increase in homicide rates leads to a statistically significant decrease in return migration (−1.779) and a modest increase in emigration (0.793). Comparing these trends with the network analysis reveals that, while the structure of the return migration network has remained stable, fewer people are returning because of violence. Meanwhile, the rise in both emigration flows and network strength due to increasing homicide rates suggests that violence acts as a push factor driving migration to the US.

The direction of the impact of homicide rates on the number of people emigrating abroad aligns with the findings of Orozco-Aleman and Gonzalez-Lozano (2018) and Daniele et al. (2023). However, these results contrast with those of Basu and Pearlman (2017), who found that violence reduced the number of people migrating to the US, likely due to the rising costs of emigration. Differences in data, identification strategies and time periods may account for these contrasting results.

Another important policy question is ultimately how many migrants nationwide were displaced by violence and what proportion of total migration this represents. We address this by calculating the total change in the migrant population between 2005 and 2020, as captured by the censuses and the intercensal survey analysed. Although these sources omit within-municipality moves and multiple migrations per individual, they still provide insight into broader displacement.

We obtain the total migration flows nationwide by using the IV results from columns 5–8 in Table 4. We multiply the estimated coefficients by the average change in homicide rates between 2005 and 2020 (11.51)

Table 4. Change in the homicide rate and change in the number of migrants.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel fixed effects				Second-stage IV panel fixed effects			
	Change in number of domestic emigration	Change in number of domestic immigration	Change in number of international emigration (leaving for US)	Change in number of international immigration (Mexicans return from US)	Change in number of domestic emigration	Change in number of domestic immigration	Change in number of international emigration (leaving for US)	Change in number of international immigration (Mexicans return from US)
Change in the homicide rate	−0.229	−0.988	−0.054*	−0.057	39.58*	33.33**	0.793**	−1.779*
	(0.388)	(0.811)	(0.030)	(0.060)	(23.95)	(14.98)	(0.398)	(1.011)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipalities fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4774	4774	4738	4774	4736	4736	4700	4736
Number of municipalities	2394	2394	2376	2394	2368	2368	2350	2368
F test of excluded instruments:					15.16	15.16	15.53	15.16
Prob > F					0.00	0.00	0.00	0.00
Stock-Yogo weak ID test critical values: 15% maximal IV size					11.59	11.59	11.59	11.59
Hansen J statistic (overidentification test of all instruments)					2.12	0.54	1.95	0.49
Chi-sq(1) *P*-val					0.15	0.46	0.16	0.49
Endogeneity test of endogenous regressors:					5.45	10.25	4.24	4.20
Chi-sq(1) *P*-val					0.02	0.00	0.04	0.04

Note: Other controls include changes in the poverty rate, annual nighttime light-per-capita and the Gini coefficient. Conley-type distance-based clustered standard errors, robust to heteroskedasticity and serial correlation, are shown in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

and the number of municipalities in the country (2454). This calculation suggests that over the period analysed (2005–2020), the rise in homicide rates contributed to an increase of approximately 1,117,968 domestic emigrants and 941,434 domestic immigrants. Additionally, it led to 22,398 more emigrants to the US and a reduction of 50,249 Mexicans returning from the US to more violent Mexican municipalities.

Thus, between 2005 and 2020, rising homicide rates accounted for about 6% of *all* domestic emigration, 5% of *all* domestic immigration and a 3% decline in *all* the return migration of Mexican nationals from the US. They also accounted for 5% of emigration to the US in the benchmark years used in our comparisons (2005 vs. 2010 and 2010 vs. 2015), equivalent to about 1% of *all* US-bound emigration recorded in the census between 2005 and 2020.

7. Violence and regional connectivity

If rising violence causes population displacement, this should be reflected in persistent declines in traffic flows connecting violent municipalities to the national highway network. Such a decline in traffic would also indicate that violence acts as a spatial friction, affecting regional connectivity, consistent with the mechanisms discussed in new economic geography (Fujita et al., 1999; Krugman, 1991). As a robustness check, we examine long-term intermunicipal highway traffic changes, which may capture displacement better than short-term changes in local trips.

We compare annualised average daily counts of cars, buses and freight trucks passing through each municipality in 2005 and 2015, a period that captures the intensification of Mexico's drug war and avoids distortions from the COVID-19 pandemic. This period aligns with the census migration reference period, which asks respondents to report moves since 2015.

To measure each municipality's connectivity to the rest of the country, we compute the sum of daily vehicle trips in all directions (inbound and outbound), applying our modified NC algorithm to estimate a z-score for each municipality. This z-score reflects changes in relative integration with the broader highway network.

Table 5 shows that the average daily intermunicipal traffic more than doubled nationwide, from 72,162 trips in 2005 to 183,229 in 2015. Yet, the average z-score fell, indicating uneven growth and declining connectivity for many locations. While overall migration volumes increased, flows became concentrated on select routes, leaving others relatively isolated.

Figure 6 visualises this shift. The network's spatial pattern reveals growing concentration in central regions, especially around Mexico City, and declining connectivity in areas marked by cartel disputes and fuel theft. In states like Puebla and Michoacán, municipalities lost long-distance links due to rising violence and extortion along fuel corridors.

To analyse the role of changes in homicide rates between 2005 and 2015 on the changes detected in the network of vehicle trips, we next use ordinary least squares (OLS). Our dependent variable is the z-score of annualised daily flows. We control for changes in nighttime lights per capita (a proxy for local economic activity), Gini coefficients and poverty rates over the same period. Poverty rates for 2005 are derived from the 2000 census, as the mid-census lacked income data.

Table 6, columns 1–4, show OLS estimates that find no significant relationship between rising homicides and either the structure or volume of vehicle flows. However, OLS estimates are unlikely to identify the causal effect of violence on mobility because unobserved factors, such as weakened state capacity and changes in criminal competition, can substantially affect both homicide rates and mobility. We cannot address this

Table 5. Descriptive statistics of annualised daily vehicle analysis.

	Mean	Standard deviation	Observations
Annualised daily number of automobiles reaching another municipality in 2005	72,161.940	95,100.600	1228
Annualised daily number of automobiles reaching another municipality in 2015	183,228.800	217,881.600	1228
Z-score of change in highway network between year 2005 and 2015	−8029.425	25,595.45	1226
Change in homicide rate between the years 2005 and 2015	8.694	26.822	1203
Change in the Gini coefficient between the years 2005 and 2015	−0.033	0.039	1203
Change in poverty between the years 2005 and 2015	−0.214	0.093	1203
Change in nightlight per capita between the periods 2001–2005 and 2011–2015	−0.200	0.209	1203
Average number of drug trafficking organisations by municipality in 2005	0.254	0.661	1203
Average number of drug trafficking organisations by municipality in 2010	1.002	1.405	1203

Source: Authors' own calculations.

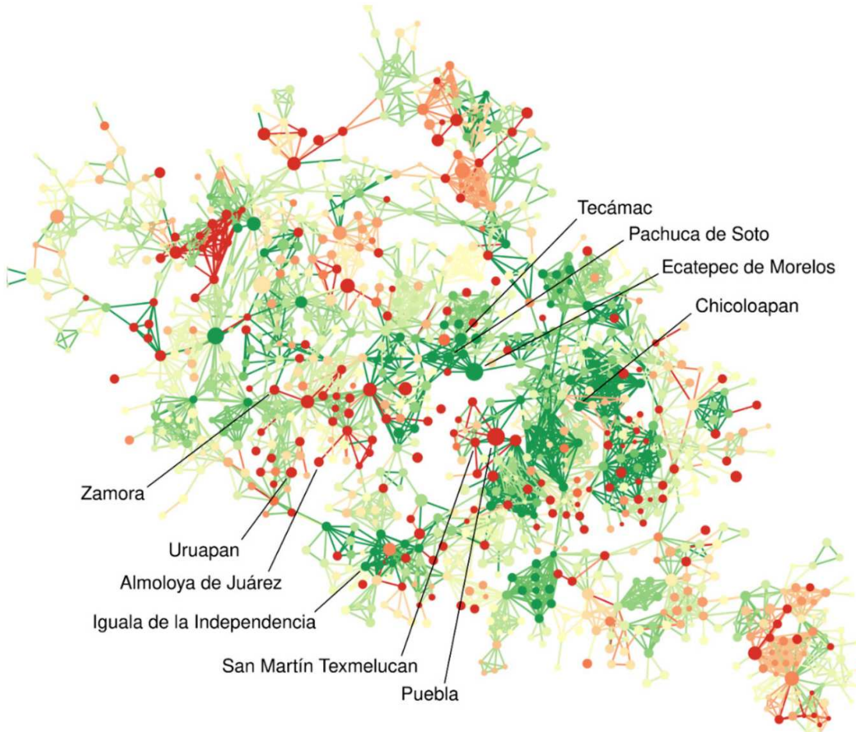

Figure 6. Change in the annualised daily vehicle trip network between 2005 and 2015.
Source: Authors' own calculations.
Note: Nodes represent municipalities, and links represent changes in annualised daily vehicles. Node colour indicates changes in vehicle flows (green: increase, yellow: no change, orange: moderate decline, red: strong decline). The layout is topological rather than georeferenced.

endogeneity concern using the same instruments employed earlier. Fuel prices and pipeline proximity, which are valid in the migration analysis, may directly affect how many driving trips people wish to do daily, violating the exclusion restriction in this setting.

We find two alternative instruments drawing on earlier literature showing that the surge in homicide rates during this period was driven by increased competition among drug trafficking organisations (DTOs) following federal crackdowns that fragmented established cartels and dispersed them geographically (Calderón et al., 2015; Castillo et al., 2020). Areas with increased DTOs experienced substantially higher violence. Our identification strategy exploits this variation. The first instrument is the change in the number of DTOs operating in a municipality between 2005 and 2010, based on data from Coscia and Rios (2012), who document DTO activity based on news reports. The second instrument uses the baseline number of DTOs present in 2005, prior to large-scale militarisation, to predict subsequent increases in violence.

These instruments satisfy the exclusion restriction because variation in DTO presence affects daily vehicle trips only through its impact on violence. Before fragmentation, DTO activity was not linked to widespread conflict nor significantly disrupted civilian life (Gutiérrez-Romero & Oviedo, 2018). Federal crackdowns increased competition among DTOs, leading to violent conflict over territory and sudden increases in homicide rates. It is this rise in violence and the associated increase in civilian risk that indirectly then affects how many daily driving trips one wishes to make, rather than DTO presence per se.

Table 6, columns 5–8, also the IV results. The first-stage regressions show that both instruments predict increases in homicide rates, with F-statistics above conventional thresholds. The Hansen and endogeneity tests support the use of IV.

The second-stage estimates show violence reduces intermunicipal traffic. A one-unit increase in the homicide rate reduced the z-score of highway connectivity by 678.8 points (column 2). Similar patterns emerge in column 3. Violence thus appears to deter long-distance travel to and from affected areas.

Table 6. Change in z-standardised scores for annualised vehicles crossing municipalities between 2005 and 2015.

	(1) Highway network z-score OLS	(2) Highway network z-score IV	(3) Highway network z-score IV	(4) Annualised daily vehicle traffic OLS	(5) Annualised daily vehicle traffic IV	(6) Annualised daily vehicle traffic IV
Change in the homicide rate between 2005 and 2015	28.17*	−678.8**	−668.2*	82.62	−10,588***	−9827***
	(15.38)	(328.7)	(345.2)	(158.6)	(4036)	(3591)
Constant	−4808	1670	1574	−76,269***	21,509	14,537
	(3146)	(4069)	(4202)	(17,080)	(38,675)	(34,139)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of municipalities	1203	1203	1203	1203	1203	1203
		First-stage regression Change in the homicide rate	First-stage regression Change in the homicide rate		First-stage regression Change in the homicide rate	First-stage regression Change in the homicide rate
Change in the number of drug trafficking organisations between 2005 and 2010		2.709***	2.480***		2.709***	2.480***
		(4.50)	(4.21)		(4.50)	(4.21)
Number of drug trafficking organisations in 2005			2.580**			2.580**
			(2.17)			(2.17)
Change in the Gini index		−45.26**	−42.07**		−45.26**	−42.07**
		(−2.81)	(−1.96)		(−2.81)	(−1.96)
Change in the poverty rate		14.22	14.25		14.22	14.25
		(1.41)	(1.09)		(1.41)	(1.09)
Change in nighttime light-per-capita		−7.043	−7.790**		−7.043	−7.790**
		(−1.46)	(−2.05)		(−1.46)	(−2.05)
Constant		6.821**	6.298*		6.821**	6.298*
		(2.74)	(1.89)		(2.74)	(1.89)
Number of municipalities		1203	1203		1203	1203
F test of excluded instruments:		20.24	11.33		20.24	11.33
Prob > F		0.00	0.00		0.00	0.00
Stock-Yogo weak ID test critical values: 15% maximal IV size		12.83	12.83		12.83	12.83
Hansen J statistic (overidentification test of all instruments)		exactly identified	0.00		exactly identified	0.22
Chi-sq(1) *P*-val			0.95			0.64
Endogeneity test of endogenous regressors:		4.82	7.17		24.25	33.12
Chi-sq(1) *P*-val		0.03	0.01		0.00	0.00

Note: Other controls include changes in the poverty rate, annual nighttime light-per-capita and the Gini coefficient. Conley-type distance-based clustered standard errors, robust to heteroskedasticity, in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We also observe large impacts on traffic volumes. A one-unit rise in the homicide rate led to 10,588 fewer daily vehicle trips entering a municipality (Table 6, column 5). With the average municipality experiencing a homicide increase of 8.69 points and 1203 municipalities connected to the highway network, this translates to roughly 110 million fewer daily intermunicipal trips (10,588 × 1203 × 8.69). This decline represents about 49% of all recorded traffic in 2015.

Some of these reductions may reflect shifts to buses or carpooling, but the magnitude suggests a deeper breakdown in regional integration. Taken together with our migration results, the evidence points to a broader disconnection of violence-affected areas from national flows of people and goods.

8. Conclusion

This paper examined how violence affects migration flows and migration networks across Mexican municipalities between 2005 and 2020. We identified the causal impact of homicide rates on migration using individual-level census data, the modified noise-corrected algorithm and an instrumental variables strategy.

The paper presented four main findings: First, rising homicide rates caused sustained net outflows from violent municipalities, pushing people into a broader range of destinations and reshaping Mexico's domestic migration network. Second, although the structure of return migration from the US remained stable, violent areas received fewer returning migrants. Third, violence significantly impacted internal displacement more than international emigration. Finally, the analysis of daily vehicle traffic on Mexico's national highway revealed that municipalities experiencing rising homicide rates lost millions of daily vehicle trips linking them to the rest of the country.

These findings point to violence as a spatial friction that reshapes both migration and development. In the tradition of migration models (e.g., Harris & Todaro, 1970), violence raises the effective costs of mobility and interaction, reinforcing mobility divergence between safer and more violent areas. The results also align with new economic geography mechanisms, whereby higher spatial frictions strengthen centripetal forces, concentrate activity in safer locations, and contribute to the peripheralisation and isolation of violent areas (Fujita et al., 1999; Krugman, 1991).

An extensive literature has shown that the violence emerging from competition among criminal organisations impacts education, labour markets, public finances and migration (Dell, 2015; Gutiérrez-Romero & Oviedo, 2018; Michaelsen & Salardi, 2020). This paper added a spatial perspective to that discussion. Even when aggregate migration flows appear stable, violence can substantially reconfigure migration networks and regional connectivity. These findings suggest that responses to criminal violence should consider not only migration flows but also their general-equilibrium spatial effects. Future research could formalise how violence-induced mobility costs affect these factors, including necessary policy responses.

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No potential conflict of interest was reported by the author(s).

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Data availability statement

All data supporting this study are publicly available and full details of the data sources are provided in the manuscript. The data are also available from the corresponding author upon reasonable request.

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